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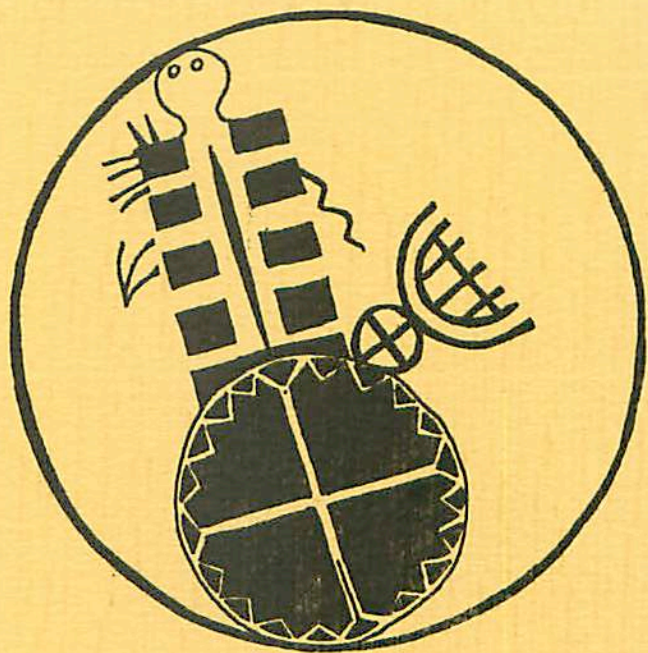
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# THE SAPIR-KROEBER CORRESPONDENCE

edited by  
Victor Golla

Report #6

Survey of California and Other Indian Languages



**THE SAPIR-KROEBER CORRESPONDENCE**



A. L. Kroeber, 1911  
(Courtesy of the Bancroft Library.)



Edward Sapir, about 1915  
(Courtesy of Philip Sapir.)

**THE SAPIR-KROEBER CORRESPONDENCE**

**Letters Between Edward Sapir**

**and A. L. Kroeber**

**1905 - 1925**

**Edited**

**With Notes and an Index**

**by**

**Victor Golla**

**Survey of California and Other Indian Languages**

**University of California, Berkeley**

**1984**

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Reports from the Survey  
of California and Other Indian Languages  
Edited by Katherine Turner, Wallace L. Chafe, and Leanne Hinton

Report #6

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cover design (Santa Barbara Chumash rock painting) by Leanne Hinton

## PREFACE

The voluminous correspondence between Edward Sapir and Alfred Kroeber spans 33 years--nearly the entirety of Sapir's extraordinarily productive career, beginning in his student days at Columbia in 1905 and ending only a few months before his death in 1939. As such things go, it is remarkably well preserved on both sides, especially in the years (1910-1925) during which Sapir was in the employ of the Canadian government. Before 1910 and after 1925 relatively few of Kroeber's letters to Sapir survive, since Sapir apparently was in the habit of destroying personal correspondence, so that all that are preserved are carbon copies kept by Kroeber. At Ottawa, however, letters sent to him at his official address became government property, whatever their content, and are still in the files of the Canadian Ethnology Service. Kroeber, for his part, seems to have been discreetly selective in what he preserved, keeping all letters dealing with intellectual topics, removing from his files only letters of a clearly personal nature, such as those concerned with Sapir's wife's tragic illness. From what remains I have censored absolutely nothing, leaving in even the most routine of acknowledgments, telegrams, postcards, and financial accounts. This has meant a bulky volume, but one without any hidden gaps. Breaking off in 1925 was largely an arbitrary, practical decision, although this is when the Ottawa file ends. A second, somewhat shorter, volume with the remaining letters is in preparation.

Many of the letters are handwritten. Both men wrote in a small, precise hand, and in a few cases--particularly in some of the letters Kroeber wrote with a pencil--it has been impossible for me to decipher certain words: these have been clearly indicated. In a few other cases an indecipherable word can be reconstructed from context, and here I give my conjecture in square brackets. In general, however, the letters are thoroughly legible.

I have been generous with my annotations, perhaps over-generous. I have attempted to identify all names--with the exception of figures such as Franz Boas or Sigmund Freud, known to every educated reader--and to provide brief biographical summaries where relevant. As a rule, a biographical summary

appears at the first occurrence of an individual's name, and for names occurring in later letters readers should consult the index for the location of the first occurrence.

The correspondence relating to the Committee on Phonetic Transcription of Indian Languages, being of a highly technical nature and in the form of a paragraphed report and commentaries, has been put in Appendix I. Appendix II contains the abstract, lecture notes, and summary of classification that Sapir prepared for his December, 1920 paper on North American linguistic groups. These were sent to Kroeber for his comments (Letters 319 and 324), perhaps along with the map Sapir had prepared for the occasion, but Kroeber returned them, and they were nowhere to be found in the correspondence files. Mr. Philip Sapir was fortunate to discover these notes among his father's papers, and has kindly given permission for their publication here.

Of the letters themselves, the following are preserved in the Archives of the Canadian Ethnology Service, National Museum of Man, Ottawa, and are published here with the permission of Annette McFadyan Clark, Chief Ethnologist: Letters 81, 86, 94, 102, 103, 104, 145, 153, 155, 166, 167, 168, 178, 180, 181, 184, 191, 199, 204, 212, 213, 214, 216, 219, 220, 234, 235, 243, 244, 245, 246, 247, 248, 249, 251, 252, 253, 254, 255, 256, 261, 265, 272, 273, 274, 275, 277, 278, 279, 280, 286, 293, 296, 297, 298, 301, 302, 308, 309, 310, 311, 313, 314, 315, 318, 323, 325, 326, 327, 328, 329, 330, 331, 351, 355, and 360. The remaining letters, and the materials in Appendix I, are preserved in the Archives of the Department and Museum of Anthropology and in the A. L. Kroeber Papers, Bancroft Library, University of California, Berkeley, and are published here with the permission of Dr. James D. Hart, Director of the Bancroft Library, and Dr. Karl Kroeber, Columbia University, literary executor of his father's papers.

The photograph of Sapir in the frontispiece and Illustration 3 were provided through the courtesy of Philip Sapir. The photograph of Kroeber in the frontispiece and Illustration 1 were provided through the courtesy of the Bancroft Library. Illustrations 2, 4, and 5 were provided through the courtesy of the Robert H. Lowie Museum of Anthropology.

This volume could not have been assembled without the encouragement and support of Leanne Hinton and Wallace Chafe, Department of Linguistics, University of California, Berkeley, the Co-Directors of the Survey of California and

Other Indian Languages. I am much beholden to Janice Gould and Arax Kizirian for their invaluable typing services, and above all to my wife, Katherine Turner, for her aid and support.

Berkeley, California  
August 1984

Victor Golla



## ABBREVIATIONS

AA	American Anthropologist
A.A.A.	American Anthropological Association
A.A.A.S.	American Association for the Advancement of Science
B.A.E.	Bureau of American Ethnology
BAE-AR	Annual Report, Bureau of American Ethnology
BAE-B	Bulletin, Bureau of American Ethnology
IJAL	International Journal of American Linguistics
UC-AR	University of California, Anthropological Records
UCPAAE	University of California Publications in American Archaeology and Ethnology
UCPL	University of California Publications in Linguistics

THE SAPIR-KROEBER CORRESPONDENCE

[ 1 ]

May 5, 1905

My dear Dr. Kroeber,

Prof. Boas wishes to have me go out this summer to Warm Spring Reservation, Oregon, to do linguistic work among the Wasco Indians.<sup>1</sup> He thinks it advisable for me, as preliminary, to study the myths and traditions of the various Indian tribes along the Upper Pacific Coast and the Columbia River. Since he has not himself any separates of your Piyute Traditions that appeared in the "Folk-Lore Journal,"<sup>2</sup> he advised me to write you for a copy of the same. Would it be too much to ask you to send me the traditions, if possible, in duplicate? Hoping that I do not seem to be intruding on your good will, I am

Yours very respectfully,

Edward Sapir.

9 St. Nicholas Ave.

New York, N.Y.

---

1 Sapir received his Bachelor's degree from Columbia in 1904 and in the following academic year enrolled as a graduate student, intending to work in Germanic and Semitic philology. He began work on a Master's thesis on Herder (Sapir 1907c). It was during 1904-05, however, that he first met Franz Boas, and was "impressed with the possibilities of inductive study of living languages by phonetic recording from native speakers" (Swadesh 1939: 132). Boas recognized the young man's potential and quickly arranged for Sapir to do fieldwork in the summer of 1905 with speakers of Upper Chinook. Although the original plan seems to have been for Sapir to work among the Wasco on the Warm Springs Reservation, in Oregon, he in fact spent July and August with the Wishram, on the Yakima Reservation in southern Washington (Sapir 1907a).

2 Kroeber 1901. Kroeber had visited several Shoshonean groups during the summer of 1900.

[2]

New York, N.Y.

May 29, 1905

My dear Mr. Kroeber,

I have received your "Ute Tales" and wish to thank you for your courtesies in acceding to my request. I desire also to thank you for the invitation extended to me to call at the Museum, though I consider it more than doubtful if I shall have any opportunity to avail myself of it, at least this summer. The field of my operations will hardly extend to San Francisco, will probably be confined to Washington and Oregon. In case chance does bring me to San Francisco, however, I shall be delighted to have the privilege of calling. Meanwhile, I remain

Yours very respectfully,

Edward Sapir.

[3]

[Affiliated Colleges,  
San Francisco, Calif.]  
Jan. 23, 1907

My dear Mr. Sapir:

I should like to propose to you that you spend the twelve months from July 1, 1907 to July 1, 1908 at our Department of the University of California in research on the languages of California.<sup>1</sup> My plan is to have you visit the survivors of one of the nearly extinct stocks, probably the Yana, and after as much time spent in the field among them as necessary, to settle down at the University to work up your material. If there were time within the twelve months for you to take up a second group of people in the same way; so much the better. We should be glad to pay you a salary of \$50.00 a month through the year, to provide funds for your field work, and to make an allowance covering your transportation to and from California.

I trust you will be able to see your way clear to accepting this proposition, which we are able to make on account of the expected leave of absence of one of our men during 1907-08 for the purpose of spending a year in the East for study.<sup>2</sup> The financial arrangement by which our Department is at present supported expires July 1, 1908, and while we are hopeful of a continuance on the present status, this is not yet assured.<sup>3</sup> The engagement would be definitely limited to one year and would therefore not interfere with plans for the future which you may have. I mentioned the matter to Dr. Boas briefly before leaving New York<sup>4</sup> and am writing him again. I am also making an analogous proposition, though of a somewhat different nature, to Mr. Speck.<sup>5</sup> Kindly advise me as soon as possible of your decision, as the recommendations for our budget for 1907-08 should be made by March 1. If your decision is favorable I will submit the plan for ratification to Professor Putnam<sup>6</sup> and President Wheeler,<sup>7</sup> who I have no doubt will approve it.

Sincerely yours,

[A. L. Kroeber]

- 
- 1 Kroeber hoped to be able to support two Research Associates during 1907-08 with money originally budgeted for his Museum Assistant, S. A. Barrett. Barrett was to spend the year at Columbia with Boas, supported by a scholarship. Correspondence between Kroeber and Boas over this arrangement makes it clear that it was understood as a reciprocal exchange: Boas "took on" Barrett in return for Kroeber "taking on" two of his graduate students. (Boas to Kroeber, 1/23/07; Kroeber to Boas, 1/28/07).
  - 2 Samuel A. Barrett (1879-1965), Kroeber's first postgraduate student. After taking a B.S. at Berkeley in 1905, Barrett worked for the Museum of Anthropology as a curatorial assistant and field collector, while preparing his dissertation on Pomo basketry. A methodical man, devoted to the study of American Indian material culture, he seemed destined for a career with the Museum of Anthropology, but at the end of his year of study with Boas the financial situation at Berkeley did not allow his assistantship to be renewed and Kroeber urged him to seek a position in a more secure institution. After a year in South America on a research fellowship, Barrett obtained a curatorial position at the Milwaukee Public Museum, where he worked until his retirement in 1959. For a survey of Barrett's career see Peri and Wharton 1965.
  - 3 The Department and Museum of Anthropology of the University of California were established in 1901 with generous support from Mrs. Phoebe Apperson Hearst, and the promise of continuing support for five years. In 1906 the Hearst subvention was renewed for a final two-year period. After July, 1908 the University assumed full responsibility for supporting the program,



and research was drastically curtailed. See Kroeber's own discussion of this period in Sapir and Swadesh (1960:v), and Thoreson's detailed history (1975).

- 4 Kroeber had been in New York at Christmastime, 1906.
- 5 Frank G. Speck (1881-1950), a student of Boas', almost exactly contemporary with Sapir, and also interested in linguistics. Speck did not accept Kroeber's offer for 1907-08, and went instead to Oklahoma on a University of Pennsylvania fellowship, and he remained associated with Pennsylvania for the rest of his life. Speck's research career was devoted almost entirely to Algonquian ethnology and linguistics. See Hallowell 1951.
- 6 Frederic Ward Putnam (1839-1915), Curator of the Peabody Museum of American Archaeology and Ethnology at Harvard. Putnam was also Professor of Anthropology and Director of the Museum of Anthropology at California from 1903 to 1909. The appointment was more than titular--he visited California on numerous occasions and played a major role in securing Mrs. Hearst's support--but he did no teaching or research in California and the day-to-day supervision of the program was left to Kroeber. See Mark (1980:14-61) and Thoreson 1975.
- 7 Benjamin Ide Wheeler, President of the University of California.

[4]

Brooklyn, N.Y.

Feb. 9, 1907

My dear Dr. Kroeber,

I was greatly flattered at your rather unexpected proposal to have me spend a year at your University in research work on Californian languages. After consultation with Dr. Boas on the subject, I have decided to accept it on terms you state in your letter: i.e., if I understand you rightly, to have all field expenses, including board and lodging while out in the field, provided for, over and above the salary of \$50.00 per month throughout the year. There is a point on which I should like to be clear. It would hardly be expedient to gather more material than could be properly worked up during the year, since, with work still to be done on Chinookan and Takelma<sup>1</sup> I would soon find myself uncomfortably swamped. I therefore suggest that I spend as much time as necessary, say three months (perhaps less, perhaps more) to obtain an adequate idea of the Yana language, then that I dispose of the

material gathered at the University, and if time should still be left over that I then try to tackle another tribe. You will readily appreciate my desire not to have too much dead weight on my hands. Thanking you most heartily for your offer, I remain,

Yours sincerely,  
Edward Sapir.

- 
- 1 The Chinookan material was obtained during the summer of 1905 (Letter 1) and Takelma during the summer of 1906. Sapir was preparing a volume of Upper Chinookan texts for publication (Sapir 1909a) and had begun work on a grammar of Takelma (his doctoral dissertation, eventually published as Sapir 1922a) and a volume of Takelma texts (Sapir 1909b).

[5]

[Affiliated Colleges,  
San Francisco, Calif.]  
February 18, 1907

Dear Mr. Sapir:

I am glad you see your way clear to accepting my proposition, which I will state once more so as to remove any possible doubts. We ask you to give us the twelve months from July 1, 1907 to July 1, 1908, for the purpose of investigating one or if possible more of the languages of California and to some extent perhaps the ethnology that you would thereby come into contact with. We shall wish your entire time for the twelve months to be given over to the prosecution of this end, namely, in field work, working over your material, and its preparation for publication. We shall pay you a salary of fifty dollars per month for the twelve months, besides allowing you all expenses of transportation. The expenses of your field work would of course be exclusive of this, and your living expenses while in the field, namely, board and lodging, would not come from your salary, but from the fund for field work.

I trust this will render the proposition entirely clear, and shall regard your letter of February 9 as a definite acceptance. Professor Putnam has

heartily approved of the plan and I have therefore no hesitation in assuring you that it will certainly be passed by the Regents of the University this spring.

As to the order of field work, I agree with you that it will be best to do one thing at a time. Such was my plan when I wrote to you. In case you begin with the Yana, which seems to me the most advisable, I should not favor your taking the field to investigate any other language until your Yana results were clearly worked up for publication.

Trusting that I have now cleared up all points, and anticipating your presence with us in the course of a few months, I remain with best wishes,

Sincerely yours,

[A. L. Kroeber]

[6]

[Telegram]

Brooklyn, N.Y. Mar. 2 [1907]

Prof. A. L. Kroeber

2848 Washington St., San Francisco

Cannot definitely accept offer wait until receipt of letter.

S. A. Pier

[7]

Brooklyn, N.Y., March 2, '07

My dear Dr. Kroeber,

You were probably rather surprised to receive my telegram of the 2nd inst., and I hasten to explain. Only a few days after I had written you my letter of acceptance I received from Dr. G. B. Gordon<sup>1</sup> of the University of Pennsylvania an altogether unexpected letter wherein he offered (or practically offered) me the incumbency of the Harrison Research Fellowship. That you may understand the nature of his offer, I quote the essentials:

"A Harrison Research Fellowship will be assigned to the Department of Archaeology for the academic year 1907-08. I understand that you will receive your doctor's degree the coming spring and you would therefore be in a position to apply for this fellowship in case you choose to do so. I send you herewith an application blank which must be in the hands of the Dean before March 1st in order to receive recognition.....'The candidate must hold a Ph.D. degree granted under conditions satisfactory to the Executive Committee of the Graduate School, and must submit plans for the prosecution of a line of research acceptable to that Committee.....He may, further, give instruction in the Graduate School or in the College of this University, provided such instruction does not exceed four hours a week during any one year.'.....The Fellowship will in all possibility lead to a Professorship in Anthropology; and it will also give good opportunity for research during its incumbency. The holder of a Fellowship receives a stipend of \$800 per annum."

Yesterday evening (March 1"), through the kindness of Dr. Boas, I was enabled to see Dr. Gordon in New York and to talk the matter over with him and Dr. Boas in greater detail. It appears that if I should be awarded the Fellowship (and there seems to be no reasonable doubt that I would be given the award, provided I obtain the degree by October), I would have the opportunity of building up in the University of Pennsylvania an elementary course in ethnography and one in American languages to start out with. As Dr. Gordon intends, in the course of a few years, to leave University work and devote himself entirely to his Museum duties, there is here offered me a chance, as you will readily perceive, to establish myself permanently in Anthropology. Certainly such an opportunity is not apt to be forthcoming any too often and ought therefore not to be lightly neglected. I have therefore ventured to apply for the Fellowship. According to Dr. Gordon the award will be made within two weeks from the date of writing, so that you will not be kept long in uncertainty as to the result of my application. (I shall of course take the earliest opportunity to inform you of the result). If my application is definitely rejected, my acceptance of your offer will naturally stand. If, however, the Committee should grant me the Fellowship, I should like to ask you as a great favor to absolve me from the obligations of my acceptance of your own offer. One circumstance should be added. Prof. Boas has



communicated by telephone to-day with Prof. Putnam and explained to him the relation in which I stand to Dr. Gordon. In view of the decided prospect for a permanent position at Philadelphia, Prof. Putnam thought that Dr. Gordon's offer was more advantageous and agreed, for his part, not to make my acceptance of the California position binding upon me. I understand that Dr. Boas will write to you in regard to this matter.

In conclusion I should like to propose to you an amendment of your own offer. The Harrison Fellowship will naturally take effect only from the fall of this year, so that the summer will be still at my own disposition. Would you consent to my doing field work in California for your University on the same (proportionate) financial basis as outlined in your first letter, the field work to start as soon as my dissertation is written out (about July 1st, I expect)? I have come to an understanding with Dr. Gordon (and probably he will be able to obtain the consent of the proper authorities therefor) as to the disposition of such Californian material. I have enough Takelma material on hand to satisfy the University of Pennsylvania in regard to the publication of anthropologic matter, so that he sees no reason why the Californian material could not be worked up for publication at the University of California during the academic year 1907-8. As far as I can see, as a net result you would have the work that you desire me to do done anyway, with the difference that the actual writing of the results will take place away from California and that the expense to the University of California will be decidedly less. I trust that you will grant me the leeway I ask for and will find it possible to accept my amendment. Hoping you will pardon my rather wearisome letter of explanation, I remain

Yours very sincerely,

Edward Sapir.

P.S. Could I trouble you for copies of your papers on N. Californian basketry and on Languages S. of San Francisco.<sup>2</sup>

---

1 George Byron Gordon (1870-1927). Gordon was a Harvard-trained archaeologist with research interests in Central America (and later the Near East). At this time he held the supervisory curatorial position in American archaeology in the University of Pennsylvania Museum and was attempting to establish a teaching department. Gordon proved to be a poor academic

administrator, and after 1910 he gradually withdrew from the academic program to devote himself to research and the Directorship of the Museum. See Darnell 1970.

2 Kroeber 1905 and 1904.

[8]

[Affiliated Colleges,  
San Francisco, Calif.]  
March 7, 1907

My dear Mr. Sapir:

Though I regret it exceedingly on my own account, I am very glad to hear that you have had a practically definite offer which will lead to a permanent position in Philadelphia, and I congratulate you on the excellent prospects you have thus achieved. We should certainly not think of letting your acceptance of our temporary plan stand in the way of your future.

As the Philadelphia matter has not yet been positively arranged, we will hold your projected position with us open for as long as possible. On receipt of final advices from the University of Pennsylvania I would ask you to telegraph me at our expense, stating whether or not you have received the appointment. If by any chance you should not have received it, I will regard your message to this effect as an acceptance of our original offer.

Regarding your suggestion to spend this summer with us in field work, preparing during the winter of 1907-08 the material thus obtained, I hope to write you shortly.

With best wishes for success if your most recent plans materialize, I remain

Sincerely yours,  
[A. L. Kroeber]

[9]

[Affiliated Colleges  
San Francisco, Calif.]  
March 8, 1907

My dear Mr. Sapir:

Following the suggestion in your last, I would like to inquire whether in the event of your receiving the Harrison fellowship at the University of Pennsylvania you would accept an offer from us to spend from two to three months this summer in the field among the Yana, with the understanding that the working over of the material thus obtained was to be given first call upon your time available for research and study during the academic year 1907-08; we to pay your expenses in the field, including transportation to and from California, but no compensation to yourself. I should have been glad to arrange something different for you in the last respect, as shown by our recent offer to you; but we cannot depart from the principle of not paying salary to anyone who is under salary from another institution for the same period, unless it were by contract for the performance of specified work, and this is of course out of the question in your case.

If you would accept an offer upon these terms, kindly advise me as promptly as possible, preferably by adding to the wire I have previously requested from you upon the result of the award of the fellowship. In case of your signification of acceptance, we shall then try to make provision accordingly and advise you as early as may be. I trust it will be possible for you to learn the result of the fellowship award within the period of two weeks you mention in your letter. I should like to hold our original offer open to you, and can do so for that time, but not indefinitely, as it will soon become necessary for us to conclude our arrangements for next year.

Sincerely yours,

[A. L. Kroeber]

[10]

[Telegram]

New York Mar 20-07

Dr. A. L. Kroeber

Affiliated Colleges, University of Calif., S.F.

Will stick to original acceptance have given up philadelphia.

Sapir

[11]

Brooklyn, N.Y., March 21, '07

My dear Dr. Kroeber,

You will probably have received my telegram of the 20th inst. adhering to the acceptance of your offer. In view of this the Philadelphia affair presumably strikes you as a Much Ado about Nothing, still I wish to thank you most heartily for your ready acquiescence to my request for leave to apply for the Harrison Fellowship. It seems that Dr. Gordon was too certain about the assignment of the Harrison Fellowship to his Department; at any rate, as I understand the matter, it is to be granted this year to some other Department. I have, in lieu thereof, been offered a regular Fellowship in course at the University of Pennsylvania, but this I have preferred not to bother about, preferring rather to stay with California.<sup>1</sup> I should like also to thank you for the separates you were so kind to send me. I have been enjoying the reading of the paper on "Basket Designs" and, although it is a very small point, you may be not uninterested to know that the N. Wintun term for sifting basket (tekes)<sup>2</sup> is evidently identical with the Takelma word for the same thing: degàs (d and g are weak surds, acoustically between surd and sonant), degēs-f<sup>ē</sup>t 'your sifting basket-pan.' It may turn out that a fair number of cultural words are common to Californian languages and Takelma.

Hoping you will pardon the unnecessary trouble I have put you to in regard to the Harrison Fellowship, I remain

Yours very sincerely,

Edward Sapir.

- 
- 1 Events apparently took yet another turn at Pennsylvania, for Frank Speck was awarded a Harrison Research Fellowship for 1907-08 (Hallowell 1951:69; Darnell 1970:88). Sapir was offered (and accepted) a Harrison Fellowship in 1908-09.
  - 2 Kroeber (1905:142). The term is actually /teqes/, and refers to a pan-shaped sifter basket or parching tray. It has been noted only in the McCloud River dialect (Schlichter 1981).

[12]

Brooklyn, N.Y.

April 20, 1907

My dear Dr. Kroeber,

I wish to thank you for your definitive information of the 11th inst. in regard to my appointment as Research Assistant in the University of California.<sup>1</sup> Following are the statistical data you require: A.B.--Columbia College, 1904. A.M.--Columbia University, 1905. Candidate for Ph.D.--Columbia University, 1905-1907. (Scholar in Linguistics, 1905-1906. Fellow in Linguistics & Anthropology, 1906-'07.) As to when I expect to get my Ph.D. degree, that depends somewhat upon circumstances. My required residence is already finished and I am now engaged in writing my dissertation, which I hope to finish by July 1, about which time I expect to leave for California. The Ph.D. degree cannot, of course, be awarded before the dissertation is approved and the final examination is passed, and as my dissertation will not be handed in until after the formal close of the academic year, I must wait for the final examination and conferment of the degree until I return from the West, although the dissertation itself may have been published long before that.

I shall certainly follow your suggestion to write to Dr. Dixon,<sup>2</sup> in case I do not have the opportunity to see him personally, for information on the

location of the Yana. I shall also make it a point to look over Curtin's<sup>3</sup> material again to get an idea of what I may expect to get in mythology; Powers,<sup>4</sup> I find, has as good as nothing to say about the Yana. Looking forward to seeing you in July, I remain

Sincerely yours,  
Edward Sapir.

- 
- 1 No copy of this letter has been preserved.
  - 2 Roland B. Dixon (1875-1934), anthropologist at Harvard. Dixon had carried out ethnographic and linguistic fieldwork in northern California during several summers, beginning in 1899. His brief work with the Yana in 1900 yielded enough linguistic data to make it clear that the language differed greatly from the others nearby (Dixon and Kroeber 1903:22). Dixon took up archaeology and ethnology after his graduation from Harvard in 1897. In 1898 he joined Boas' Jesup North Pacific Expedition to work in British Columbia and Alaska, and in 1899 he began work in California. He received his Ph.D. from Harvard in 1900 with a dissertation on the Maidu language (later published as Dixon 1911). He remained associated with Harvard until his death, becoming Curator of Ethnology in the Peabody Museum in 1912, and Professor of Anthropology in 1915. Dixon's field work focused on California until about 1907, concentrating (by agreement with Kroeber) on the northeastern part of the state. He and Kroeber collaborated on an early typological classification of California languages (Dixon and Kroeber 1903) and Kroeber included him as co-author in his later classificatory statements (Dixon and Kroeber 1913a, 1913b, 1919). In his later career Dixon was chiefly concerned with problems of large-scale geographic distribution and other general topics.
  - 3 Jeremiah Curtin (1838-1906), linguist and translator, and for a time (1883-1890) a field worker for the Bureau of American Ethnology, chiefly in Oklahoma, California, and Oregon. His interest in traditional literature led him to collect numerous mythological texts in the course of his linguistic work, and in 1898 he published a volume of North American creation myths, including 13 texts (in English) that he had collected from the Yana.
  - 4 Powers 1876, the only general survey of California Indian groups then available.

[13]

New York, N.Y.

May 1, 1907

My dear Dr. Kroeber,

If it is not asking too much I should be greatly obliged to you if you could let me have copies of your recently published works: "Shoshonean Dialects of California" and "The Yokuts Language of South Central California."<sup>1</sup>

Yours very sincerely,

Edward Sapir.

---

1 Kroeber 1907a and 1907b.

[14]

Brooklyn, N.Y., June 11, 1907

My dear Dr. Kroeber,

My dissertation on the Takelma language has so grown under my hands (say 175-200 pp. of print) that this, together with translation that still remains to be done on the Wasco (Upper Chinook) texts advertised by the Ethnological Society,<sup>1</sup> will take me until about July 1st to clean up. Surely it would be a nuisance to have to carry over some of this work into the California year. I shall be greatly indebted to you therefore if you can find it possible to let me have a furlough of a few days, so that I can start for the West on July 1st. Trusting this arrangement will cause you no difficulty, I remain

Yours sincerely,

Edward Sapir.

P.S. I have recently had the opportunity to visit Dr. Dixon in Harvard and obtain from him information on the whereabouts of the Yana. Are there any regulations, maximum limit of expense, or anything of that kind to be observed in regard to expenses of transportation from N.Y. to San Francisco?

I am taking this opportunity to thank you for your studies on the Shoshoni and Yokuts languages.

- 
- 1 A collection of Upper Chinook texts (Sapir's Wishram texts, and a number of Wasco texts collected earlier by Curtin and edited by Sapir) appeared as the second volume of a new series, Publications of the American Ethnological Society, edited by Boas (Sapir 1909a).

[15]

[Draft]

6/24/07

Dear Mr. Sapir,

On my return from the field I found yours of the 11th. We shall be glad to have you take the desired delay, but expect you to start on July 1, as you propose. As your engagement here is limited and for specified work, I do not feel that a longer postponement would be desirable.

The allowance for travelling expenses will cover all ordinary necessary first class expenses, including fare, berths, meals, porter's services, baggage transfers.

Trusting that your Wasco and Takelma publication will be completed to your satisfaction, and with best regards, I remain,

Sincerely,

[A. L. Kroeber]

[16]

Brooklyn, N.Y.

June 30, 1907

My dear Prof. Kroeber,

I have just received your note of the 25th inst. and wish to thank you for granting me the furlough asked for. At date of writing all arrangements, including berth to Chicago, have been made for leaving New York on the morning



of July 2nd, so that it is no longer possible to set out on July 1st, as originally intended; still, the difference is not great, and I trust you will pass it over. The route that I have decided on is by way of the Lehigh Valley, Burlington, & Denver & Rio Grande. Hoping to see you in a few days, I remain

Yours very sincerely,

Edward Sapir.

[17]

Redding, Cal.

July 12, 1907

Dear Dr. Kroeber,

It seems that there is practically nothing to be done here in Redding.<sup>1</sup> A couple of residents who seem to know have advised me to proceed up the hills to Montgomery Creek, where there is more or less permanently stationed a group of Indians who will probably turn out to be Yanas. It does not appear likely that any Indians have their residence in Redding. My next move is to take the stage route this evening to Montgomery Creek.

Yours sincerely,

Edward Sapir.

---

<sup>1</sup> Sapir probably arrived in San Francisco about July 6. Within a week he was in the field. The small city of Redding, at the head of the Sacramento Valley, was the place closest to Yana territory reachable by railroad.

[18]

Montgomery Creek, Cal.

July 15, 1907

Dear Dr. Kroeber,

I greatly regret to say that the sum total of my accomplishments up to date is nil. The conditions, though at first glance not unfavorable, are in

reality peculiarly adverse. The Indians available for my purpose are strangely independent, largely because of the great scarcity of farm hands hereabouts. Some of them refused to do work for me because they were busy with their own farm work, others have been engaged for a week or so by white men and are at present unavailable. Over and above this they are an unusually suspicious set of men and I am told by some of the whites that this is due to the fact that they have been imposed upon of late years by various whites. All I have been able to do at present is to hire one of the women from (coming) Thursday on for 3 or 4 days out of the week, as she is busy elsewhere the rest of the time. I hope that some of the others will come around in due time. The total number of Yana speaking Indians is probably large enough for all linguistic purposes. Kelsey's list<sup>1</sup> is faulty in that it contains duplications and a large number of Pit Rivers. I am sorry to have been forced to waste so great an amount of time in mere canvassing and waiting, but I am sure that little else could have been done.

Yours sincerely,

Edward Sapir.

---

1 C. E. Kelsey, a San Jose attorney, was appointed a special Indian Agent in 1905-06 to carry out a census of California Indians living outside reservation lands. The results, forwarded to Washington, were not officially published, but a transcript copy was given to Kroeber in 1906 for use in his research. This transcript, edited by Robert F. Heizer, was published as Kelsey 1971.

[19]

Montgomery Creek, Cal.

July 19, 1907

Dear Dr. Kroeber,

Yesterday and today I have been at work on Yana with Betty Brown, a fairly intelligent informant, though not considered one of the best available. While much, in fact practically everything, is still obscure, a few points have come out with sufficient clearness. There is no doubt of the difference

between men's and women's speech, but the matter is apparently complicated in certain cases by a difference in form according to the sex of the person spoken to as well.<sup>1</sup> The number of verbal suffixes, some of them of very specialized meaning, found in Yana must be immense, as at least 25 such derivative elements have already shown up. I am hoping to get text material as soon as possible; without it a great deal, particularly the tense, modal (if any), and pronominal forms, will probably remain obscure.

Yours sincerely,  
Edward Sapir.

---

1 A difference between men's and women's speech in Yana had been noted by Dixon (Dixon and Kroeber 1903:15). It is likely that Kroeber specifically instructed Sapir to investigate the matter further.

[20]

Montgomery Creek, Cal.  
July 23, 1907

Dear Dr. Kroeber,

I have now had 4 day's work with Betty Brown; as she washes for white people 3 days out of the week, 4 days is all she can put at my disposal--in other words I am forced to spend 3 more days (Monday to Wednesday) in idleness. Perhaps I shall have made arrangements for continuous work throughout the week by next Monday; 4 days is all I can safely rely on now. I have obtained 2 texts from Betty, one supposedly on Marriage, the other a myth on the well-known theme of the Visit of two Girls to Panther and Coyote's Imposition. The texts are not, as far as I can tell, any too good, the probability being that she is distinctly inferior in that regard to some of the other Yanas.

I was very likely mistaken in regard to the difference of word forms as addressed to male or female, though of the difference as to male or female speaking there can be no doubt. The female form is always the shorter, being characterized either by the lack of a suffixed element found in the male form

or by a reduction to a whispered vowel of the final vowel of the male form. Thus -na, -a, -ma characterize the male forms of the following words but are lacking in the corresponding female forms:

<u>Male</u>	<u>Female</u>
t'ú <u>ina</u> 'sun'	t'úi
lál <u>la</u> (from lal <u>na</u> ) 'foot'	lal'
haláik' <u>ia</u> 'to-morrow'	haláik'i
apdjísiwā <sup>ε</sup> <u>m</u> 'I kill you'	apdjísiwā <sup>ε</sup>
mildjasimi <sup>ε</sup> <u>mā</u> 'you run'	mildjasimi <sup>ε</sup>
mildjasimi <sup>ε</sup> man <u>ā</u> 'did you run?'	mildjasimi <sup>ε</sup> man

In all these cases it would seem most likely that the underlined elements are distinct suffixes and are not merely mechanically sloughed off in the female forms. This is certain for -na, as evidenced by forms like áu'nidjā 'my fire' (-nidja 'my') from áuna, and by Curtin's jewínna 'chicken-hawk' but jewínpa (really -p'a) 'young chicken-hawk', where -na and -pa are evidently parallel and mutually exclusive suffixes. Examples of mechanical reduction are:

<u>Male</u>	<u>Female</u>
-madù 'place of...'	-mat' <sup>u</sup>
wáwi 'house'	wáw <sup>i</sup>
	( <u>w</u> = whispered or voiceless w)
ts'oréw <u>a</u> 'elk'	ts'oréw <sup>a</sup>
maríni <sup>ε</sup> <u>i</u> 'woman'	maríni <sup>ε</sup> <u>i</u>
míts'! <u>i</u> 'coyote'	míts'! <sup>i</sup>
-djūs' <u>i</u> Agentive	-djūs'! <sup>i</sup>

Of course the whispered vowel is often difficult to hear and may easily be entirely missed.

Another sex phenomenon is the use of different verb stems in some cases according to the sex of the person referred to, e.g. nisasi 'he goes away' but asasi 'she goes away' (cf. Dixon's nī tu sa sindja and ā'tu sa sindj; the change of nī- to ā- is here not really in the same line with -ndja : -ndj--- a woman, in speaking of a man, would say nisās 'he goes away', but asās 'she goes away'); burīsì 'he dances' but djarīsì 'she dances'.

The number of verbal suffixes (chiefly of local and modal significance) continues to grow ad libitum, about 70 being now on file; the exact

determination of their significance is often difficult. A good example of verb stem with suffixes is ba-tcā-t-k'i-wadji-si-mdjà 'go-fast at-night go-somewhere hither causative tense-sign I-him' = 'I told him to come back at night'. The terribly special character of some of these suffixes (e.g. N., S., E., and W. are denoted by certain suffixes: nīhausì 'he goes E.') leads me to expect an unlimited number to turn up.

Yours sincerely,  
Edward Sapir.

[21]

Montgomery Creek, Cal.  
July 30, 1907

Dear Dr. Kroeber,

In reply to your letter of July 25,<sup>1</sup> I should like to say that in my opinion there will probably be little difficulty in getting the postmaster of Montgomery Creek to cash money orders for \$50 or even larger amounts. I am at present on another 3 days' furlough, all my further attempts to make arrangements with any Indian other than Betty Brown for a full week's work continuing fruitless. While Betty Brown is fairly good up to a certain point in the getting of grammatical forms, she is distinctly poor when it comes to finer points, such as discrimination in the exact significance of forms, and apparently unable to give texts of intelligently consecutive narrative. She has at times also an uncomfortable way of unconsciously contradicting some former statement or implication. For these reasons I am very anxious to change informants, at least for a time. One man, who seems both more intelligent and more reliable, also better able to speak English, than Betty, I have ventured to offer \$2 a day. He refuses, however, to work for less than \$2.50. Not being willing to pay so large an amount I let him go; I leave it to your judgment to decide whether it would be worth trying him, say for a few days or a week, at that rate, or whether it would be better to continue as at present with the mythologically, if not perhaps linguistically, poor text material I

am now getting, on the hope of supplementing it with better myth material in the winter.

Sincerely yours,  
Edward Sapir.

- 
- 1 No copy of this letter survives, nor of any of Kroeber's letters to Sapir while he was in the field with the Yana.

[22]

Montgomery Creek, Cal.  
August 5, 1907

My dear Dr. Kroeber,

I am sending you by express a package of plant specimens with numbered tags referring to my notes of Indian names and uses. If you can have them identified for me at your earliest convenience, I shall be greatly indebted to you. Will you kindly let me know if you have received the baskets?

Sincerely yours,  
Edward Sapir.

[23]

Montgomery Creek, Cal.  
August 17, 1907

My dear Dr. Kroeber,

I have received the salary check for \$50.00 and wish to thank you for the same; the voucher is being forwarded to the Regents by return mail. As to my progress in Yana, I am still working with Betty Brown on the basis of 4 days out of the week. As the texts I am able to get from her are not chiefly original mythological material and therefore not the most desirable kind of material, it seems likely, if I do not before long succeed in hiring another informant, that the best plan would be to close up in the first week or two of

September with the expectation of returning in the winter, when the Indians will presumably be less in demand as help. The elucidation of the language itself is going on satisfactorily enough, but the virtually limitless number of verbal suffixes makes the complete mastery of Yana grammar practically an endless job. As a matter of fact I am more than half persuaded that a large number of the suffixes had best be considered, in analogy to Jones' Algonquin terminology,<sup>1</sup> secondary stems or stems of second position which unite with primary stems but can never be used alone. Some of these secondary stems are so alarmingly special and concrete in significance that one hesitates to group them together with the more purely modal and local suffixes, of which alone the number is disturbingly large. E.g. -lili-, which never occurs first in the word, means 'smooth, slippery': yullilisi 'he mashes it down absolutely smooth'; dalilī'sasi 'it looks smooth'; djalilī'p!asi 'it slips down in eating (like oysters)'; buī'lilip!asi 'it is slippery (to one walking on it)'. While -lili- has only an ancillary existence, the syllables that precede it have, although perhaps less concrete in significance, greater independence in use and must hence be considered the main verb stems; buī-, e.g., denotes forcible action with the feet: buīsi 'he kicks him'. Other secondary stems (or suffixes, if you prefer) are syllables denoting ideas such as: wake up; break; peel off; blue; green; black; blind man walks; break piece out of something. Practically every verb of more than one syllable (excluding, of course, purely formal elements of mode, tense, and person) must be considered as probably consisting of main stem + 1 (or more?) secondary stems + (ad libitum number of) local and modal suffixes; of course suffixes may be and often are entirely absent. Incorporation of nouns occurs rather frequently but seems obligatory (or even possible) only in certain cases; e.g. with stem mits!- 'have' is combined au- of á(ua) 'fire' into mits!áusindja 'I have fire'. Some nouns appear slightly changed when incorporated: bá(ua) 'deer' but -wai-, e.g.: dji/wái/si 'it tastes like deer meat'; bul/wai/si 'there are 3 deer' (bul- is '3'---all numerals are verb stems and all constructions involving numerals are really verbal in form).

There are many other points of interest in Yana; indeed, it is a very queer language and quite unlike what seems to be the most common Californian type (Yuki, Yokuts, Maidu). I shall not take up more of your time, however,

except to mention a trait that reminds me of Kwakiutl and Salish: certain adverbs such as 'and', 'not', 'intending', 'looks as if', 'if', 'although', take the tense and person endings, the verb following without them. E.g. we have *opdjī'sindja* 'I kill him' but *k'usindj opdjī<sup>ε</sup>i* 'I do not kill him'; *dè'waisp'andja* 'I would have seen it' but *asp'andj dè'wai<sup>ε</sup>i* 'if I would have seen it.' For the last sentence women say: *asp'andj dè'wai<sup>ε</sup>1*.

Yours sincerely,

Edward Sapir.

- 
- 1 William Jones had recently published his dissertation on Algonquian morphology (Jones 1904). Jones, a brilliant scholar, and by descent a Fox Indian, was killed while doing fieldwork in the Philippines in 1909. See Boas 1909.

[24]

Redding, Cal.

Dec. 6, 1907

My dear Dr. Kroeber,

I have arrived here this morning, have looked up Sam Batwi,<sup>1</sup> and had half a day's work with him at the hotel. He promises to give information daily at the hotel, the rate being \$1.50. As he is a speaker of the S. dialect and seems to know the myths, he will probably prove to be a valuable supplement to Betty Brown. Her statement that her dialect was limited to the immediate vicinage of Montgomery Creek is confirmed by Sam Batwi, so that the geographical distribution of the two dialects seems to have been quite unequal in extent.<sup>2</sup> To talk N. Yana is to *garī<sup>ε</sup>i*, to talk S. Yana is to *gat'ā<sup>ε</sup>a* (ga- 'to say, speak, utter'). Please address communications c/o Hotel Lorenz, Redding, Cal.

Yours sincerely,

Edward Sapir.

- 
- 1 Sam Batwi had been one of Dixon's informants in 1900, and earlier an interpreter for Curtin (Sapir 1910a:3-4). Sapir probably learned of his whereabouts during the fall, while he was back in Berkeley between field trips.



2 As Sapir was later to discover, there were actually three major dialects of Yana: Betty Brown's Montgomery Creek dialect (Northern Yana), Sam Batwi's ordinary dialect, from Cow Creek (Central Yana), as well as an Antelope Creek dialect he recalled from his childhood (Southern Yana). Sapir also collected Southern Yana material from Betty Brown, and he felt that Ishi's "Yahi" represented the same dialect. See Sapir 1910a:2-3; Sapir and Swadesh 1960:13-15; and Letter 195.

[25]

[Berkeley, Calif.]

Feb. 8, 1908

My dear Dr. Kroeber,

Inasmuch as I have left only \$7.50 of the appropriation put at my disposal, I should be greatly obliged to you if you could let me have a check for a matter of \$100.00 or so, so that I may be enabled to pay Miss Vore for the work she is now doing for me.

Yours sincerely,

Edward Sapir.

[26]

[Berkeley, Calif.]

Feb. 13, 1908

My dear Dr. Kroeber,

I have just received your bill for \$100.00 for which pray receive my thanks. I have presented it at the Secretary's office and expect to have the check mailed to me in 6 or 7 days. I enclose a statement of expenditures since the last statement rendered to you; it does not include the expense incurred in Miss Vore's typewriting that she is now doing for me at her home and of which I shall give you a statement on the completion of the work.

As regards openings for next year, I am just at present as much in the air as ever. Dr. Gordon has written me that my application for the

Harrison Fellowship (with income of \$800.00) is still on file and I have written the Dean in regard to the matter. Judging from the tone of Dr. Gordon's correspondence, there seems good reason to believe that I shall obtain the fellowship this year. Naturally the Harrison Fellowship for next year, coupled with more or less definite prospects of advancement in the Dep't, would be preferable to my present position as Research Assistant here.

Hoping that you will have a bully trip among the Mohave, I remain

Sincerely yours,

Edward Sapir.

[27]

[Berkeley, Calif.]

Feb. 20, 1908

My dear Dr. Kroeber,

Dr. Gordon has written me recently calling my attention to the fact that the Harrison Fellowship can be awarded only to one already possessing the doctor's degree, though it may also be awarded conditionally to the candidate's being about to receive the degree, in which case the incumbency does not actually begin until the degree has been conferred. Inasmuch as the incumbency of the Harrison Fellowship begins in October and, according to plans up to date, I should not be enabled to take my final examination until that time, it might very easily happen that my not actually having the degree would stand materially in the way of my getting the award of the fellowship. I should therefore like to suggest to you the possibility of my leaving for New York some time between April 15th and May 1st (commencement takes place on May 24th this year at Columbia), so that I may be in time to take my doctor's examination before the summer. This would doubtless, if made known to Dr. Gordon and the Dean of the Graduate School of the University of Pennsylvania, remove the objection they might otherwise encounter towards the award of the fellowship to me. I should like especially to make it

clear that I shall not take my departure from Berkeley before the Yana Texts are ready for the printer and that in New York, as well as here, I shall continue to work upon my Yana grammar until July 1st plus the time taken up by traveling to New York and attending to the degree. In this way the terms of our original agreement will not be altered in any essential respect.

Kindly let me know at your earliest opportunity what attitude you assume towards my proposal. With best regards

Yours sincerely,

Edward Sapir.

[28]

[Berkeley, Calif.]

March 12, 1908

My dear Dr. Kroeber,

I should like to acknowledge receipt of list of my expenditures up to date. I should like to call your attention to the third item in this list, which reads

Fieldwork, December,           \$84.45

My own statement, according to my account, dated Dec. 28, '08, summed up to \$90.40. This would make a total expenditure of \$420.70, leaving a balance on hand of \$79.30. With this correction our accounts tally perfectly.

Yours sincerely,

Edward Sapir.

P.S. I have been to see Pres. Wheeler about leaving around May 1st for New York. As he raised no objection whatever, I have written definitely to Dr. Boas as to my intention to take the doctor's examination at Columbia before Commencement.

[ 29 ]

[ Berkeley, Calif. ]

March 17, 1908

My dear Dr. Kroeber,

I am glad to learn that you have been able to reconcile our two accounts. As to my "Yana Texts", I expect to have everything ready for the printer by April 10th or thereabouts.

Yours sincerely,  
Edward Sapir.

[ 30 ]

Faculty Club  
University of Calif.  
March 27, 1908

My dear Dr. Kroeber,

I have received a draft for and deposited \$220.00. I have just received a reply from Little, Brown, and Co. to the effect that Mrs. Curtin, who holds the copyright, has no objection to my reprinting "The Theft of Fire," provided due acknowledgement is made.<sup>1</sup> The coast is thus clear all around.

Yours sincerely,  
Edward Sapir.

---

1 Sapir had re-elicited this myth from the Curtin collection (Curtin 1898: 36-70) by reading Curtin's English text to Betty Brown and having her translate it back into Yana sentence by sentence (Sapir 1910a:160).

[31]

[Berkeley, Calif.]

April 15, 1908

My dear Dr. Kroeber,

As Tuesday's meeting of the regents developed nothing new for the department and as Dr. Goddard<sup>1</sup> tells me that Prof. Putnam holds forth no prospect of definite information within a month,<sup>2</sup> I thought it best to accept the Harrison Fellowship, at least provisionally. I have accordingly wired my acceptance to Dean Ames.

Yours sincerely,

Edward Sapir.

- 
- 1 Pliny Earle Goddard (1869-1928). At this period Goddard was Assistant Professor of Anthropology at Berkeley, the only other full-time member of the department besides Kroeber. His specialty was Athabaskan linguistics and the ethnography of the Hupa, a northwest California tribe to whom he had once been a missionary. His relationship with Kroeber was not an especially close one; he had little interest in museum work or in ethnographic field work beyond the California Athabaskan groups he knew well. In 1909 he left California for a position at the American Museum of Natural History, where, under Wissler's and Boas's patronage, he achieved a relatively important place in American anthropology through the 1920's. See Kroeber 1929.
- 2 The department was in a period of budgetary stringency brought about by the ending of Mrs. Hearst's private support. Sapir had apparently been told that this meant there was little chance for a more permanent position for him at Berkeley. A passage in a draft of a letter from Kroeber to Putnam, dated 4/15/08 (A.L. Kroeber Papers, Bancroft Library) reveals a more political motive on Kroeber's part:
- I think that he [Sapir] is not the man we want most...because he is no museum man, in spite of his exceptional ability for languages and probably making an excellent lecturer. As I told you, I think Speck a man who in the end would prove more satisfactory. What we need above all are men of unfaltering loyalty to the Dept. The possession of this quality is what has made Barrett so valuable, in spite of his deficiencies; and I feel that in a crisis we could rely more on Speck than on Sapir.
- Kroeber's concern with "loyalty" arose out of a feud that had erupted between himself and Goddard (see Kroeber to Putnam 2/19/08, A.L. Kroeber Papers, Bancroft Library). Goddard had proposed severing the academic department from the Museum and focusing its work on linguistics. A linguist like Sapir might well be expected to side with Goddard if the

dispute were to come into the open; a more museum-oriented man like Speck would probably side with Kroeber. See Letter 42, FN 2.

[ 32 ]

San Francisco, Calif.

April 20, 1908

My dear Mr. Sapir:

I shall probably go south Saturday the 25th, to be gone a week in connection with the Indian conference at Riverside. As you may wish to leave before my return, I should be glad of a chance to see you. Can you come and take dinner at 2848 Washington Street Wednesday? We eat about 6:30, but if you can come earlier I shall be at home and glad to talk over with you anything that may remain to be arranged before your departure.

If convenient, may I ask you to bring over a specimen now in my desk at Berkeley, for which Dr. Goddard has a key? It is one of the shrunk human heads from Ecuador.

Sincerely yours,

[ A. L. Kroeber ]

[ 33 ]

[ Berkeley, Calif. ]

April 23, 1908

My dear Dr. Kroeber,

I find that I have forgotten my umbrella in your study. In case you have an opportunity to come to Berkeley some time before May 4th, I should be grateful to you if you could take it with you. If not, would it be too much trouble to express it at my expense?

Yours sincerely,

Edward Sapir.

[ 34 ]

[ Berkeley, Calif. ]

April 30, 1908

My dear Dr. Kroeber,

Could you kindly let me know what balance of my \$720.00 appropriation I now have according to your account, the \$150.00 that I returned to you being taken account of?

Yours sincerely,

Edward Sapir.

[ 35 ]

[ Berkeley, Calif. ]

May 4, 1908

My dear Dr. Kroeber,

Leaving 10 P.M. today by Santa Fe via Grand Cañon. Please address further communications to

615 5th Ave., Brooklyn, N.Y.

How about the umbrella?

Yours sincerely,

Edward Sapir.

[ 36 ]

Brooklyn, N.Y., May 14, '08

My dear Dr. Kroeber,

I have just received your letter of May 8th and should like to thank you for letting me know what balance is still credited to me; I find that your account agrees perfectly with my own. Enclosed you will find an itemized statement of all expenditures up to date.

Yours sincerely,

Edward Sapir.

P.S. Remember me to Mrs. Kroeber and to Mr. and Mrs. Rothschild.<sup>1</sup> The only disposition of the umbrella now possible would seem to be to sell it and use the proceeds, if any, towards the maintenance of the Department.

---

<sup>1</sup> Kroeber's wife's parents.

[ 37 ]

[Postcard]

[Postmark: Brooklyn, N.Y.  
May 20, 1908]

My dear Dr. Kroeber,

The great event has transpired and the upshot is that our noble Alma Mater has earned \$35.00 and I a doctorate. Boas was characteristically laconic at the exam.

Yours, Edward Sapir.

[ 38 ]

Brooklyn, N.Y.  
July 13, 1908

My dear Dr. Kroeber,

As I presume that Dr. Goddard is now out in the field, I am sending you under another cover Dr. Dixon's Yana text material, normalized in orthography and provided with interlinear and free translations and footnotes, to serve as an appendix under the title of "Supplementary Texts" to my "Yana Texts." Enclosed you will find the receipt for the typewriting expenses incurred.

If you care, as you suggested, to have me go over Dr. Dixon's Chimariko material with a view to grammatically utilizing his texts, I shall be glad to do so now.<sup>1</sup>

Yours sincerely,  
Edward Sapir.



1 Dixon apparently chose to work up this material (collected in 1906) on his own. It was published in Dixon (1910). See Sapir's critical review (Sapir 1911a).

[39]

Philadelphia, Pa.

Oct. 19, '08

My dear Dr. Kroeber,

I was very glad to receive your interesting letter of Oct. 12th<sup>1</sup> and hasten to reply. In regard to the matter of the volume of Memoirs in honor of Prof. Putnam, I shall be very glad to do my humble share. (Dr. Boas has not written me on the subject as yet.) I would suggest that I work up my scattered notes on the Yana that are of an ethnological character into a short paper on the culture of the Yana, including, e.g., what information I have on village names and locations, food plants and preparations, basket designs, and so on. On a few points the paper would probably give a fact or two of interest, in other perhaps more important particulars it would be very defective; this unevenness I cannot help, however, -- the paper would not claim to be more than a set of Notes on the Culture of the Yana Indians.<sup>2</sup> Dr. Dixon's note-book on the Yana, from which I have culled the two supplementary texts that I sent in some time ago, contains also miscellaneous information of ethnological character; perhaps, if I get his consent, I can work his notes in with my own. By the way, I should be very much obliged to you if you could let me have photographs of my Yana specimens (about 20 or so, I believe, of which 14 are baskets) with attached museum numbers (also, if you still have them, with my own tag numbers, to which my notes refer); if you happen to think of it, you might also ask Setchell<sup>3</sup> the Latin name of the Indian tobacco specimen I gave him last winter (there were also several bulb specimens that Hall couldn't identify but said he might grow--perhaps you could get the identifications with my corresponding tag numbers). I shouldn't bother you with these petty things, were it not that it seems necessary to make the most of what little information I have for the purpose of the paper.

Where Bernstein (please remember me to him when you get a chance) got his information as to my having been out in the field I don't know. The truth is that the only field work I did was to visit Coney Island once or twice, the rest of the summer being taken up with sweltering and laziness in Brooklyn. My work with Gordon consists partly of teaching 2 hrs. a week of American Linguistics (the course is to continue throughout the year--I have now about 6 students), partly of preparing my "Takelma Texts" for publication in a new series of Anthropological Papers that Gordon is getting up.<sup>4</sup> Speck, who has been appointed instructor and gives 2 hrs. of introductory Anthropology and 2 of topical Ethnology, is to usher in the great series with his "Ethnology of the Yuchi Indians." It is a good thing, in a way, that I'm to do the Takelma texts now, for I'll probably be getting proof sooner or later of the Takelma Grammar dissertation (to be part of Vol. 1 of Boas' "Handbook of American Linguistics"), and it is well to concentrate on grammar and texts at the same time.<sup>5</sup> This means, of course, that the Yana grammatical work is being laid on the shelf for the while, but, as the Yana Texts are not yet in galley and I shall want to refer to the texts in the grammar, no far greater delay than otherwise need result therefrom.

Remember me to Mrs. Kroeber (I enjoyed her Pima Myths<sup>6</sup>) and to Mr. & Mrs. Rothschild.

Yours sincerely,

E. Sapir.

- 
- 1 Not preserved. Kroeber's letter presumably solicited a contribution to the Putnam Anniversary Volume, a Festschrift for Frederic Ward Putnam on his 70th birthday (1909), for which Boas was general editor.
  - 2 This paper was never finished by Sapir. The manuscript, found among Sapir's papers at his death, was completed and published by his student, Leslie Spier (Sapir and Spier 1943).
  - 3 W. A. Setchell, Professor of Botany at Berkeley.
  - 4 Sapir 1909b.
  - 5 Sapir's Takelma grammar was not included in Volume 1 of the Handbook of American Indian Languages (Boas 1911), but was held over for Volume 2. Although the full volume was not published until 1922, the individual grammatical sketches were issued as separate publications as they were completed, Sapir's Takelma in 1912 (see Sapir 1922a).

- 6 H. Kroeber 1908. Kroeber's first wife, Henriette, accompanied him on several field trips. She published a few short collections of myths.

[40]

Philadelphia, Pa.

Nov. 5, 1908

My dear Dr. Kroeber,

In reply to your letter of Oct. 30th, I should like to say that I shall be glad to get the museum and tag numbers with brief descriptions of the Yana specimens. However, I think it best not to publish the material without the photographs. If you can get the photographs done before the Putnam volume is definitely made up, the paper can be submitted by Jan. 1st and the plates or drawings sent in later by way of supplement. If, on the other hand, it will take too long before you get a chance to photograph the specimens, the best thing to do would probably be to hold the paper for later publication in some other form.

Yours very sincerely,

Edward Sapir.

[41]

Philadelphia, Pa.

March 4, 1909

My dear Dr. Kroeber,

Many thanks for the photographs of Yana specimens that have just arrived. The delay has really not proved fatal in this case, as I had given up the idea of writing a paper on Yana culture for the Putnam volume. This was owing to the fact that I could not possibly finish it by the time set as limit by Dr. Boas, owing chiefly to the necessity of concentrating on my "Takelma Texts" for the University of Pennsylvania, work that is taking much more of my time than I had anticipated. My present intention is, as soon as the Takelma work lets

up, to write the Yana sketch and submit it to you for possible publication in your California series, should it be deemed of enough importance for that purpose. As part of the sketch I have prepared a map showing Yana habitat and villages. I have also had a set of 24 schematic basket-designs drawn, taken from baskets figured in your and Dixon's papers on California basketry and for which I had obtained Yana interpretations (several of the names are not duplicated in my own set). I intend to have a further set of schematic basket-designs drawn from the photographs you have sent me; all told we shall then have a fairly respectable number of Yana designs and design names. I am sorry, however, that you left out the Hat Creek basket (1/13445), for I see from my original notes that it illustrates two Yana design names ("skunk's ear" and "lark's neck") not otherwise found in my material. Could you perhaps send me a photograph or drawing of the basket, or, at any rate, drawings of the designs on it?

Yours very sincerely,

Edward Sapir.

[42]

[handwritten draft]

[San Francisco, Calif.]

5-1-09

Dear Mr. Sapir:

I learn that your Yana Texts have at last moved and are all in your hands. I presume you will be well pleased when they are out of the way. We think well of the new interlinear method on account of the saving of cost, and I trust that you will find it satisfactory.<sup>1</sup>

How is your Yana ethnology?

You have perhaps heard from Goddard that he is about to leave us for the New York museum, which has made him a most flattering offer. Our prospects are looking up, as compared with this year, but as far as Goddard personally is concerned, he seems to feel that the American Museum can do more for him.<sup>2</sup>

I should be glad to hear of your activity and plans, and whether Philadelphia has begun to offer what looks like permanent prospects. I have never

found any outsider who really knew what the movement was likely to amount to in the end. Even Culin<sup>3</sup> seems to have regarded it as a fact rather than a situation. Gordon has plans, which he has told me, but I don't know Gordon well enough to know what relation plans in his mind bear to actual conditions. Shall you be in the field this year?

I am still undetermined, as regards the summer, Goddard's sudden decision having put many things up in the air that seemed settled before.

With best regards,

[A. L. Kroeber]

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- 1 The interlinear English translation in Sapir's Yana Texts is printed as a continuous line, with the translations of individual Yana words separated by vertical bars instead of being centered beneath the words themselves. This "new method", apparently adopted to facilitate typesetting, was also used for Goddard's Kato Texts (1909), but then abandoned.
  - 2 Pliny Goddard resigned from Berkeley at the end of academic 1908-09 to go to New York as Assistant Curator of American Ethnology at the American Museum of Natural History. As noted earlier (Letter 31, FN 2) Goddard's relationship with Kroeber--never an easy one--deteriorated rapidly after the winter of 1907-08, when Goddard proposed to President Wheeler that the Department of Anthropology be reorganized as a Department of Linguistics, with himself as effective head. Kroeber naturally felt this undercut both the Museum of Anthropology and his own position at the University, and he fought bitterly (see Kroeber's letters to Wheeler dated 2/24/09 and 3/2/09, A. L. Kroeber Papers, Bancroft Library). When Goddard received an offer from the American Museum early in 1909 he attempted to use this as a bargaining device, threatening to resign from the University and move to New York if the reorganization was not implemented. Kroeber was able to call Goddard's bluff, by vigorously lobbying with Wheeler and the University Regents for the tabling of Goddard's plan, and Goddard resigned in April. Goddard's disappearance from the scene not only ended a period of considerable uncertainty for Kroeber, but created a significant opening at Berkeley. In this letter Kroeber seems to be sounding out Sapir's interest in the position, but he was also negotiating--more seriously--with Frank Speck (Kroeber to Putnam, 5/21/09, A. L. Kroeber Papers, Bancroft Library). When Speck declined an offer from Kroeber, preferring to stay on at Pennsylvania, Kroeber did not pursue the matter further with Sapir, and instead kept the position open for a year (Kroeber to Putnam, 6/4/09, A. L. Kroeber Papers, Bancroft Library). In 1910 the job went to Kroeber's student and protégé, T. T. Waterman.
  - 3 Stewart Culin (1858-1929), an anthropologist best known for his studies of American Indian games. Culin had been in charge of American archaeology and ethnology at the University of Pennsylvania Museum from 1892 to 1903 (Darnell 1970:82-3). He was subsequently Curator of the Brooklyn Museum for many years.

[43]

Philadelphia, Pa.

May 7, 1909

My dear Dr. Kroeber,

I was very glad to hear from you and to learn of the progress of events in California. I am well satisfied that the Yana Texts have at last been put in my hands, though it will probably take quite some time before I shall be able to return them all corrected. As it happens, the proof of my Takelma Grammar, which, you may remember, is my dissertation and which is going to go into Boas's Handbook, is coming in at the same time, so that I shall have two masses of proof to attend to at once now. Besides, I still have a considerable bit to do before my Takelma Texts are in complete readiness. The reason why this latter work is proceeding slowly is that I am preparing a vocabulary (stems and important derivatives) at the same time that I prepare the MS., to be added, if I can have my way, to the volume as an Appendix; then, also, I am jotting down a great number of grammatical points to be inserted as riders in the Takelma Grammar proof. Add to all this 4 hrs. of teaching and an unconquerable disposition to waste time under one pretext or another, and you will supply the answer yourself to your query: "How is your Yana ethnology?" Nuff said.

I am doing the Yana proof mostly in the evening. It's slow work and rather trying; I confess I should prefer larger type, particularly for the interlinear--but that can't be helped now. I have no complaint to make about the new interlinear method, though I didn't like it much at first, but it needs a lot of correcting in the way of transposing misplaced lines.

Goddard's new move is rather a surprise to me. He had written me that he was to work among the Kiowa Apache this summer for the American Museum, but I did not know that he had since connected himself permanently with it. What's to be done with the Rousselot apparatus in the tin building?<sup>1</sup>

My own plans for next year are still undetermined. Gordon has offered me a regular Instructor's position for \$1200 as a starter with opportunity this summer, presumably other summers also, for field work. He speaks very encouragingly about prospects here--rapid promotion, professorship in the near future, plans for the development of the Department, and so on. But it is difficult to

tell what is substantial and what isn't in Gordon's talk. He doesn't know enough about the necessary work in American ethnology and linguistics to know just what he wants, though he may mean well. I rather think he banks largely on Heye and Hyde<sup>2</sup>, both of whom are now on the Board of Trustees of our Museum, and I should like one of these days to have a chance to talk with one of them. On the whole, though, I must confess that prospects seem reasonable here for me. Speck, by the way, has also been offered an instructorship at the same salary and will probably stay. What makes me a wee bit doubtful as to whether I shall decide to remain in Phil. is an offer that Dorsey<sup>3</sup> made me some time back to attach me to his staff at the Field Columbian for \$1200 a year to begin with, my work to be field research and publication. But, and here comes the characteristic Dorsey note, he wrote me some time back that since he had made his offer circumstances of a "peculiar and quite unexpected" character had arisen which made it impossible for him to consider the matter before about the last week in (last) March. I innocently waited for further information, particularly as he had written that he would like, if possible, to have me keep the matter of acceptance of Gordon's offer open until he could make me a new proposition, but thus far silence. A few days ago I wrote Dorsey a note reminding him of my existence. I'm curious to see what he'll come up with.

Please excuse my unduly personal letter. Best regards to Mrs. Kroeber and Mr. and Mrs. Rothschild.

Yours sincerely,

Edward Sapir.

P.S. I had almost forgotten to say that Gordon is back from his trip to Egypt. It seems probable that I shall go out to the field this summer, but where, I do not know as yet. Speck is thinking of going to Hamilton Inlet, Labrador.

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1 Goddard had been carrying out instrumental studies of the phonetics of several California languages, using a kymograph and other devices. During his stay at Berkeley in 1907-08 Sapir seems to have helped Goddard with the phonetics of Kato (see Goddard 1909:68). Goddard described his phonetic work in a paper in 1905.--The "tin building" was the familiar name for the quarters of the Anthropology Department on the Berkeley campus, a "temporary" structure "with corrugated iron walls and a hangar-type roof" (T. Kroeber 1970:62).

2 Two wealthy amateur archaeologists who gave significant financial backing to

anthropological work in America around the turn of the century.--George G. Heye (1874-1957), founder and chief benefactor of the Heye Foundation, Museum of the American Indian, in New York, was an enthusiastic collector who helped finance several expeditions.--B. Talbot Babbit Hyde (1872-1933), together with his brother Frederick E. Hyde, Jr. (1874-1944), underwrote the "Hyde Expedition", which excavated the ruin of Pueblo Bonito in Chaco Canyon, New Mexico, between 1896 and 1899. B. T. B. Hyde also served for a number of years as Treasurer of the American Anthropological Association.

- 3 George A. Dorsey (1868-1931).--Shortly after receiving his doctorate from Harvard in 1894, Dorsey went to Chicago to join the Field Museum of Natural History, where he became Curator in 1896. Under his direction the Field Museum's Department of Anthropology became one of the major forces in the discipline, ranking with the American Museum of Natural History, Boas's department at Columbia University, and the Smithsonian. Following World War I Dorsey left anthropology and turned to a career as a popular science writer. See Cole 1931.

[44]

Philadelphia, Pa.

May 26, 1909

My dear Dr. Kroeber,

Dr. Dorsey has gone back on his offer to me entirely. Dr. Gordon, it seems, was somewhat rash in offering me \$1200 for next year, his precipitation being due to the offer that Dorsey had made me. As usual, however, Gordon reckoned without his host. The University authorities, according to what he now tells me, will not be willing to pay me more than \$1000 for next year; moreover, the chances for field work this summer seem very slim.

I am thus quite in the dark now as regards the summer's plans. Speck will probably go to Labrador either for the Museum or on his own account; Dr. Gordon informs me that he has decided to stay at the Un. of Penn. next year as Instructor for \$1200 a year. On the whole I find that Gordon has such an uncomfortable way of building card houses and then demolishing them that I shall be glad, sooner or later, to paddle my canoe in other waters. Pardon mixed metaphors. The Yana proof is getting on rather slowly, but surely. Hoping to hear from you in regard to California plans and prospects, I remain

Yours sincerely,

Edward Sapir.



[45]

Philadelphia, Pa.

July 2, 1909

My dear Dr. Kroeber,

Am sending by registered mail the corrected galleys of Yana proof submitted to me some time ago, also the copy of Dixon's Supplementary Yana Myths to be incorporated in the volume. Subjoined is the itemized list of expenses up to date.

Yours sincerely,

Edward Sapir.

Balance on hand, May 14, '08	\$28.10
Typewriting, July 8, '08	<u>7.00</u>
Ink drawings of Yana specimens (Jan. 29 & June 21, '09) as per enclosed receipts	4.75
Typewriting of Dixon's Yana Myths, July 2, '09, as per enclosed receipt	4.25
Postage on registered package, July 2, '09	<u>0.22</u>
	\$16.22
Balance on hand, July 3, '09	<u>\$11.88</u>

[46]

Philadelphia, Pa.

July 6, 1909

My dear Dr. Kroeber,

In reply to your letter of June 29th I may say that 100 copies of the "Yana Texts" is plenty for my own purposes; I surely should not care for more. The doctor's degree, that you were kind enough to ask about, was formally conferred upon me at the last Columbia Commencement in June. As to the New York appointment<sup>1</sup>, of which Frachtenberg<sup>2</sup> told you, I have not yet definitely decided

whether to accept it or stay in Philadelphia as Instructor, Gordon having improved upon his offer to me and outlining prospects that seem at least as flattering as those in New York. At present I am still pretty busy and can hardly hope to get away for the field before the beginning of August. Should I decide to become Assistant Curator of the American Museum, my work will be in the Pueblo region; should I stay here, I do not yet know what field to tackle--possibly the Shoshonean tribes.

Yours sincerely,

Edward Sapir.

- 
- 1 Leo J. Frachtenberg (1883-1930), a student of Boas's who had visited Kroeber briefly in June, 1909, while on his way to Oregon to do field work on Coos.-- Frachtenberg, an Austrian by birth, came to the United States in 1904, enrolling as a graduate student in anthropology at Columbia in 1905. His first field of study was the demographics of immigrant groups, but Boas soon recognized his talent for linguistics. Frachtenberg received his Ph.D. in 1910 for a grammar of Coos, and later worked on Lower Umpqua and Alsea, all languages of the Oregon coast. From 1912 to 1917 he was a member of the research staff of the Bureau of American Ethnology, but was dismissed from this post during the period of anti-German hysteria surrounding America's entry into World War I. His career shattered and his health failing, he devoted the remainder of his life to Zionist organizations.
  - 2 Sapir had been tentatively offered an appointment as Assistant Curator of American Ethnology at the American Museum of Natural History--a position parallel to the one Goddard had recently taken--but the offer was withdrawn. Boas, in a letter to Frederic Ward Putnam dated 8/13/09 (Boas Correspondence, American Philosophical Society), expressed his indignation over the fiasco: The Museum (that is, Bumpus) recently [invited] Sapir to come to the Museum. He made it a condition of his offer that Sapir should not mention it to anyone. When I learned about it by chance, I was rather pleased, to think that Bumpus selected the two men [Sapir and Goddard] for the Museum who are by their training and previous work most strongly committed to the linguistic side,...which the Museum refused to continue while I was there, and I thought the time might be ripe for a re-establishment of the relations between the Museum and the University. I got, therefore, the money for enabling us to offer Sapir a lectureship at the University, after obtaining the consent of the Museum. As soon as this was done Bumpus reared around and said to Sapir, he did not want him; however he wanted him for the Museum alone...When will that scamp break his official neck! Every time a matter of this sort comes up I feel like kicking myself for having allowed myself to be duped by him for so long.

Hermon C. Bumpus (1862-1942), a zoologist, was Director of the American Museum of Natural History from 1902 to 1911. The acrimonious break between Boas

and the Museum administration dated to 1905, when Boas resigned as Curator of Ethnology (to be replaced by Clark Wissler); Freed and Freed (1983:804-810) have recently described the circumstances of this feud.

[47]

[postcard]

On road, Pueblo to Mack,  
Aug. 19, 1909

My dear Dr. Kroeber,

Mason<sup>1</sup>, a Un. of Penn. Museum assistant, and myself are out to Uncompahgre Reservation to try our hands at the Utes. Regards to Mrs. Kroeber.

Yours sincerely,

Edward Sapir.

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1 J. Alden Mason (1885-1967), Sapir's most significant student at Pennsylvania.--A native Philadelphian, Mason received his B.A. from the University of Pennsylvania and began graduate work in anthropology there in 1907. Sapir himself described Mason's graduate career in a letter to Kroeber (Letter 55 below). After Sapir's departure for Ottawa in 1910 Mason transferred to California, where he took his doctorate with Kroeber in 1911 with a dissertation on Salinan ethnology (Mason 1912). After a series of fellowship and research appointments, including another stay in California in 1916 to work on Salinan linguistics (Mason 1918), he became a curator at the Field Museum in 1917, moving to the American Museum of Natural History in 1924, and finally returning to the University of Pennsylvania Museum in 1926. See Satterthwaite 1969.

[48]

[postcard]

Ouray, Ut., Aug. 23, '09

My dear Dr. Kroeber,

I am now located at Ouray among the Uncompaghres, a jolly set but rather difficult to make much progress with because hardly any understand English to any extent. Ute phonetics extremely difficult; "my nose" is mivö' ē<sup>ö</sup>ön',

"your nose"  $miv\delta\bar{t}^{\delta\epsilon.\xi}$  ( $\bar{t}$  = long aspirated t). If you have any proof, send it over here.

Yours,

E. S.

[49]

White Rocks, Utah

Sept. 7, 1909

My dear Dr. Kroeber,

Your letter of the 1st has been received, and I am glad that you think the Shoshonean field a good one. I did not get along as rapidly as I should have liked at Ouray, almost none of the Uncompahgre Utes speaking serviceably intelligible English, and so moved on to White Rocks among the Uintah Utes. I have been working with Charley Mack, an old friend of yours, I believe;<sup>1</sup> he is probably the best informant to be had among the Utes. As I have barely more than a month at my disposal for field work this summer, I shall not have been able to accomplish very much by the time I return in the first week of October. It is my intention, however, to return here next summer earlier in the season and, if possible, to take in the Bear and Sun dances.

I am sure I don't know why Shoshonean linguistics has always been considered so simple. Morphologically Ute is rather a loose-jointed affair perhaps, though there are many features that are as yet quite puzzling to me, but phonetically it is exceedingly difficult, partly owing to a certain obscurity or slovenliness of pronunciation, partly owing to the subtle character of the phonetic system. Aspirations, whispered vowels (in fact whole whispered syllables), glottal catches, nasalized vowels, and doubled consonants all frequently occur and are all theoretically important. The chief difference between Uncompahgre and Uintah Ute seems to lie in the fact that the former dialect has developed the mp, nt, and nq (p, t, and q are "intermediate" between voiced and voiceless) of the latter into long or "doubled" consonants:  $\bar{p}$ ,  $\bar{t}$ , and  $\bar{q}$ , a peculiarity, curiously enough, that characterizes Northern Yana as contrasted with Central Yana. At Ouray I had noted "grouse" (there incorrectly translated as "wild tur-

key") as ayōq̄ap̄ūt̄c (χ = sonant velar spirant, ɔ̄ = close o, ˉ = long consonant); at White Rocks this word metamorphosed into ayūŋqamp̄ūt̄c. There are plenty of apparently primary doubled consonants in Uintah Ute also, however, e.g. t'q̄áq̄aq̄' = (they) eat it. It seems necessary to distinguish between slowly pronounced and "allegro" forms, the latter often sounding alarmingly different from the former:

<u>Absolute</u>	<u>Allegro</u>
māŋac = that one (animate)	māc (, = nasalized vowel)
p'ax <sup>ax</sup> q̄áŋ <sup>ε</sup> umbani = about to kill	p'xā <sup>ε</sup> umbani
p̄ōrūq̄ <sup>ε</sup> wa <sup>ε</sup> aipūga = they moved on	p̄ōrūq̄ <sup>ε</sup> w <sup>ε</sup> apūga

Whispered vowels are particularly important to notice. It seems that very few words really end in consonants; thus: q̄áiv<sup>a</sup> = mountain (v = voiceless bilabial spirant); yámp<sup>a</sup> = camass (cf. Yampár<sup>i</sup>q̄a<sup>ε</sup>m' = camass-eaters, White River Utes; r̄ = voiceless r; -r̄<sup>i</sup>qa-, t'<sup>i</sup>qa- = Shoshone -rika-, -dika-); ōy<sup>i</sup> = salt; quŋc<sup>u</sup> = buffalo (cf. plur. q'<sup>u</sup>t̄cūm'). Glottal catches are very important, cf. wī<sup>ε</sup> = to fall and wī' = knife. Where words seem to end in vowels, they probably nearly always end either in aspiration or catch; thus: pā' = water; qavá' = horse; ō' = arrow (qavá would become qáv<sup>a</sup>).

Some of the more interesting morphological peculiarities so far noted are: distinct forms for singular and plural of verbs in nearly every case (sometimes unrelated stems are used); use of compound verbs (t'<sup>i</sup>q̄áyarī<sup>ε</sup> = sit still and eat!); enclitic use of pronominal elements (yāvayai<sup>n</sup> = I am afraid; áwin yāvayai' = now-I be-afraid; mō<sup>ε</sup>ōman' = hand-with-my); 9 personal endings (1 per. sing.; 2 sing.; 3 sing. animate; 3 plur. animate; 3 inanimate; 1 dual incl.; 1 plur. incl.; 1 plur. exclusive; 2 plur.). On the whole I must confess that I am more interested in Ute phonetics than morphology; the latter is not nearly as interesting as in the case of Takelma or Yana, for instance. It seems to be a somewhat gelatinous affair, leaving no decided impression on the mind.

I have written to Philadelphia for the proof. I am as desirous as you to get the Yana Texts off our hands, as plenty of other work remains ahead. Please congratulate Mr. Waterman for me on his being appointed fellow at Columbia.

Yours sincerely,

Edward Sapir.

1 Kroeber had worked among the Utes in 1899-1901 (Kroeber 1908).

[50]

[postcard]

En route Seattle to Spokane

Sept. 29, 1909

Dear Dr. Kroeber,

We are returning home after a look at the Seattle Fair. Yana revised proof was never received in time & will be returned as soon as possible. Best regards to Mr. and Mrs. Rothschild and Mrs. Kroeber.

Yours sincerely,

E. Sapir

[51]

Philadelphia, Pa.

Dec. 12, 1909

My dear Dr. Kroeber,

Many thanks for your kind remarks about my "Wishram Texts."<sup>1</sup> I am greatly interested in what you say about your Shoshonean Rousselot experiments, and am sending you a carbon copy of an abstract on Ute that I have just prepared for MacCurdy.<sup>2</sup> You will see from what I say there what opinions I hold in regard to Ute phonetics, at least for the present. As for the whole surd-sonant tangle, it strikes me that one can do worse than follow Jespersen's "Lehrbuch der Phonetik,"<sup>3</sup> which I am making some use of in a 1/2 year course on "Phonetics" I am now giving. His chapter on glottal action, pp. 67-113, is worth while reading, anyway. According to my analysis, Ute possesses his "weakly aspirated surds" (§100) and "voiceless mediae" (§102). This is a pairing of stopped consonants that is very common in America (also Takelma, Yana), perhaps more common than that of surd and true sonant.

By the way, I didn't get a copy of your recent "Notes on Shoshonean Dialectics of S. California,"<sup>4</sup> though I have read the paper. There seems to be some hitch about my name's being on the list. I remember I had to write to Goddard for the Sparkman paper,<sup>5</sup> too. If you can do so, I shall be obliged for copies

of your recent "Anthropologist" articles,<sup>6</sup> also the paper that appears in the current number of the "Journal of the Royal Anthropological Institute."<sup>7</sup> Speck would also like to have copies.

Best regards to Mrs. Kroeber and to Mr. and Mrs. Rothschild.

Yours sincerely,

Edward Sapir.

- 
- 1 In a letter that has not been preserved.
  - 2 George Grant MacCurdy (1863-1947), a zoologist by training, and later an influential paleoanthropologist. Affiliated with Yale for most of his career, MacCurdy served for many years as Secretary both of the American Anthropological Association and of Section H of the A.A.A.S. It was in the latter role that Sapir was dealing with him here, submitting the Ute paper to Science (Sapir 1910b). See also Letter 71, last postscript.
  - 3 Otto Jespersen (1860-1943).--Jespersen's Lehrbuch der Phonetik (1904) was the German translation of the first half of Fonetik (1894-96), a work that has been called one of the great monuments of classical phonetics.
  - 4 Kroeber 1909a.
  - 5 "The Culture of the Luiseño Indians" (Sparkman 1908).
  - 6 "The Bannock and Shoshoni Languages" (1909b) and "California Basketry and the Pomo" (1909c).
  - 7 "Classificatory Systems of Relationship" (1909d).

[52]

Philadelphia, Pa.

Feb. 4, 1910

My dear Dr. Kroeber,

Would it be possible to have Mr. Paul Radin<sup>1</sup> put on the list of recipients of publications of the Univ. of Cal. anthropological series? Mr. Radin, as you have doubtless observed, is the author of a set of "Winnebago Tales" that recently appeared in the Journal of American Folk-Lore.<sup>2</sup> He is a pupil of Dr. Boas' and will get his doctor's degree this spring. He has done an immense amount of ethnologic and linguistic work among the Winnebago; a study by him of the Winnebago Midewin is to be published by the Bureau of Ethnology as one of

the Annual Reports.<sup>3</sup> Hoping you will find it possible to accomodate Mr. Radin, I remain

Yours very sincerely,

Edward Sapir.

P.S. I am now working with a young Kaibab Paiute from Carlyle. He seems to be an excellent informant.<sup>4</sup>

- 
- 1 Paul Radin (1883-1959).--A Boasian anthropologist of German-Jewish background, with strong literary and linguistic inclinations, Radin early along became a close friend of Sapir's (and later of Kroeber's). After a precocious childhood, and flirtations with zoology and history, Radin took up anthropology in 1907, receiving his Ph.D. from Columbia in 1910 with the dissertation on the Winnebago Midewin (medicine society) that Sapir mentions in this letter (Radin 1911). Following a brief, unhappy association with the Bureau of American Ethnology, Radin spent a year in Mexico studying the Zapotec and then went to Ottawa for a research post with Sapir. He taught in California from 1917 to 1920; was in England for a while; taught at Fiske University in Nashville for a few years; spent more time in Mexico; and returned to Europe. From 1930 through about 1950 he lived in Berkeley without any steady academic employment, though continuing to write and do research. Towards the end of his life he returned once again to Europe, spending several years in Switzerland and Italy, but at the time of his death was back in the United States teaching at Brandeis. Chronically peripatetic, Radin's interests were extraordinarily broad, his personal manner charming, his fecklessness legendary. For a biographical sketch see Du Bois 1960.
  - 2 Radin 1909. This was Radin's first publication on an American Indian topic.
  - 3 This presumably is the paper that was published in the Journal of American Folklore in 1911 under the title, "The Ritual and Significance of the Winnebago Medicine Dance". A monograph, The Winnebago Tribe, appeared in the BAE Annual Report for 1915-16 (Radin 1923).
  - 4 Tony Tillohash, a student at the Carlisle Indian School, not far from Philadelphia.

[53]

Philadelphia, Pa.

Feb. 25, 1910

My dear Dr. Kroeber,

In reply to your letter of February 18th, I should like to suggest Mr. J. Alden Mason of this city for the \$400 fellowship you think is likely to be



granted your department. Mr. Mason was with me on last summer's trip to the Northern Utes. Besides doing some archeologic reconnaissance for cliff houses and petroglyphs in Nine Mile Canyon, he collected a very considerable body of Uintah Ute mythologic material, most of it new in subject matter. This set of myths has been accepted by Dr. Boas for the Journal of American Folk-Lore.<sup>1</sup> At present Mr. Mason is a scholar in the post-graduate department of the university and has had several years of experience here in handling museum specimens. As for linguistic training he will have had two solid years of my 2 hr. course in American Linguistics (besides 2 extra hours in Paiute I am giving this second half year) and a 2 hr. half-year course in General Phonetics. I believe that you will find Mr. Mason an enthusiastic and capable student and field man. Trusting you will consider him for the fellowship, I remain

Sincerely yours,  
Edward Sapir.

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<sup>1</sup> Mason 1910.

[54]

Philadelphia, Pa.  
March 3, 1910

My dear Dr. Kroeber,

Mr. J. A. Mason, of whom I spoke in my last note to you, is making an application for a fellowship in your department this evening. I shall be glad to give you any information you may desire concerning Mr. Mason.

Yours sincerely,  
Edward Sapir.

[55]

Philadelphia, Pa.

March 16, 1910

My dear Dr. Kroeber,

In reply to your letter of March 9th I should like to submit the following data in regard to Mr. Mason's anthropological work. During his undergraduate course in this university he took practically all of the work of our department that Dr. Gordon was giving. On graduating in 1907 as A. B. he was awarded a half scholarship in Anthropology but, owing to financial reasons, could study for only about one term. The following year he was made a regular Museum assistant and, besides gaining experience in handling specimens, took courses in our department as special student. For the year 1909-10 he is holding a regular University scholarship and is taking courses with Dr. Gordon, Dr. Speck, and myself. The only regular field work he has done was during the summer of 1909, when he spent something over a month under my charge with the Northern Utes. During this time he collected archaeological data, a very considerable body of myths now in Dr. Boas' hands for the Journal of American Folk-Lore, and an extremely good set of photographs. I may say incidentally that Mr. Mason is considered a crack photographer hereabouts.

As to his personal powers it is hard to speak, owing to the fact that he has been given but little chance as yet for field work. As far as I can see, he is very well adapted for technological work; in some basket work he has recently done for Dr. Gordon he developed a great deal of interest and enthusiasm and very quickly learned a great deal about basket weaves, styles of design, and tribal characteristics. I should say that Mr. Mason is eminently fitted for good museum work. I do not believe he can be a successful teacher, owing to the unfortunate circumstance that he stutters. This defect, however, does not seem to have seriously interfered with his work among the Utes, so that you may anticipate a good share of useful field work from him. He has not tried his hand at original linguistic research, but I know that he has a very good ear and is following all my linguistic work (2 hrs. of Wasco and Yana, 1 term each, last year; 2 hrs. of Takelma and Paiute, 1 term each, this year; 2 hrs. of Phonetics, 1 term, this year; 2 hrs. of special work in Paiute, 1 term, this year) with abil-

ity and apparently profit. I believe he can do very creditable field work in linguistics. He is interested in all phases of ethnological work, but, as I have already indicated, I believe his forte would be in material culture.

In regard to personality, I have no doubt you will find him very likeable, though one must make some allowance for his speech defect. He is absolutely reliable in all respects and will not be averse to putting in good hard work. On the whole I think you would do very well to award him the fellowship.

Yours sincerely,  
Edward Sapir.

[56]

Philadelphia, Pa.

Apr. 7, 1910

My dear Dr. Kroeber,

Enclosed you will find the Yana vocabulary you have asked for.<sup>1</sup> In some cases I have not been able to find the words, though it is likely that for some of these more thorough ransacking of my notes would yield something. Dr. Dixon's Yana notes have helped out in a number of them; if I should obtain new data in regard to these and the missing words, I shall be glad to let you know. I am afraid that the extremely synthetic character of Yana will make most of the words unsuitable for comparative purposes. Thus p'ubílla "duck" consists of p'ũ- "swim", -bil- "about, here and there", and -la (from -na), noun ending; diláumauna "dead" is really "being dead" from dilau- "die", itself to be analyzed as di- "move in a straight line" and -lau- "from an enclosed space out into the open, from the plain on to the mountain". I know of two really striking resemblances between Yana and Wintun (Curtin's "Creation Myths"<sup>2</sup>): Yana s'áuna (stem s'au-) "acorn bread", Wintun sau; Yana yúna (stem yu-) "acorn", Wintun yu. There is also a rather striking resemblance between Yana cuk!ún-na "[illegible: coiled?] basket" and Takelma cugun-i- "root basket". There are three other resemblances between Takelma and Californian languages: of ta!íxi "dog" I have already spoken in an Anthropologist article;<sup>3</sup> Takelma ó<sup>u</sup>p' "tobacco" is practically identical with Achomawi ōp'; Takelma degès "sifting basket-pan" is identi-

cal with Wintun tekes. You will observe that all of these words are names of cultural objects, a class of words most liable to borrowing.

In regard to the "Yana Texts", I have as yet found only a few errors, which I list for you on a separate sheet. If I come across new ones, I shall send them on to you. On the whole I am well satisfied with the volume. Mr. Allen has sent me a list of those I tabulated who get the volumes of the series.

I have hardly done anything on working up Yana grammar since leaving Berkeley; barely half of my MS material has been collated under stems, grammatical elements, and processes. There can be no talk of writing the grammar in final form until all such collation has been done. Last year I was too busy with my soon to appear "Takelma Texts" and with proof-reading for my "Takelma Grammar" into which I introduced a very large number of inserts, to think of doing much with the Yana material. This year, again, I am head over heels in Paiute, being continually occupied since February with my Carlisle informant,<sup>4</sup> from whom I am getting lots of good stuff (I have already over 175 songs, most of them mourning or "cry" songs with Mohave words. Would you care to have me send you one or two texts of songs for diagnosis?). Nevertheless, I am very eager to get at the Yana material, for I am perhaps more interested in Yana than in any other Indian language, not even excepting Paiute, about which I am naturally most excited at present. Before buckling down to Yana grammar, however, I should like to get off my hands the "Notes on Yana Culture;" I expect to get at that some time this summer, after my informant has returned to Carlisle.

Let me be frank with you. I can hardly expect the Museum to let me devote my time to Yana next year when I have been and am gathering material for publication in its own series.<sup>5</sup> This means that practically only certain evenings will be left open to me next academic year for work on Yana. As I understand it, the \$600 that I received as research assistant were intended to pay for field work and as much working over of material as could be done within the year. Hence you can hardly expect me to go on indefinitely with the Yana work, when more than half yet remains to be done, and that in the evenings besides, except on some definitely understood basis. I suggest that I be compensated at so and so much per hour, but leave it to you to suggest more explicit arrangements.

I have not yet received a copy of your "Notes on Shoshonean Dialects of

California." Could you let me have a copy? If you could spare separates of your papers on "Noun Composition" and "California Archeology" in the Putnam Volume, I should be greatly obliged.<sup>6</sup> Please remember me to Mrs. Kroeber and Mrs. Rothschild.

Yours sincerely,

Edward Sapir.

- 
- 1 The letter with Kroeber's request for this vocabulary has not been preserved, but Kroeber apparently asked for the Yana equivalents of a standard list of 225 glosses. He had begun to compile a comparative vocabulary of all California languages in order to study patterns of borrowing and "in the event of any relationship existing between languages then considered unrelated, to determine this fact" (Dixon and Kroeber 1919:49). This was the first step toward the discovery of the "Hokan" and "Penutian" groups.
  - 2 Curtin 1898:70.
  - 3 "Notes on the Takelma Indians of Southwestern Oregon" (1907b).
  - 4 Tony Tillohash.
  - 5 That is, his Paiute material. In fact, however, Sapir's study of Southern Paiute was not published by the University of Pennsylvania, which Sapir was shortly to leave, but--after a delay of two decades--by the American Academy of Arts and Sciences (Sapir 1930-31).
  - 6 Kroeber 1910a; 1909e.

[57]

Philadelphia, Pa.

June 6, 1910

My dear Dr. Kroeber,

I understand from Mr. Mason that he has been appointed fellow in your department. I hope that you will have every reason to be satisfied with him, despite the speech defect that I referred to some time ago. Mr. Waterman<sup>1</sup> stopped over here on his way back to California via Washington and Georgia. He is evidently brimful of enthusiasm for his recent work with Dr. Boas and is looking forward with gusto to his new work in California.

Will you kindly add the following entries to our little Yana account, an account making up in long-windedness what it lacks in quantity.

Express charges on payment of "Yana Texts" (as per enclosed bill and receipt of Apr. 22, 1910)	.....\$3.00
Cartage on 1 box books from California Hall to S.P.R. (as per enclosed bill of Apr. 6, 1910)	..... 0.50
Freight charges on above box (as per enclosed bill of March 18, 1910)	..... 2.60
Cartage on above box from R.R. station to Museum (as per enclosed receipt of May 1, 1910)	..... 0.50
	----- \$6.60

As the above expenses were unavoidably incurred in securing the 100 copies of "Yana Texts" to which I was entitled, they are properly to be charged to the Dep't of Anthropology. Enclosed you will also find a new set of corrections to the texts.

Yours sincerely,  
Edward Sapir.

---

1 Thomas Talbot Waterman (1885-1936), Kroeber's student and--from 1910--colleague at Berkeley.--"One of the vivid figures of American anthropology" (Kroeber 1937:527), Waterman was a clergyman's son who switched his studies from divinity to anthropology shortly after getting his B.A. from Berkeley in 1907. He was a museum assistant for Kroeber from 1907 to 1909, while doing graduate work and field research. He spent 1909-10 studying with Boas, receiving his Ph.D. from Columbia in 1913 with a dissertation on Yurok linguistics. He taught at Berkeley from 1910 to 1918, and again in 1920-21. In his later years he held a series of teaching and research appointments of no great consequence. Kroeber described Waterman's personality as "simple, sincere, direct, always vigorous, often drastic" and noted that "some were offended by his brusqueness" (1937:528). Sapir was one of the latter.

[58]

Philadelphia, Pa.

June 22, 1910

My dear Dr. Kroeber,

Enclosed I am sending a combined receipt for expenses of freighting and carting copies of "Yana Texts;" this was received from Berkeley after I last wrote you. On looking up my account I find that I had omitted two small items;

these are to be added to my bill.

Oct. 15, '09--Postage for Registered Package of Proof	\$0.21
Jan. 17, '10--Dit.	0.23
	\$0.44

Balance, July 3, '09 -----	\$11.88
Expenses incurred since July 3, '09 -----	7.04

Balance on hand, June 22, '10-- \$4.84

In another decade I venture to predict that this scrawny sum will have evaporated into nothingness.

I am afraid that I'm to do no field work this summer. I regret this the less that I have secured a good body of material during four months work with my Paiute Indian from Carlisle. Please remember me to Mrs. Kroeber and Mrs. Rothschild.

Yours sincerely,  
Edward Sapir.

[59]

[San Francisco, Calif.]

August 30, 1910

Dear Mr. Sapir,

I hear from Mason that you have had a fine opening in Ottawa,<sup>1</sup> and I take pleasure in extending to you my heartiest congratulations. I hope that it may be all that you wish. I should be glad to hear from you at your leisure concerning your prospects.

Sincerely yours,  
[A. L. Kroeber]

<sup>1</sup> Sapir had accepted the post of Chief of the Division of Anthropology, Geological Survey of Canada, "the Canadian analogue of Major Powell's position

with the Bureau of Ethnology at the Smithsonian" (Murray 1983:72). Boas played a major role in securing this appointment for Sapir (Preston 1980, Murray 1981).

[60]

Alberni, B.C.<sup>1</sup>

Nov. 23, 1910

My dear Dr. Kroeber,

I have your note of Nov. 11th. I can think of no Yana word like komodo meaning "go away" or "go back". "To go away" is nisá- (man), 'asá- (woman), or 'ánsa- (plur.); "to go back" is nidúsa-, 'adúsa-, or 'ándusa-, also ídusa- (plur. p'ídusa-).

I had hoped to be able to stop off at San Francisco on my way back to the east, but as it is, it does not seem likely that I shall have the opportunity. On the whole I believe I have been fairly successful this trip. Shall probably stay on for a week in December. Please remember me to Mrs. Kroeber and Mrs. Rothschild, also Waterman and Mason.

Yours sincerely,

Edward Sapir.

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1 On Vancouver Island. Soon after taking up his duties in Ottawa, Sapir travelled to the west coast to determine the feasibility of working with the Nootka, a relatively unstudied group related culturally and linguistically to the Kwakiutl with whom Boas had worked.

[61]

[Postcard]

Victoria, B.C., Dec. 9, 1910

My dear Dr. Kroeber,

Have just spent an enjoyable day with Newcombe<sup>1</sup> preparatory to making for Ottawa. Am well satisfied with the Nootka as an object of study and hope I shall be able to spend several years on them. Ethnologically the most prominent



trait is the far-reaching specialization of nearly all cultural traits in families (e.g. there are several marriage ceremonies, methods of bewitching, whaling methods in each tribe; each family possesses an indefinite number of songs, secret medicines, charms). Linguistically Nootka is undoubtedly related to Kwakiutl, but considerably specialized phonetically; seems to have many more derivative suffixes than Kw. Regards to Mrs. K.

Yours, E. Sapir.

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1 Dr. Charles F. Newcombe (1851-1924), a retired physician who had made a number of studies of the history, archaeology, and ethnology of Vancouver Island.

[62]

Geological Survey

Ottawa, Ont., Feb. 20, 1911

My dear Dr. Kroeber,

I have your note of Feb. 14th in regard to estimates of material to be published by your department during the coming year. Owing to stress of other work I have been unable since leaving San Francisco to continue on my Yana grammar. What spare time I have in the evenings now that I care to devote to non-Canadian work I am using to get my Paiute and Ute texts into shape for publication, as it seems desirable to me to get text into print as soon as possible, grammatical results following at a leisurely pace. I cannot set any definite date at which my Yana grammar is to be completed, but I am very eager to get at the work as soon as may be. If you wish, you may announce my grammar as "in preparation," using the title "Phonology and Morphology of the Yana Language."

Yours sincerely

Edward Sapir.

[63]

Ottawa, Ont.

Apr. 29, 1911

My dear Dr. Kroeber,

You may be interested to know that I got married last Sunday, Apr. 23rd. Florence<sup>1</sup> is a most charming girl and I should be delighted if you and Mrs. Kroeber could meet her this summer. Could you not manage to pass through and stop over at Ottawa on your way to or from New York? You see, I have heard that you're to give summer school courses at Columbia this year.

My Nootka work is not advancing very rapidly, as most of my time is taken up with trucking and unpacking boxes, checking off specimens, and such other work. My assistant, Mr. Barbeau,<sup>2</sup> is at present getting what field data he can on Huron. This summer Dr. Goldenweiser<sup>3</sup> is to work up Iroquois totemism for us in Ontario, Mr. Mechling<sup>4</sup> is to do Malecite, and Dr. MacMillan of McGill is to collect Micmac data. I shall probably not continue on Nootka till say September. I have not done as much as I expected on my Paiute texts recently, as much of my time was taken in preparing a lecture on "The History and Varieties of Human Speech," which I delivered at Philadelphia on April 1st.<sup>5</sup> As for Yana, requiescat in pace for the present. But a day of awakening will come, never fear. Please remember me to Mrs. Kroeber and Mrs. Rothschild.

Yours sincerely,

E. Sapir.

- 
- 1 Florence Delson (shortened from Seidelson). Like Sapir, she was from an immigrant Jewish family, and also quite gifted. At the time of their marriage she was a student at Radcliffe. For biographical and personal details see Perry 1982:244-245.
  - 2 Marius Barbeau (1883-1969). A folklorist of French-Canadian background, Barbeau joined the Survey in January 1911, remaining with it (or with the National Museum of Canada) until his retirement in 1949. See Preston 1976.
  - 3 Alexander A. Goldenweiser (1880-1940).--An urbane scholar from a cultivated Russian-Jewish family, Goldenweiser studied philosophy and anthropology at Harvard and Columbia and took his Ph.D. under Boas in 1910 with a dissertation on totemism. His two seasons of work among the Northern Iroquois, in 1911-12, were Goldenweiser's only major field experience. He was best known for his lecturing (at Columbia and the New School, in New York, and later at

the University of Oregon) and brilliant writing. See Wallis 1941, and Letter 81.

- 4 William H. Mechling, a student of Sapir's from Pennsylvania. He appears to have left anthropology after World War I. See Letter 81.
- 5 Sapir 1911c.

[64]

Affiliated Colleges

San Francisco, Calif., June 1, 1911

Dear Dr. Sapir:

My best congratulations. I am sure Mrs. Sapir is charming, and, what is almost equally important, intelligent. Please give her the best wishes of Mrs. Kroeber and myself, and advise her for us that she ought to make you get over being so apologetic. We are really not at all surprised at your getting married: most men do, and generally to less worthy partners.

I regret that we can not avail ourselves of your kind invitation to stop over in Ottawa. I shall probably make the trip without Mrs. Kroeber and am likely to be so short of time as to have to take the most direct route to and from New York.

Mason has passed his examination and gone home as a doctor of Philosophy. I am sorry we have no permanent opening for him but as things stand at present, Waterman, Nelson<sup>1</sup> and myself are all that the University can carry. His thesis on Salinan Ethnology<sup>2</sup> is an excellent piece of work and shows more grasp of the ethnography of California than I thought he could acquire in so short a time. His linguistic material was somewhat restricted by eternal difficulties of the kind you would have encountered if your Yana had been Spanish speaking Catholics of 140 years standing, but I think he has succeeded in determining the fundamental traits of the language so far as they persist.<sup>3</sup> He expects to hold a fellowship in Mexico next year, he writes me. He would be a good man for an institution like yours or the Bureau, and I should be glad if you could sometime find a place for him. I am much pleased, by the way, to hear of your multiplying activity and its evidence of resources at your command. Are you running the others, or are you all cordially taking orders from some non-anthropological authority?

As to Yana, I am glad it only slumbers. With the end not in sight, however, don't you want to get out a grammatic sketch?

With best regards to No. 1, and yourself,

Sincerely,

A. L. Kroeber

- 
- 1 Nels C. Nelson (1875-1964), an archaeologist, later associated with the American Museum of Natural History.--For an evaluation of his pioneering work in stratigraphic excavation, see Woodbury 1960.
  - 2 Mason 1912.
  - 3 Mason returned to California in 1916 on another fellowship to continue work with Salinan linguistics (see Mason 1918).

[65]

[Telegram]

San Francisco Calif Sept 6th - 1911

Dr Edward Sapir, Geological Survey Ottawa Ont

Have totally wired southern yana at museu<sup>1</sup> also sam Batur fairly successful interpreter do you want to come and work him up if so both at your disposal if not so would appreciate immediate sending of list of yana grammatical elements for better understanding of language and analysing of texas

A L Kroeber

---

1 I.e., "Have totally wild Southern Yana at Museum." This was, of course, Ishi (d. 1916), the story of whose discovery and subsequent life at the Museum of Anthropology in San Francisco has been vividly told by Theodora Kroeber (1961). (See also Heizer and T. Kroeber 1979 for contemporary reports.) Ishi had been with a small group of Southern Yanas that had taken refuge in the back country during the period of White-Indian hostilities. When everyone else in the group had died, Ishi, then in middle age, gave himself up by walking into Oroville, Calif., on August 29, 1911. He spoke no English, only Yana, and was nearly naked. The authorities contacted Kroeber and Waterman, who took Ishi under their charge and brought him to San Francisco a week later. They were able to establish some degree of communication with Ishi through

Sam Batwi (see Letter 24), a Central Yana who knew a little Southern Yana.

[66]

Geological Survey

Ottawa, Ont., September 7th, 1911

Dear Dr. Kroeber,

I have just received your night letter of September 7th. I do not quite understand all it contains, but gather that you have found an Indian that is able to speak the southern Yana dialect, and that he is now at the Museum. I further gather that Sam Batwi is to serve as interpreter. As you probably know, I have a very small amount of southern Yana material scattered around in my own manuscript, but the opportunity to work the dialect out thoroughly is not to be neglected. Naturally it is not possible for me to seize the opportunity myself, so that I hope that you or someone else in California will take the matter up. I shall send you a list of Yana grammatical elements at the first opportunity. I presume you would be more interested in getting the purely formal rather than derivative elements. There are so many of the latter that it would take quite a while to make up a complete list.

Yours very sincerely,

E. Sapir.

[67]

Geological Survey

Ottawa, Ont., September 11th, 1911

Dear Dr. Kroeber,-

Under another registered cover I am sending you some grammatical material in regard to Yana. It comprises lists of verbal and nominal derivative suffixes and pronominal, tense-modal, passive, participial, and interrogative formal elements. These lists should by no means be considered complete, particularly in regard to the verbal derivative suffixes. I have small doubt that if all my

manuscript material were systematically collated, which as yet it is not, I should be able to give half as many again. There are several rules in regard to changes of stem vowels, accent, and other matters, which one should really know before he can analyze forms satisfactorily, yet I hope that what I am sending you will be enough for your purpose. Kindly inform me from time to time as to your success with your Southern Yana informant. I am rather curious about the matter, as I should like to see how your results compare with what little Southern Yana material I have been able to secure from Sam Batwi. Phonetically I believe that Southern Yana is characterized by many more aspirations than Northern or Central Yana.

With best regards to Mrs. Kroeber and Mrs. Rothschild,

Yours very sincerely,

E. Sapir.

[68]

Affiliated Colleges

San Francisco, Calif., September 12, 1911

Dear Dr. Sapir:

Your letter of September 7th at hand, and I am glad to hear that we shall soon receive the list of grammatical elements. The Indian belongs to the Mill Creek party that has been in hiding for forty-five years and is totally wild. Sam Batwee's Southern Yana is either considerably different from this man's or he has pretty thoroughly forgotten it, as they understand each other only imperfectly.

With best regards, I am,

Sincerely yours,

A. L. Kroeber

[69]

Affiliated Colleges

San Francisco, Calif., October 7, 1911

Dear Dr. Sapir:

I received your list of Yana grammatical terms and wish to thank you heartily for them, and your prompt attention to the matter. I have of late not been able to do anything with Ishi, as we have named our wild man, on account of the rush of affairs connected with the opening of our Museum to public exhibition, which occurred yesterday. Mr. Waterman has also had his time pretty well taken up but has had leisure to get some good texts, Rousselot apparatus tracings, and miscellaneous information. Sam Batwi has been with us for a month, but proves to be only an indifferent interpreter. The two dialects are tolerably distinct and I presume that Sam is too old to make himself acquainted very readily with a new form of speech. I think, by the way, that you must have misunderstood Sam when you stated in your Yana Texts that he had been born among the Southern Yana. He claims to be from Cow Creek and it is evident that he never knew anything of the dialect which Ishi speaks. You will see from the above that it is too early to say anything about linguistics except that it is perfectly plain that this man's language is an offshoot of Yana. The phonetics and apparently a majority of the stems are identical. I will take pleasure in keeping you posted as we learn anything. I may add that there is no question of this man having been wild all of his life. He would be delighted to be able to talk Yana with you.

With best regards, I am,

Sincerely yours,

A. L. Kroeber

[70]

Geological Survey

Ottawa, Ont., November 2nd, 1911

Dear Dr. Kroeber,

I was much interested in your letter of October 7th. I had seen quite a good deal about Ishi in newspaper clippings that various friends had sent me, but was glad to have your letter confirming statements made in regard to his almost absolute wildness. I am sorry that Sam Batwi proves a rather indifferent interpreter. In my first foot-note of page 6 of "Yana Texts", I did not exactly say that Sam was born south of Battle Creek but admit that I implied it. My original field notes show that I read too much into what Sam had told me. They run as follows: "Sam used to use this language [Southern Yana] when a boy down at k!úwiha and neighboring places but gave it up for the northern language [i.e., Central Yana] when moving up to Cow Creek. His father was from Old Cow Creek and mother's mother was from Big Meadows, but mother was half Yana and half Maidu." From this you see that Sam would seem to have had a passing acquaintance with Southern Yana in childhood, an acquaintance which, according to his own statement to me, was practically entirely lost in later life. Of course I had no right to imply that he was born in Southern Yana territory.

I am in hopes particularly that either you or Waterman will be able to get much of ethnological interest out of your find, but can readily imagine how, with his complete ignorance of English and Sam's lack of thorough familiarity with Southern Yana, you will find it rather slow work. I hope that my published texts and the list of elements that I sent you some time ago may be of some assistance in analyzing Southern Yana texts. Please do not fail to inform me of your progress. You may remember that when the Putnam Anniversary Volume was in progress I began to put my scattered Yana ethnological notes into shape but found that I did not have time to finish the article. I have lately resumed the matter and hope some time before spring to send you a paper entitled "Notes on the Culture of the Yana".<sup>1</sup>

E. Sapir.

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<sup>1</sup> Published posthumously (Sapir and Spier 1943). See Letter 39, FN 2.



[71]

Affiliated Colleges

San Francisco, Calif., Nov. 18, 1911

Dear Dr. Sapir:-

I am very much interested in your article in the "Anthropologist",<sup>1</sup> and delighted with it. If only one or two of the authors who have touched on the problem in regard to a single language had shown as much insight as you did with half-a-dozen, the matter would have been clear long ago, and my paper<sup>2</sup> would never have had to be printed. As it is, I am glad that my challenge was productive to your essay.

I have very little new to report about Ishi. He has learned to speak no English whatever, and both Waterman and I have been too busy with other things to pick up much of his language. We have some excellent texts and other information, but Sam's translations are nearly hopeless. A short comparative list which I enclose may be of interest to you. His language would be an easy matter for anyone half-familiar with northern Yana, and I wish that we could put in some time at your disposal. He is perfectly tractable. It does not seem to enter his head that a better means of communication between himself and us is called for.

I shall be delighted to receive your article "Yana Culture" as soon as possible. The Secretary of the University Press informs me that there is at present a dearth of material in the printing office, and that papers handed in at the present time can be put through with unusual despatch.

Sincerely yours,

A. L. Kroeber

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1 "The Problem of Noun Incorporation in American Languages" (1911d).

2 "Noun Incorporation in American Languages" (1910b). This paper was written for the 16th International Congress of Americanists (Vienna, 1908). In it Kroeber lay down a challenge: "...proof of the existence of objective noun-incorporation in the verb of any American language remains to be made, and theoretical considerations impose the burden of proof on the upholder of the reported phenomenon...the general credence given to the phenomenon is based on a misconception" (575). In his 1911 reply, Sapir drew on data from Uto-Aztecan, Yana, Takelma, Iroquoian, Pawnee, Tsimshian, and other languages, to illustrate the prevalence of true noun-incorporation in North America.

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Geological Survey

Ottawa, Ont., November 20th, 1911

Dear Dr. Kroeber,-

As I understand that there may possibly in the near future be a vacancy in the scientific staff of the department of anthropology in your University, I should like to urge upon you the consideration of Dr. Paul Radin as one eminently qualified to fill that vacancy. Owing to circumstances, the details of which I am not familiar with, Dr. Radin has recently severed his connection with the Bureau of American Ethnology,<sup>1</sup> and is therefore in a position to take up research and lecture work elsewhere. Of the high quality of his ethnological work it is not necessary for me to dilate upon, for you have doubtless read his recent publication "The Ritual and Significance of the Winnebago Medicine Dance"<sup>2</sup> in which his mastery of the supplementary literature and powers of analysis are quite evident. You probably also know that Dr. Radin's manuscript material on all phases of Winnebago culture forms perhaps the largest and most valuable body of ethnological material in text form yet obtained from any tribe east of the Rockies. I can say very sincerely that I do not know of any of the younger Americanistic students that it would be more advantageous for you to have associated with you in your work. Aside from his excellent training and already accomplished work in ethnology, Dr. Radin is also well qualified to take up serious linguistic work of a high grade of excellence. As I know from personal contact with him, he has good preparation for this kind of work and has a far better than ordinary phonetic equipment. If, then, it is true that you will before long have a vacancy in your department, I hope that you will consider Dr. Radin.

With best wishes,

E. Sapir.

Ethnologist, Geological Survey of Canada.

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1 Radin had been accused of "mishandling accounts".

2 Radin 1911.

[73]

Geological Survey

Ottawa, Ont., November 27th, 1911

Dear Dr. Kroeber,

Thank you for your flattering letter of November 18th, also for your lists of Southern Yana material sent in later envelopes. I shall begin with a few comments on these.

S. Y. tatna "grizzly bear" doubtless has  $t^e$  as stem ending; in N. Y. "grizzly bear" has t'et<sup>e</sup>- as stem. When  $t^e$  is followed by n it becomes n<sup>e</sup> (n = voiceless n). It is acoustically rather difficult to keep apart  $t^e$  and n<sup>e</sup>. I should be very much interested in Rousselot tests for nasalization in this and similar cases. Have you yet tried Ishi on the machine?

C. Y. wáwi "house" should be wáwi. In this and similar words a often becomes colored to open o; I have recorded both wáwi and wówi. S. Y. wówi I suspect to be really wówi with short close o.

móhu, móyu "tobacco" should be móhu, móyu.

k'undjua- "to like" should be -u<sup>e</sup>a-. Glottal stops are often very weak in Yana but it is always highly important to note them, for they are never without etymological significance unless initial.

C. Y. bódja- "to throw": S. Y. bādja-. As you probably have discovered by this time,  $\delta$  becomes  $\bar{a}$  in passive forms (see Yana Texts, 26.3 and 28.12). Is S. Y. bādja- actually correspondent of bódja- or merely form with passive vocalism?

S. Y. -na in bana, auna, hana, kūmauna, is disturbing to me. I imagined that N. Y. and C. Y. -na were regularly replaced by -hi in S. Y. Have you determined when -na is used?

C. Y. djodjunau- "to give" should be djódunau-.

S. Y. māma- probably contains same stem as N. Y. mó-du-wau- "to hand over to". -ma- is probably indirective.

C. Y. go- "to hear" should be gó-.

-tc'it "by pressing" should probably be -tc'it<sup>e</sup>-. It is difficult to distinguish -p, -t, -k, from -p<sup>e</sup>, -t<sup>e</sup>, -k<sup>e</sup>, but highly important that this should be done. In C. Y. no syllable can end in p, t, or k; it may however end in l,

m, n, l<sup>ε</sup>, m<sup>ε</sup>, n<sup>ε</sup>, p<sup>ε</sup>, or t<sup>ε</sup>, whereas in N. Y. syllabically final m and n become respectively p and t. I suspect that all apparent cases of syllabically final p and t in S. Y. are really p<sup>ε</sup> and t<sup>ε</sup>. There are many apparent cases of syllabically final aspirated surds or fortis consonants, but these are always due to secondary loss of short vowel.

S. Y. -'ningu "to leave" should probably have m<sup>ε</sup>, which, I admit, is not easy to keep apart acoustically from simple m.

S. Y. -ai(?) "in fire" should doubtless be -<sup>ε</sup>ai-.

S. Y. -ni(?) "remote past" should doubtless be -<sup>ε</sup>ni-, provided that this element really occurs in S. Y.

S. Y. -anti- (not quotative) is new to me if t is to be interpreted as t'. As this element has been abstracted from recorded texts, I imagine that it is really -<sup>ε</sup>andi-, which, together with its post-vocalic form -adi-, you will find given in my list.

S. Y. -a'an-. I do not believe there is any such suffix. It is most likely to be -ma'au-. With S. Y. p'onma'auai "writes" compare C. Y. p'unma<sup>ε</sup>a- "to write" (I quote from memory and may not be quite accurate). (Have looked it up in my notes and find it is p'unma<sup>ε</sup>a-.)

I am glad that you are getting so much text material from Ishi. When I resume my Yana grammatical work and have collated all of my material, I shall perhaps have enough at my disposal to make it possible to satisfactorily analyze your new Yana material. When I get to that point I shall be glad to have you turn that material over to me. I hope you are having it typewritten without interlinear translation. With Waterman's present manuscript interlinear and my own material for supplementary analysis, I shall perhaps be able to make up grammatically accurate interlinears.

I have read your paper on Mohave phonetics<sup>1</sup> with great interest and have enjoyed it very much. Perhaps some day I shall get down to Rousselot experimentation myself. I have also read Waterman's paper on Northern Paiute phonetics<sup>2</sup> with interest but must confess that I was somewhat disappointed with it. It strikes me as being somewhat padded despite its brevity, and one can not help feeling moreover that his knowledge of Northern Paiute morphology is rather elementary. His statement (page 25): "Although the modified glottal stop is a very frequent occurrence in Paiute, it is doubtful if it ever has any etymological sig-

nificance" is very unfortunate, I venture to think. In Ute and Southern Paiute glottal stops are also at times extremely difficult to hear, but in every single case they have very definite morphological significance. I have never yet met any Indian language where glottal stops were morphologically negligible quantities, except initially in Takelma, Yana and Nootka. Southern Paiute initial vowels may be preceded by glottal stops; such vowels are always to be very carefully distinguished from initial vowels without preceding glottal stop. This difference is of influence on accent. I am at present working somewhat on Uto-Aztekan in which, I may as well confess, I believe. I think that I am on the trail of several important phonetic laws.

With best regards to Mrs. Kroeber and Waterman,

Yours sincerely,

E. Sapir.

P.S. It occurs to me that if you are somewhat disappointed in Sam Batwi as an interpreter, you might get someone else from around Montgomery Creek who might serve your purpose better. The brightest Indian I know of around there and who at the same time speaks the best English is Stonewall Jackson. He is a hunchback, very fidgety, and talks rapidly. The only trouble with him is that he is generally too busy telling you how busy he is. I was unable to secure him for my own work although I very much wished to. He speaks Central Yana, by the way, although living with Northern Yana and Pit River Indians. Others living around Montgomery Creek that might be of service to you are Canyon Bill and Dick Gere. The trouble with these is, however, that they are very independent. I was unable to secure the services of either of them, though Dick Gere had promised to work for me. Canyon Bill said he would not work for me for less than \$5 a word. P.P.S. I am rather sorry you and Waterman quoted<sup>3</sup> my Ute sketch as published in "Science",<sup>4</sup> for which I read no proof. It was so badly printed that I had Hodge send me proof before it was published in Anthropologist. It is in much better form in latter journal.

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1 Kroeber 1911a.

2 Waterman 1911.

3 In Kroeber 1911a and Waterman 1911.

4 Sapir 1910b.

[74]

Affiliated Colleges

San Francisco, Calif., Jan. 4, 1912

Dear Dr. Sapir:-

There is little new to report about Ishi, except that he has recently recovered from a month's seige of Pneumonia in the hospital and is not yet showing much disposition to learn English.

Our texts from him are at your disposal whenever you are ready to do anything with them. However, it will not be feasible to have them typewritten as Waterman's hand is so bad when he writes under pressure that a copy would certainly be replete with errors, even without counting the difficulties introduced by interlineations and corrections.

I am sorry that you do not think better of Waterman's Paiute paper,<sup>1</sup> as I am largely responsible for its shortcomings. I was compelled by circumstances to set him a practical problem whereas his interest in phonetics was and perhaps still is entirely general. You may remember the physician from Oregon who has collected good texts and grammatical information in northern Paiute.<sup>2</sup> For a number of years I have done what I could to encourage him, but as always, in such cases, the phonetics were a stumbling block. The only way of getting any light in this matter was to bring an Indian down and as I was otherwise occupied at the time, it fell to Waterman to do the work. He was therefore under the great disadvantage of working with a language with which he had had no previous experience by ear. When he first wrote his paper it practically consisted of what is now the second or general part. After he added the specific description, at my suggestion, the two halves did not fit in well together with the result that the paper may seem padded without actually being so. No doubt, he has not secured information on some of the finer points of the language, but so far as his paper goes I believe it is sound and would go on record in favor of what he has said. I am certain from my own experiences that Northern Paiute, which is Bannock, is quite different phonetically from Ute and southern Paiute.

The illustrations are unfortunate, partly because Waterman had had no previous experience with the engraver and partly because the printer botched the fairly tolerable plates which we had coaxed out of the difficult tracings.

I hope very much that you will soon be able to do something with your Yana Ethnology. As I remember your material is not very extensive and you got at least part of it in shape for the Putnam Volume.

You have started me thinking further about Incorporation and I am going to have another paper on the subject in the Anthropologist.<sup>3</sup>

Sincerely yours,

A. L. Kroeber

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- 1 Waterman 1911. See preceding letter.
  - 2 Dr. W. L. Maraden of Burns, Oregon.
  - 3 Kroeber 1911c.

[75]

Affiliated Colleges

San Francisco, Calif., December 16, 1912

Dear Dr. Sapir:

I have heard most favorable comment upon a paper which you are said to have published on the Chinese Language in the Popular Science Monthly. If you have a copy to spare I should greatly appreciate receiving it.

Our work with Ishi has come to a complete standstill, as we are now waiting for the time when he will know enough English to interpret better. He is learning very slowly, but I believe that in perhaps a year his translations may be of some use.

Is there any chance of your taking hold of him to work with, and if so under what conditions? I am afraid that at present shipping him to Ottawa would not be quite feasible, but in time it might be possible. How would you look upon a vacation of a month or two on the Pacific Coast?

Sincerely yours,

A. L. Kroeber

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Geological Survey

Ottawa, Ont., December 23rd, 1912

Dear Dr. Kroeber,-

I am flattered to learn that you have heard most favorable comment upon a paper of mine on the Chinese language. However, I regret to say that I have not written any such paper. I presume that the paper referred to was the one I wrote on "The History and Varieties of Human Speech," which appeared in the Popular Science Monthly.<sup>1</sup> You are doubtless familiar with that. The Chinese part of that paper consists of a sentence or two containing nothing startlingly original.

As to work with Ishi, it is rather embarrassing for me to know just what to do. I am so head over heels behindhand in writing up the results of back work that I dread committing myself to new responsibilities, although of course nothing could be more interesting to me than to work up Southern Yana. As a matter of fact, I do not believe I have ever worked on any language that I really enjoyed as much as Yana. I am expecting to leave for Vancouver Island some time in 1913, and it is just barely possible that I could obtain permission to spend a month or two in San Francisco on my return. I can not, however, promise definitely to do so. I presume all necessary extra expenses would be borne by the University of California.

You may be interested to know that I am expecting to read a paper entitled "Southern Paiute and Nahuatl, A Study in Uto-Aztekan," at the Cleveland meeting.<sup>2</sup> I believe I now have enough phonetic, morphological, and lexical evidence at my disposal to demonstrate the soundness of your claims beyond cavil.<sup>3</sup> I have even unearthed some morphological resemblances of detail which are so peculiar as to defy all interpretation on any assumption but that of genetic relationship. After the meeting I am thinking of preparing the formal paper for the Journal de la Société des Américanistes de Paris on this subject, and shall be glad to let you have a carbon copy.

With best regards to Mrs. Kroeber, and with best wishes for the New Year,

Yours very sincerely,

E. Sapir.



- 1 Sapir 1911c.
- 2 Annual meeting of the American Anthropological Association, December 30, 1912-January 2, 1913. The paper was published in two parts (Sapir 1913a and 1915a).
- 3 Powell and Henshaw, in the BAE classification of American Indian languages (Powell 1891), had denied--or regarded as unproven--the genetic relationship of Shoshonean, Piman, and Nahuatl, although the connection had been suggested by several scholars, including Brinton (1891). Kroeber had reasserted the unity of Uto-Aztecan (Brinton's term) in his study of California Shoshonean (1907a:154-163).

[77]

Hotel Statler, Cleveland  
Jan. 1, 1912 [read: 1913]

Dear Dr. Kroeber,

I have been instrumental in having a committee appointed at the Cleveland meeting of the A.A.A. which is intended to propose a new practical system for phonetic rendering of Indian languages. Chairman: Boas; other members: you, J. P. Harrington,<sup>1</sup> Goddard, and myself. On my way back to Ottawa via N.Y. I expect to consult with Boas and Goddard on this matter. Our system will, if accepted, become official (i.e., recommended) for A.A.A. and will doubtless be presented at Washington in 1914 (International Congress of Americanists). This system, which should be adequate, yet not cumbrous, would naturally try, where possible, to adhere to what may be considered general consistent usage in America, and should also aim, at least in part, to approximate current European usage. Aim: to further uniformity in American and to bridge over gulf separating American and European usages to some extent. Powell's system to be definitely abandoned. Please write Boas or myself what suggestions you have to offer.

Happy New Year!

Yours sincerely,

E. Sapir.

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1 John Peabody Harrington (1884-1961), at this time a research ethnologist with the School of American Archaeology in Santa Fe.--Harrington was raised in southern California, and from at least 1903 (when still an undergraduate at Stanford) he took a deep interest in the field recording of California Indian

languages. He spent 1905-6 in Germany, studying linguistics (chiefly phonetics), and after a brief stint as a high school teacher in Santa Ana (1906-9) settled down to a career of almost uninterrupted field research--after 1915 as a staff member of the Bureau of American Ethnology. His skill as a phonetician was unsurpassed, but he lacked scholarly discipline, published little, and had few friends in academic circles. He grew quite secretive about his work, and the extent and value of his notes came to be appreciated only after his death. See Stirling (1963) for biographical details, and Walsh (1976) and Mills (1981) for his work.

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Affiliated Colleges

San Francisco, Calif., January 3, 1913

Dear Dr. Sapir:

We should not be able to pay you any salary for coming here to work with Ishi, but I had in mind paying your expenses when I made the suggestion. We shall be so extremely short of funds for the six months remaining in our current fiscal year that I should not be able to arrange even this, but if your trip extends beyond July the outlook is more promising. At any rate I should probably know about April what we could do in summer or autumn. Nothing could suit me better than to bring about such a step.

I have lately become very much interested in Yana once more. You will remember that for two or three years past Dixon and I have been collecting lexical information on all the California dialects with a view to working out character and extent of borrowing between unrelated languages.<sup>1</sup> We were both convinced that until this information was available it would be impossible to furnish convincing proof of the relationship of Chimariko to Shasta<sup>2</sup> and of Costanoan to Miwok,<sup>3</sup> which we both believed in. You will recall being good enough to furnish me with a list of some 250 Yana terms for the purpose, which, by the way proved exceedingly convenient in the early stages of our acquaintance with Ishi. Our material and the tabulations based upon it were recently completed and in Dixon's absence in India I have been going over same pretty thoroughly. I could, however, get no intelligible result from our data until finally in desperation I dropped the assumption, under which we had all along been working,

that all these resemblances were due to accident or borrowing and assumed genetic relationship between those languages that had the greatest number of similarities. From this time on the skein unwound itself and when I turned to grammatical structure for confirmation it was lying at hand ready made in every instance. My wonder now is that we have overlooked the obvious so long and my only explanation is that we missed the clue and, comparing each language sometimes with the related and sometimes with unrelated ones, confounded genetic with induced similarities until we were in such a state of demoralization that we gave up the problem. I know this is exactly what happened to me on my first attempt in the same direction ten years ago.<sup>4</sup> I feel so confident of what we now have in hand that I am sending a brief statement to Science announcing that Wintun, Maidu, Miwok, Costanoan, and Yokuts all constitute only one family and that another family is made up of Shasta, Chimariko, Pomo, very probably Karok and--this will surprise you--possibly Yana.<sup>5</sup> In any event, whether they represent relationship or borrowing, it is clear that the affinities of Yana are all with this last family. On the basis of stems alone, I should have no very great hesitation in joining Yana with the others, but the structure still seems quite different. As we are, however, without available information as to Yana grammar beyond what I can recall from conversation with you and the list of suffixes that you were kind enough to send me about a year ago, I write to ask you whether you will favor me as soon as convenient with answers on the following points which may go far toward clearing up the true status of the language.

Is there any suffix indicating plurality of object or subject or, to put it more broadly, the number of either object or subject, which is added to the verb stem? This is a universal trait in all the languages of the Shasta group and I am inclined to think a more important one than has yet been recognized.

Am I right in gathering that Yana does not make the distinction between the inclusive and exclusive first person plural?

In your list you speak of a noun plural in -wi. My impression from hearing Ishi is that he does not distinguish between singular and plural nouns. Can you tell me something more as to the nature and extent of the usage of this suffix?

Do there appear to be any or many verbs that possess entirely different

stems in singular and plural? This seems to be a characteristic trait of the Shasta group of languages, but practically lacking in the Maidu-Yokuts group.

Does the normal verb stem in Yana appear to be monosyllabic or bisyllabic?

Is there true composition either of noun or of verb-stem with verb-stem?

Am I correct in gathering that Yana adjective stems are converted into real verbs merely by the addition of tense and pronominal endings belonging to verbs, and that the addition of other suffixes which make them attributive really converts them into nouns?

A few of these points are probably covered by the notes which you have already sent me, but if so I have either overlooked the fact or I am left in uncertainty.

Even if the suspected relationship breaks down for Yana, and I am very far from confident that it will hold, I think it is an interesting fact that the affinities of Yana, and therefore presumably its former contacts, are with the Shasta and Pomo group rather than with Wintun and Maidu, by which Yana territory in historical times has been almost surrounded.

Thanking you for any assistance you may be able to render Dixon and myself in the above matter, I am with best wishes,

Sincerely yours,

A. L. Kroeber

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1 See Letter 56, FN 1.

2 This was first suggested by Dixon (1910:337).

3 Kroeber's suggestion (1910c:259).

4 Kroeber undertook a survey of California languages in 1902, which resulted in a classification (reported in Dixon and Kroeber 1903) into three "morphological" groups: a northwestern group, a southwestern group, and a large central group of "typically Californian" languages. The latter--characterized by "a certain simplicity or transparency"--included not only the five languages later to be called Penutian, but also Pomo, Yuki, Esselen, and Shoshonean.

5 This announcement, "Relationship of the Indian Languages of California" (Dixon and Kroeber 1913a), was also printed in the American Anthropologist for Oct.-Dec. 1912 (which apparently did not reach subscribers until well into 1913). A fuller statement (Dixon and Kroeber 1913b) appeared in the American Anthropologist several months later. Kroeber also delivered a paper, "Lin-

guistic Evidence of the Prehistory of California", at a meeting of the Archaeological Institute of America, in San Francisco, April 10-12, 1913 (see AA 15:375-6, 1913).

[79]

Affiliated Colleges

San Francisco, Calif., January 6, 1913

Dear Dr. Sapir:

I shall be very glad to serve on the committee on orthography, if for no other reason than to serve as a brake on any extremists that may develop. I have long ago settled to the conviction that comparatively little could be done by legislation and that the greatest danger for progress lay in attempts to impose rules that were too hard. Specific conditions have changed a good deal since Powell's time, but practical considerations remain as important now as then. I should not be surprised for instance if you would have a good deal of trouble from Harrington. He is as keen and well informed on the subject as anyone in the country, but perhaps because he is a young man has shown a riotous inclination to indulge in the expressions of fine shades of sounds in the symbols used for them. Most institutions that do much printing in our field have by this time acquired a certain stock of type or linotype matrixes, and might be unwilling to invest in any new equipment of similar character, especially as they would not have assurance that any scheme now adopted would be reasonably permanent. You must realize how one law produces another.

As regards characters for consonants, I believe strongly in as few diacritical marks as possible, and to as close an adherence as possible to general modern European alphabets. In other words, to the current orthography of the world. For this reason I should be glad if the majority favored retaining x with its present value. Most laymen will continue to mispronounce it, but they would equally mispronounce any other symbol that might be chosen. As for our present t and tc, I think that the obvious way has been paved for us by Sanskrit and Indo-Germanic usage. We should also have single characters for ts and dz, but I do not know what to suggest. For the th and dh I have been satisfied with

Greek theta and delta. They are certainly preferable to Powell's symbols. Two things are my special aversion:

The inverted 3 or superior epsilon for the glottal stop and the mark of exclamation for the glottally affected stops. For the latter an apostrophe above or below the stop would perhaps be most appropriate, but this will make untold trouble not only in printing but in typewriting, which is coming to be more and more of a factor.

For vowels, I favor, so far as quality is involved, using diacritical marks as much as possible rather than the new creations of the French association, my reason being that vowels with diacritical marks occur to some extent in all fonts of type and are usually handled with ease on the typewriter. I long ago worked out with Goddard a scheme to distinguish quality and length by one accent, which has the advantage of simplicity, convenience, and appearance. It is:

Short and open, no diacritical mark.

Long and open, the grave accent.

Short and close, the breve.

Long and close, the macron.

The circumflex may seem more desirable than the grave in the above, but I have preferred the latter because the circumflex has been so much associated with length alone and with accent. For the same reason that two or more diacritical marks on one vowel are difficult and unsightly, I prefer retaining the accent where it has usually been placed, namely, after the vowel. Nasalized vowels will make another problem. In conclusion I should like to congratulate you on having had this committee appointed and on its personnel. The undertaking is a much needed and important one. I will take the liberty, however, of uttering the warning that if we attempt too much, our efforts will undoubtedly break down. For this reason it might be wise to weight our recommendations in different classes, or to make them gradually, beginning only with the most absolutely urgent.

Sincerely yours,

A. L. Kroeber

[80]

Affiliated Colleges

San Francisco, Calif., January 8, 1913

Dear Dr. Sapir:

I would like to add one suggestion to those I have already made about orthography. I believe it will not be feasible to restrict characters all to one font. This will mean falling back in some cases either on bold-face letters, italics, or small capitals. Of the three I feel strongly that precedence should be given the small capitals. They are least obtrusive and yet, barring x and o, more readily distinguishable than the italics. They occur in every font and on the board of every reputable linotype machine. Above all they are available as large capitals on every typewriter without the necessity of going back and underlining letters as for italics. I think this is a point of very great importance. I believe that more and more of our work will be done by Indians, particularly through the use of the typewriter. It is obvious how much easier it is to train an illiterate producer to strike a capital n which is already made for him on the keyboard and which the printer can receive instructions to treat throughout as a small capital than to confuse him by making him go back and underline the n every time he wishes to distinguish a surd from a sonant sound. Even apart from Indian informants, however, the consideration is an important one in the time it will save in copying and the much smaller likelihood of errors.

I am wondering whether a plan of action has been mapped out. Would it not be practical to make a preliminary collection of opinions on specific points from members of the committee and from such others as are interested on theoretical grounds, formulate or classify these and then take stock to see whether there are any principles or specific points on which there is practical unanimity. After this I would suggest the submission of these preliminary results for general discussion and a particular reference of them also to men who are interested in the problem primarily from the institutional or practical side, such as Hodge,<sup>1</sup> Putnam, and Dorsey. After this we could settle down to our real work.

Sincerely yours,

A. L. Kroeber

- 1 Frederick Webb Hodge (1864-1956), Ethnologist-in-Charge of the Bureau of American Ethnology from 1910 to 1917. An archaeologist, Hodge was associated with the BAE from 1886 to 1917, and later worked at the Museum of the American Indian and the Southwest Museum. From 1902 to 1914 he was also Editor of the American Anthropologist. See Cole 1957.

[81]

Geological Survey

Ottawa, Ontario, January 15, 1913

Dear Dr. Kroeber,

I have just received your letter of January 8th, and will take this opportunity to answer this and your letters of January 3rd and 6th. First, as to your query of January 3rd in regard to whether I know of anyone who would be likely to be of service to you as instructor in your department I believe that you could get no more excellent man than Goldenweiser, but feel absolutely certain that \$1200 a year would not interest him, if for no other reason than that, as I understand it, it is almost certain that he will be made instructor of anthropology in Columbia next year. There are other reasons of a somewhat peculiar nature which make it particularly difficult for him to leave New York. He has told me, however, that if he were ever offered a really attractive position elsewhere, he might make shift to do so. He has done two summers' field work for us in Iroquois and I know from conversations that I have had with him as well as from correspondence, that he is going into the whole project in a very thorough manner. Now that he has had considerable experience in field work, I believe the purely theoretical interest that he started off with has been considerably sobered, so that, taking all in all, I hardly know of anyone who could be considered a more excellent ethnologist in America, that is among the younger men. I believe he also makes an excellent lecturer, having a very decided feeling for both logical arrangement and outward form. If you really decide to try to secure his services, I think you may have to offer him as much as \$2000 a year, but I think it should be well worth the University's while if it could only be managed. As far as I am personally concerned, I should almost rather not have him go out West, as I am very eager to see him continue undisturbed on his



Iroquois work, knowing from personal experience how difficult it is to keep in touch with back work.

In quite another class is Mechling. His type of mind is more that of the average American of considerable enthusiasm but relatively little theoretical grounding. He was one of my students in Philadelphia and seemed to show some ability in practical linguistic matters. He has done two seasons of field work for us among the Malecite Indians of New Brunswick and though I have not had a chance to see the results as yet, I believe he has obtained much of interest and value. Inasmuch as you wish me to be very frank, I must say that Mechling has one or two defects which might well be taken into consideration in considering him as an applicant for a position. In the first place, he has rather little feeling for form and is apt to do things in a somewhat slipshod manner. A second defect, judging at least by what I have been able to learn from Boas, is an unwillingness to work harder than circumstances absolutely warrant, though to be fair one must hasten to add that he was ill during most of his time in Mexico. To put it in a nut shell, I should say that Mechling has real ability and enthusiasm, but that these qualities are somewhat hampered by temperamental defects. Please do not exaggerate the significance of these statements, as I have purposely gone out of my way to be far more frank than one generally is in communications of this nature. As far as research ability is concerned I should suppose that he was about in Waterman's class, perhaps better. As for teaching ability, I should doubt whether he was as good.

Very superior to Mechling for a teaching position would be W. D. Wallis,<sup>1</sup> who is now Instructor in Anthropology in the University of Pennsylvania. He has had three years of formal anthropological work at Oxford under Marrett, Balfour, and Thompson (physical anthropology). He has had two seasons of field work among the Micmac. You see from these statements that he has had excellent preliminary training, both academic and as regards field work. I may add that he was a Rhodes Scholar during his three years work in Oxford. My colleague, Mr. Barbeau, is a personal friend of his, and thinks very highly of his ability. I have had relatively little opportunity to learn much of Wallis from personal intercourse, but have so much faith in Mr. Barbeau's judgment that I feel certain he would be an excellent man for the position. Wallis has published rather little as yet, but what he has gives clear evidence, it seems to me, of a grasp of

anthropological work, an interest that should perhaps be more clearly developed than it is as yet in America. I may add that I have reason to believe that Wallis might look favorably upon a proposal coming from you. I should say, off hand, that he would be the most likely man you could find to prove satisfactory for the position.

Your data in regard to Californian linguistics are, of course, extremely interesting to me. Let us for the present leave quite open the possibility of my returning via San Francisco and working with Ishi, as I should not be able to do anything in the matter until well in the summer or autumn. Your new syntheses of linguistic stocks in California are rather exciting, but I see no theoretical reason why they should not be sound. Everything depends, of course, on the matter of your evidence, the presentation of which I am looking forward to with very great interest. As for Yana, I should not have been disposed myself to believe that it is in any way related to Chimariko, Pomo, or Karok, judging from the small amount of material that has been published on these languages. One striking difference between Pomo and Yana is the absolute lack of instrumental prefixes in the latter, Pomo and Chimariko in this respect agreeing with Shoshonean. In fact there is not a single prefix in Yana, a point that will probably have to be considered. In answer to your queries in regard to Yana, I submit the following data:-

1. There is no regular suffix indicating plurality of object or subject. -ka is very frequently used to denote "all" or "completely", but this element can hardly be labelled a pluralizing one. However, there are a fair number of words that have plural stems related to, yet different somewhat in their actual forms, from the singular stems. A dental element is generally characteristic of the plural stem. Thus, from samsi- "to sleep" is formed sādimet- "several sleep"; from k!a'wi- "to be tired" is formed k!adjawmi- "several are tired". Other verbs form the plural stem by inserting an a- element, which may cause a secondary i to be added. Thus from djal- "to laugh" is formed djāli- "several laugh"; from djul- "to be long" is formed djauli- "several are long".
2. There is no difference between inclusive and exclusive 1st plural.
3. There is no regular noun plural. However, certain noun stems may take on a suffix -wi which has a pluralic, more properly collective, force.

Thus, from i- "tree, stick" is formed iwi "wood, firewood". Similarly, from dal- "hand" is formed daluwi "hand and fingers". Quite a number of nouns form a plural in -wi, at the same time inserting t' (not t!) in the stem, the vocalic form of which is sometimes thereby slightly modified. Thus, from dāt'i "child" is formed dat'iwi "children". Another example is bambamūt'giwi "fly", a word which is used only in the plural, or rather collective, form.

4. There are some verbs whose stems are entirely different in the singular and plural, but I should say off hand that there are less of these than of such as have different but related stems in singular and plural. Examples are: -ni- "man walks" and 'a- "woman walks", as compared with 'an- "several walk"; 'ī- "to move along" as compared with p'ī "several move along"; ba- "to move quickly, rush" as compared with cu....'a- "several rush".
5. I should say that most verb stems in Yana are monosyllabic. They may be classified as to whether they end in a short vowel, long vowel or diphthong, vowel followed by l, m, or n, or vowel followed by p' or t'. Stems that do not at first blush seem to fit into any one of these types have generally undergone certain secondary changes which can be ascertained.
6. Noun stems can be freely compounded. Independent verb stems are never compounded.
7. There are no adjective stems. All adjective stems are per se verb stems unless, like any other verb stems, they are converted into participles by suffixed mau(na). In fact there are only two parts of speech in Yana, nouns and verbs, outside of an inconspicuous number of particles.

As I have already taken up so much of your time, I shall not go into details of phonetic orthography. I wish to thank you for your comments of January 6th and 8th, which will certainly be given due attention at the next meeting of the quorum. Boas, Goddard, and myself have had two meetings in New York recently and have discussed various matters in a preliminary way. As secretary of the committee I shall, before very long, draw up a connected account of what has so far been done and shall send you and Harrington copies. This will serve as a more

definite basis for further comments. I hope that both you and Harrington will be able to reply almost immediately, so that new points could be taken up at the next meeting, which I believe is to be held in March.

With best wishes,

Yours very sincerely,

E. Sapir.

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1 Wilson D. Wallis (1886-1970). Although circumstances prevented Kroeber from taking on anyone new in 1913-14 (see Letter 101 below) he was impressed by Wallis and arranged for him to teach at Berkeley in 1915-16, Kroeber's sabbatical year. A permanent position did not open up for him at Berkeley, however, and after a few years of teaching in high schools and small colleges in California, Wallis finally joined Albert Jenks in setting up an anthropology program at the University of Minnesota, where he spent the remainder of his long career.

[82]

Affiliated Colleges

San Francisco, Calif., January 21, 1913

Dear Dr. Sapir:

Many thanks for your letter of January 15, especially the information you give as to the several men. I much appreciated what you told me and shall regard it as strictly confidential.

I also appreciate your information about Yana. Every point you mention either shows a positive resemblance to the structure of the Shasta group or at least a negative agreement. The lack of prefixes is what has all along bothered me most. This seems, however, not to be an insuperable obstacle. Maidu has developed instrumental prefixes which are entirely lacking in the other languages of the group to which it belongs and yet if there is anything at all in our present point of view there can be no question of the connection of Maidu with the four other stocks. Moreover there are enough striking instances in both Uto-Aztekan and Algonkin of the transfer of certain elements from prefixes to suffixes or vice versa, to make a difference of this sort not insuperable. I have a feeling, however, that the spirit of Yana grammar is quite different from that

of the other languages of the northern group and while, in the scarcity of available information, this feeling may be little more than blind intuition which will be dispelled as soon as we know more, it is strong enough to make me go slowly.

Sincerely yours,

A. L. Kroeber

[83]

Geological Survey

Ottawa, Ontario, January 27th, 1913

Dear Dr. Kroeber,-

In reply to your letter of January 21st, I may say that I am interested in your statement regarding the Shasta-like character of Yana. I do not know if the lack of prefixes in Yana is an insuperable obstacle, but it certainly suggests a fundamental difference of grammatical structure between it and Chimariko, for instance. I do not believe your analogy of Uto-Aztekan and Algonkian is a good one. In these two linguistic stocks the elements which vary between prefixed and suffixed positions are not derivational in character but are pronominal elements which do not enter into as close combination with the verb or noun stem as would instrumental prefixes, for example. The pronominal suffixes of Ute and Paiute, for instance, I do not consider as suffixes, properly speaking, but as enclitic elements, or at most as enclitic elements which have a tendency to be treated like suffixes. The fact that they may attach themselves to almost any word in the sentence, regardless of whether they inherently belong to that word or not, indicates that they are hardly suffixes in the true sense of the word. Conversely I have somewhat of a feeling that the pronominal prefixes of Nahuatl, perhaps of most Shoshonean dialects also, are hardly more than proclitic elements, hardly true prefixes in the sense in which pronominal elements are prefixes in Iroquoian or Chinookan, for instance. This point of view may make it seem less strange why pronominal elements should be prefixes in some Uto-Aztekan dialects and suffixes in others. I imagine that such a phenomenon as instrumental prefixes in Maidu might develop as a special form of

noun-verb or verb-verb compounds under the influence of Pomo or other non-related language possessing instrumental prefixes, but perhaps such compounding would be hardly possible in Maidu and its group.

Yours sincerely,

E. Sapir.

[84]

Affiliated Colleges

San Francisco, Calif., February 4, 1913

Dear Dr. Sapir:

Referring once more to your letter of January 15, I would say that other advises have reached me which tally in every respect with what you say about Goldenweiser and Mechling. If you can add anything about Wallis or refer me to someone who is in a position to speak more intimately of him, I should appreciate it. I know of him chiefly through his reviews, which show an unusual grasp. From what you say I judge that he is an American with English training and a young man, but I am absolutely ignorant of his history, circumstances, and personality.

I still agree with you that there seems to be a fundamental structural difference between Yana and the languages of the Shasta group. I believe I attached fully as much weight to the matter of suffixes versus prefixes as you do. I am also ready to admit that Algonkian and Uto-Aztekan are only partial parallels. At the same time their analogy is not entirely without point and so far as relationship of Yana to the other languages goes, I am not yet ready to give this up. I unquestionably can not prove it, but so far structural similarities have so regularly followed in the course of lexical resemblances that I will refuse to disbelieve in the connection between Yana and Shasta until it is positively disproved.

I appreciate also the force of your keen remark as to the Maidu prefix. Dixon makes it quite clear that at least some of them are merely the first elements of compound verbs.<sup>1</sup> The difficulty that remains therefore is not the prefix but, as you correctly observe, the existence of the compound. When we know

more about the original structure of the group and about the transition from a synthetic to an analytic type, which I suspect it has undergone, such compounds may, however, prove to be a feature retained by Maidu. If this is not the proper explanation, I can see nothing for it but to take the bull by the horns and admit that Maidu has developed a type of compound entirely new to the family at large. I would rather make this assumption than to throw out of court all the accumulative evidence relating Maidu to the family.

While I see that I must be more careful with you than with most people in citing analogies, I will take a chance at another in this connection. While I know nothing very definite about Indo-European philology, am I not correct in believing that Latin, for instance, has lost nearly entirely the compounding characteristics of Greek and Sanskrit? Is there not also a pretty fundamental reversal of method in the habit of Romanic languages of putting the qualifying element last in noun compounds? I will admit that even if you affirm these questions there is no evidence of any such radical change in Indo-European as the development in any one language of an entirely new type of composition. But do not these languages provide at least partial precedent for my situation?

Sincerely yours,

A. L. Kroeber

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1 Dixon 1911: 694-699.

[85]

Geological Survey

Ottawa, Ontario, February 11th, 1913

Dear Dr. Kroeber,-

In reply to your letter of February 4th I may say that the best men, as far as I know, to turn to for more explicit information in regard to Mr. Wallis would be Mr. Barbeau of this Survey, who was a fellow student of his at Oxford, and his instructor in anthropology, Mr. R. R. Marrett, of Exeter College, Oxford. As to Mr. Wallis' personality, it is quite clear that he is rather modest and unassuming in his personal demeanor and certain to make himself liked

by both colleagues and students.

I have recently read your note in "Science" in regard to linguistic developments in California.<sup>1</sup> I hope you do not have the idea that I am personally opposed to such syntheses. In fact, I feel strongly that there will be more of them made as our knowledge progresses. Everything naturally depends on the specific evidence. As regards parallels, I may say that I am not personally inclined to lay much stress on them one way or another, except as merely suggesting possibilities. The actual historical process must be worked out independently on the evidence supplied by the particular problem. Parallels never constitute evidence. They merely predispose one in favor of or prejudice him against accepting specific evidence as convincing. Please do not imagine that I think that radical changes in grammatical form may not, in course of time, be brought about within a linguistic stock. As you point out, it is true that the ability of Greek and Sanskrit to form noun-compounds freely has been considerably weakened, if not entirely lost, in Latin. There are plenty of such developments in Indogermanic that, on general principles, would seem quite revolutionary. They are generally brought about by inconspicuous-looking phonetic processes of one kind or another, which disturb old conditions and bring about regroupings. Thus, in Old Irish we have a series of enclitic pronominal elements, quite distinct either from independent personal pronouns or pronominal suffixes in verbs, which come between verbal prefixes and verb stems. They are thus practically buried in the verb form and are indeed known by Celtic students as infixes, though they are, of course, not infixes in the most legitimate and narrow use of that term. In modern Irish the formation of the past tense is regularly associated with a set of changes of the initial consonant of the verb stem. This surprising state of affairs, which sounds distinctly non-Indogermanic at first blush, is quite simply due, at last analysis, to the operation of regular phonetic law whereby a consonant standing immediately after a preceding vowel became spirantized. Arguments of possible developments on general principles never lead very far, because, if we assume enough time and favoring conditions, almost any change, no matter how apparently radical, may be plausibly shown to be possible. All that I personally feel like insisting on is that the evidence itself be such as to force one to adopt such and such conclusions. If you could spare the time, I should be very thankful if you could let me have at



least that part of the evidence which relates to Yana. Please do not imagine that I have any personal reason for preferring not to have Yana demonstrated as genetically connected with Shasta. In fact, I should be delighted if the total number of linguistic stocks in California should turn out to be very few in number, after all.

Yours sincerely,

E. Sapir.

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1 Dixon and Kroeber's announcement of the new California linguistic groups (1913a) appeared in the issue of Science dated February 7, 1913.

[86]

Affiliated Colleges

San Francisco, Calif., February 18, 1913

Dear Dr. Sapir:

I agree with you entirely as to parallels. They are nothing but analogies and analogies are never proof. I will be glad to comply with your request to send you the evidence relating to Yana, although in this one case it is almost entirely lexical. It may be some days before I find time to copy it out. In this connection I will only say what I have told you before, that the resemblances between any two individual languages are never so convincing as those which appear when an entire group is examined. This in fact is the crux of the whole matter. I have been comparing Yokuts and Maidu for a long time, but it was not until I drew in Wintun, Miwok, and Costanoan that the results commenced to be conclusive. I hope you will bear this in mind when examining the rather incomplete material which I shall send you. If by the way you can add any similarities that will strengthen our case I shall be very much obliged to you.

Sincerely yours,

A. L. Kroeber

[87]

Geological Survey

Ottawa, Ontario, February 27th, 1913

Dear Dr. Kroeber,-

Thank you for your linguistic material on Yana, which I have looked over with interest and of which I have had a copy made. Enclosed you will find returned your manuscript.<sup>1</sup>

Your material is certainly suggestive, but I cannot feel that I have any right to adopt a definite stand in the matter until I know far more about Shastan morphology than I do. As you may remember, I pointed out in my review of Dixon's Chimariko paper<sup>2</sup> that it is difficult to know how to weight lexical correspondences without a definite knowledge of grammatical features as well. A few of your examples certainly seem best explained as local borrowings. This seems emphatically so for instance of "buckeye". Some of your examples I should be inclined to erase without much hesitation. These are "tongue", "sun", "fire", "smoke", "horns", "tail", "good", "two", and "drink". Other examples, on the other hand, such as "man", "woman", "heart", "shaman", "arrow", "water", "stone", "louse", and "sleep" seem quite suggestive and, taken all together, can certainly not be explained on the basis of mere accident. Whether or not borrowing can explain these resemblances, it is of course impossible to say at present, at least for me, who am not as well acquainted with central Californian morphology, outside of Yana, as yourself. You will find one or two comments added to your manuscript that may be of interest.

I am sorry that the phonetics of the material that you are dealing with leaves so much to be desired. You must realize, sooner or later, that exact phonetic material is indispensable for any solid comparative work. Once definite phonetic relationships have been worked out, other examples, that at first sight would have seemed far-fetched, fall right into one's hands. To be sure, some examples which, at first sight, seem plausible enough have later to be discarded. To stick for the moment to the material in hand, I rather think that some of your examples would have seemed even stronger had the phonetics of all the languages been as accurate as desirable. Thus, under "stone" you give for Chimariko qa'a. Now, I happen to know from the study of Dixon's Yana

manuscript that one of his most constant personal errors is to hear glottally affected k as velar k (q), so that his qa'a is likely as not kla'a, which brings it a notch nearer to Yana k!ai-. And here let me warn you against Dixon's phonetics. They are indeed deplorable. His Yana manuscript shows clearly that he has entirely confused no less than five distinct series of stopped consonants. His t, for instance, may mean either intermediate t (what I have been writing d), aspirated t, glottally affected t (t'), long t (Northern Yana td developed from nd), and t followed by glottal stop (t'). Just as little reliance is to be placed in Dixon's record of quantities. In fact, his whole work is amateurish to a degree. When I read his Maidu sketch<sup>3</sup> I thought it was the last straw. If anything, he is supposed to know Maidu, but I think it quite clear from internal evidence that his knowledge of that language is quite elementary. He simply can not hear well, and the more frankly one recognizes the fact the better.

Yours sincerely,

E. Sapir.

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1 It is not known whether this manuscript survives. Kroeber included very little lexical data in the Hokan section of the paper "New Linguistic Families in California" published later in 1913 (Dixon and Kroeber 1913:651-3), and in the longer study published several years later (Dixon and Kroeber 1919) the substantiation of Hokan largely is left to rest on Sapir's work (particularly Sapir 1917a).

2 Sapir 1911a, a review of Dixon 1910.

3 Dixon 1911.

[88]

Affiliated Colleges

San Francisco, Calif., March 6, 1913

Dear Dr. Sapir:

Thanks for your comments on my Yana list. They will be helpful. I did not expect the list to convince you thoroughly. You will realize as clearly as I that we cannot be finally judged until our evidence is presented. I am glad

that you agree however that we already have enough of a showing to make out a case. I am more sorry than you that our phonetics leave so much to be desired, as I realize the handicap under which it places us. I believe that I am sufficiently aware of my own deficiencies in this regard, but have hopes that I have heard enough of most of the languages in California to prevent me from falling into very many gross errors. The deficiencies you point out in particular have long troubled me. A very cursory examination of material, even without any knowledge of the language which it represents, is enough to reveal the situation. However, we must do the best we can. We have anyway not yet got to the point of writing a comparative analysis of any new family. Even if the quality of our material will prevent an unhesitating acceptance of our conclusions by the most critical, I am confident that we will come near enough to establishing our points to give work in the California field a new direction, and make it possible for those who are technically better equipped to definitely prove or disprove what we believe. As to what their final verdict will be, I have not the slightest hesitation.

Sincerely yours,

A. L. Kroeber

[89]

Geological Survey

Ottawa, Ontario, March 12, 1913

Dear Dr. Kroeber,-

I have your letter of March 6th.

Quite some time ago I sent you a detailed report of the preliminary meetings of the phonetic committee at New York.<sup>1</sup> I solicited comments. You have not once referred to this matter, so I am beginning to wonder whether you ever received the report. Please let me know.

Yours sincerely,

E. Sapir.

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1 See Appendix I (A).

[90]

Affiliated Colleges

San Francisco, Calif., March 13, 1913

Dear Dr. Sapir:

Your report of the meeting of the Phonetics Committee got among my printed mail and has been overlooked until now. I am much pleased that you have seriously got to work so soon and will write you fully in reply to your report next week.

I expect that before the month is out Harrington will be in San Francisco and that we may be able to send you our joint recommendations.

Sincerely yours,

A. L. Kroeber

[91]

Affiliated Colleges

San Francisco, Calif., March 18, 1913

Dear Dr. Sapir:

Herewith my opinions on the preliminary recommendations of the phonetic committee....<sup>1</sup>

Sincerely yours,

A. L. Kroeber

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<sup>1</sup> See Appendix I (B).

[92]

Geological Survey

Ottawa, Ontario, March 20, 1913

Dear Dr. Kroeber,-

I am much relieved to learn that you got my report on phonetics after all,

and I shall be very glad to get your and Harrington's recommendations at an early opportunity.

Yours sincerely,

E. Sapir.

P.S. Note following addition to anthropometric material on hand:--

March 14th, 8 lbs. 1 oz. of infant humanity (male) presented to me by Mrs. Sapir.<sup>1</sup>

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1 H. Michael Sapir, the Sapirs' first child.

[93]

Geological Survey

Ottawa, Ontario, March 27th, 1913

Dear Dr. Kroeber,-

Thank you very much for your reply of March 18th to my phonetic report. It seems that you may have misunderstood me on one point. I did not by any means intend my report to be a finished document, to be printed as it stands. Had that been my purpose in writing it, I would have, of course, taken pains to express myself more concisely and definitely. It was intended merely to air various opinions in order to solicit definite responses. It was purposely conservative and undecided in tone, something of course which a final formulation must not be. I am having carbon copies made of your reply and shall have them forwarded to Harrington, Boas, and Goddard, so that all may know how opinions stand at present. When I hear from Harrington, I shall adopt a similar procedure. Perhaps the best plan after that will be for me to formulate a definite scheme on the basis of all that has been said, and giving preference, merely by way of starting the ball rolling, to my own opinions, except where they are obviously entirely unsupported. I could then send around a copy of this to all concerned, who could indicate in each case whether they accepted it, or, if not, what they suggested in its place, perhaps also what would serve them second best.

I have read your reply over very carefully. On many points I thoroughly

agree with you, and I see that in regard to some of them we are more in accord than Boas, Goddard and I were. On others, I am compelled to disagree with you. In one or two cases, I think your facts are wrong. By way of getting down to something specific, I should like to take up your replies in order with what seem to me appropriate comments....<sup>1</sup>

Please let me know whether you are willing to give way on the points that I disagree with you about. If at least we two can come to an agreement on practically everything, it ought to help in getting matters fixed up. I think that Boas is very fair-minded in this whole matter and not at all liable to insist on personal likes or dislikes. From Harrington, I have not yet heard.

Yours sincerely,

E. Sapir.

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<sup>1</sup> See Appendix I (C).

[94]

[San Francisco]

4/12/13

Dear Dr. Sapir,

Compare Esselen -nex, -nax (A.A.E. II 63)<sup>1</sup> with Yana -na

water	ha-na	asa-nax
fire	au-na	a-nix
bow	man'i	payuc-(n)ax

etc.

Sincerely,

A. L. Kroeber

We are having a really good meeting.<sup>2</sup>

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<sup>1</sup> Kroeber 1904:63.

<sup>2</sup> The Archaeological Institute of America met in Berkeley, April 10-12, 1913.

[95]

Affiliated Colleges

San Francisco, Calif., April 22, 1913

Dear Dr. Sapir:

Mr. Harrington and I have had a good deal of talk about phonetic orthography in the last month while he has been here with us.<sup>1</sup> We both feel that the Committee is attempting to do too much. We do not believe that any orthography which might be passed by a vote of three to two in the committee, or which was essentially a compromise satisfactory to no one, would carry much weight, particularly as members of the committee would probably be the first ones to depart from the standard set up. I would therefore like to suggest as a guiding principle an understanding that only such points be included in our report as would pass the approval of the committee with unanimity and to which all our members would agree to conform in their own work, except where positively prevented by definite practical considerations. We probably should have very little to report if we adopted this policy, but what little there was would at least stand a chance of being permanent. Harrington is in full accord with me on this point and I very much wish you would submit it to the members of the committee. I am beginning to feel that if we continue a discussion of detailed points as heretofore we shall either drift apart without reaching any conclusions, or will construct a patch work which each of us individually will abandon at the first moment. As I said in my first letter to you,<sup>2</sup> I am of the opinion that the slower we go the more we shall ultimately accomplish.

Sincerely yours,

A. L. Kroeber

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1 Harrington was in San Francisco and Berkeley during April, 1913, to attend the meeting of the Archaeological Institute and to do archival research in the Bancroft Library.

2 Letter 79 (January 6, 1913).



[96]

Geological Survey

Ottawa, Ontario, April 28th, 1913

Dear Dr. Kroeber,-

I have your letter of April 22nd, in which you refer to talks about phonetic orthography with Harrington. I am having copies of this letter sent to both Boas and Goddard, in order that they may know exactly how sentiment is going in these matters. Enclosed I am sending you a copy of a letter I received today from Harrington.<sup>1</sup> You are doubtless already acquainted with its contents, but I thought it would be a good idea for you to have Harrington's opinions on file. I am somewhat afraid that Harrington is more interested in exhibiting his wide acquaintance with phonetic matters than in getting down to business. I agree with you, on the whole, but I think that if we are too timid about recommending things, we subserve no very definite purpose. I have personally never been much worried by multiplicity of orthographies. The one thing that has irritated me is the persistent failure of many linguists to explain their phonetic symbols.

Yours very sincerely,

E. Sapir.

P.S. Kindly let me know whether you wish to have Harrington's motion, calling for unanimity on the part of the members of the committee, sustained. Personally I have nothing against it.

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<sup>1</sup> See Appendix I (D).

[97]

Ottawa, Ontario, April 30, 1913

Dear Dr. Kroeber,

Are you acquainted with:-- Pit River ōp' "tobacco",  
 Takelma ō<sup>u</sup>p' dit., Diegueno up dit.

E. Sapir

[98]

## Affiliated Colleges

San Francisco, Calif., May 8, 1913

Dear Dr. Sapir:

You will recall what I said some months ago about "dangerous extremists". I find Harrington very hard to fathom. His general attitude is extremely broad and reasonable, but he seems to completely contradict it at times by most surprising individual recommendations. I am, however, heartily in favor of his suggestion that the Committee recommend, at least to begin with, only such things as pass it unanimously. Please consider me as formally seconding his motion. If this should prevail, we can then hope to get down to something definite as a first step very shortly.

Your Takelma resemblance is interesting even though it relates to tobacco, for which we have clear cases of loan words in California, as might be anticipated.<sup>1</sup> I am confident, however, that the scope of our work will ultimately extend far outside of California. The Oregon field is particularly promising.

If Gatschet's material<sup>2</sup> was in better shape I think we might do something to relate Lutuami definitely to one of our California families. The Hokan group in particular lends itself to the supposition of being widespread. In spite of J. N. B. Hewitt, I have always believed Seri to be Yuman.<sup>3</sup> You will also remember Brinton's connection of a South Mexican language with the same family.<sup>4</sup> However uncertain these indications may be, I should be surprised if our Hokan group did not prove ultimately to be the nucleus of a very large stock.

Sincerely yours,

A. L. Kroeber

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1 Words for "tobacco" are widely diffused in North America. For the forms in Letter 97 see Dixon (1912:30).

2 Albert S. Gatschet (1832-1907), working for the Bureau of American Ethnology, collected extensive data on Klamath-Modoc ("Lutuamian" in the BAE nomenclature) in the 1870's. His major publication on the language (Gatschet 1890)

is a vast, disorganized work, encompassing grammar, lexicon, texts, and ethnography.--Gatschet, a Swiss German by birth, studied in Bern and Berlin before emigrating to the U.S. in 1868. He was a language teacher in New York for several years, but soon became interested in American Indian linguistic research. In 1877 Powell offered Gatschet a position as Ethnologist--in fact, field linguist--with the Geological Survey, and in 1879 he became one of the original members of the BAE staff.

- 3 Powell and Henshaw (Powell 1891) classified Seri as a Yuman language, apparently on the advice of Gatschet. This was also Brinton's opinion (1891: 335). A study of the question of Hewitt (1898), however, threw doubt on the validity of grouping either Seri or Waikuri with Yuman. The separation was accepted by Gatschet, and became the official BAE position (Langdon 1974:15-17). For Kroeber's subsequent work on the Seri-Yuman connection see Letter 127 ff.--John Napoleon Brinton Hewitt (1859-1937), of Tuscarora descent, joined the BAE in 1886 and remained on its staff to the end of his life. He worked chiefly on Iroquoian ethnology and linguistics, but in the 1890's he was asked by Powell to investigate two or three dubious genetic groupings--most significantly, Sahaptian with Klamath-Modoc ("Lutuamian"), and Seri-Waikuti with Yuman. He decided in favor of the first (though his results were not published, nor reflected in BAE policy) and against the second. See Swanton 1938.
- 4 Brinton (1891:147-8) had pointed out resemblances between Yuman and Chontal de Oaxaca (Tequistlatecan).--Daniel Garrison Brinton (1837-1899), a physician by training, was one of the pioneers in academic anthropology in the U.S., holding the first professorship in the subject in the country (at the University of Pennsylvania). His particular interest was American Indian linguistics, and his most important general statement, The American Race (1891), is largely an attempt at a hemisphere-wide classification of languages. For details, and an assessment, see Darnell (1967; 1970:84-86).

[99]

Ottawa, Ont.

May 9, 1913

Dear Dr. Kroeber,

"in" or "into" is -wul as verb suffix, as independent adverb (really noun) it is iwúlu "inside". "Out" or "out of" is -dam, -ram as verb suffix (in many cases -dāmi, -rāmi; as independent adverb (really noun) it is irāmi.<sup>1</sup>

Yours,

E. Sapir.

- 1 These are Yana forms. Sapir was apparently replying to a request for information on how to express "in" and "out" to Ishi. See Letter 101.

[100]

Ottawa, Ont.

May 15, 1913

Dear Dr. Kroeber,

Some time ago you wrote me asking my opinion of Mechling and Goldenweiser as possible candidates for a teaching position in California. You said that Mason and Radin were out of the question, so that in my reply I made no further reference to either of them. Should you, for some reason or another, fail to secure Mr. Wallis' services, I should like, in spite of your elimination, to say a word for Radin. Where you got the impression that he could not be considered, I do not know, but I venture to think that it is quite unfounded. If from Waterman, I may say, between ourselves, that I have good reason to believe that he is prejudiced against Radin. It is true that Radin, as I have been informed, might not make a brilliant lecturer, but he has no speech impediment like Mason so can hardly be classed with him on that score. As a matter of fact, however, I see no reason whatever why Radin, like plenty of others who are not orators by temperament, should not make an entirely satisfactory lecturer. As regards knowledge of anthropology, particularly American, Radin is head and shoulders above Mechling, Mason, and I suspect, Wallis. His descriptive knowledge is doubtless better and more detailed than that of Goldenweiser. For field work there is no better man to secure abundance of valuable data (whether on ethnology or linguistics) than Radin. He gets tremendous amounts of text material, hears well, and investigates problems of social organization, religion, and mythology thoroughly. On technology, I believe, he is weaker. He has had plenty of experience. His Winnebago work you probably know of. The Bureau is getting out an annual report on his Winnebago material (general survey),<sup>1</sup> which is but the merest fraction of his total Winnebago material, as I understand. For us he has done work on Ojibwa linguistics and ethnology (chiefly religion, social organization, and mythology). I have seen some of this and

know that it is good stuff. Then in Mexico he did a lot of work on Huave and Zapotecan. Over and above his anthropologic attainments, Radin is unusually well read on almost all conceivable topics (he is what one would call a "shark"), and is a man of culture. Him and Goldenweiser I frankly consider the cream of the younger anthropologists in this country. Some people, let me frankly admit, dislike him because of a certain irresponsibility (or naiveté) of demeanor. We (my wife and I) have always found him a most delightful friend. I may say that Boas was at first rather down on him, but has modified his estimate of Radin completely, as far as I know. Take my word for it--he is excellent.

I have been planning to get him for our Survey. It has been impossible to "swing" this for the present fiscal year (Apr. 1, 1913-March 31, 1914), though I have estimated for further field work for him. I am hoping to get him attached, if he is to be had, next fiscal year (1914-15). Why, then, you naturally ask, do I recommend him to your consideration? Simply because his wife has consumption and it is quite possible that she may find it impossible to live in Ottawa or any other Eastern town. I have just received word from him that he has left for Santa Fe to see if his wife would improve there. I shall write him in New York, where my letter will probably be forwarded to Santa Fe, requesting him to drop you a note as to his whereabouts, so that, should you be inclined to take up the matter, you may know where to write him. This letter of mine is entirely at my own initiative and has by no means been solicited by Radin. I should hate to lose the opportunity of getting him for certain, but, as I said, his wife's condition may make a warmer and milder climate imperative for him. There is no man whose position makes me feel as blue as Radin's, penniless as he is and hampered by a practically incurable disease on his wife's part. I really do hope, if it is at all possible, that you will be able to make him an offer. Best regards to Mrs. Kroeber.

Yours sincerely,

E. Sapir.

---

1 Radin 1923.

[101]

Affiliated Colleges

San Francisco, Calif., May 16, 1913

Dear Dr. Sapir:

Goddard has just sent me his second manuscript for publication in our series since he left us, and definitely promises the third issue within three months.<sup>1</sup>

Of all the men who have been with us and have gone away, he is the only one who has yet found time to do anything of the kind. I realize that you are busy with many new and important undertakings, but I am certain that if you could again, in the near future, take up your Yana it would be to your advantage as well as ours.

Thanks for the word for 'in' and 'out'. Ishi understood them at once.

Sincerely yours,

A. L. Kroeber

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1 Goddard published several linguistic and ethnographic papers on Athabaskan groups (Hupa, Chilula, Wailaki, and Kato in California; Sarsi in Canada) in the UCFAAE series after leaving Berkeley in 1909. These were all based on work he had done for the University before he left, or represented extensions or revisions of this work.

[102]

Affiliated Colleges

San Francisco, Calif., May 19, 1913

Dear Dr. Sapir:

Earnest A. Hooton,<sup>1</sup> Rhodes scholar at Oxford, has referred to you in writing to me concerning our prospective position for Museum Extension work. He has sent me an outline of his academic record. In case you know him I should be greatly obliged if you would give me an estimate of his personality.

The point which it is necessary for me to have particularly in mind in regard to this position is that the incumbent must be able in his lectures to

interest the public and our classes of school children. The best anthropologist in the world would be wasted if he bored his audiences. We expect to provide field work for him on the same scale as in the past, and I am therefore very desirous of securing a man who has anthropological capacity, but the prime requisite is to find some one who can develop this public lecture work instead of running it into the ground.

Sincerely yours,

A. L. Kroeber

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1 (1887-1954), a physical anthropologist. Hooton joined the Harvard faculty in 1913, and remained at Harvard for his entire career.

[103]

5/21/13

Dear Dr. Sapir:

If we get a new man at all, it will be from University Extension funds for extension lecture work. There will be no question, except on my own part, whether he is a good, fair, or rotten anthropologist. He has got to earn his salary by doing the work there is to be done. The unanimous testimony of men who know Radin is that he is a poor speaker. As we cannot run University work into the ground to give him experience, I see no way in which I can consider him any more than Mason, much as I feel sorry for him. I might have put him in our museum a year ago, but from all I could learn he is unfit in practical affairs and would be unable to handle people. There again I had to protect my own work. You will realize that like most Universities we are anxious to get men who are scientists and pay them only for other work. Any research they may do is really a voluntary contribution and superfluity. We are lucky to be able to give them even a small opportunity for field work. I see no opening for men like Mason and Radin except with yourself, the Bureau, or a great museum like New York or Chicago which can carry a few men for their scientific ability alone without giving them administrative work. We are not in that class. I appreciate greatly your efforts on behalf of Radin, but I think you will see that

he is out of the question. I wish you knew the difficulty I have getting any means even for practical work. That is why I am considering Wallis, whom I don't know, and have written you about Hooton.

Best thanks.

Yours,

A. L. Kroeber

[104]

Geological Survey

Ottawa, Ontario, May 26th, 1913

Dear Dr. Kroeber,

In reply to your letter of May 19th in regard to E. A. Hooton, I may say that I know nothing in regard to him further than that he wrote to me some time ago as to whether it would be possible for me to obtain him anthropological work in our Division. As there seemed nothing that I could do for him, I ventured to refer him to you. I believe that Mr. R. R. Marrett, of Oxford, would be the best qualified to give you information in regard to his abilities.

Yours sincerely,

E. Sapir.

[105]

Geological Survey

Ottawa, Ontario, May 30, 1913

Dear Dr. Kroeber,-

Under another cover I am sending you a carbon copy of the first instalment of my paper on Uto-Aztekan, as I thought you might be interested in seeing the material before it came out in published form, which might be quite some time from now. The treatment of the consonants will follow as the second instalment, while a third is intended to take up the points of morphological similarity, many of which, indeed, are incidentally referred to in the course



of the present instalment.<sup>1</sup>

À propos of larger linguistic units, which seem to be somewhat in favor just now, I may say that I have been occupying myself of late with Athabaskan, Tlingit, and Haida, and that I have collected enough evidence to convince myself at least of the genetic relationship of these three.<sup>2</sup>

Yours very sincerely,

E. Sapir.

- 
- 1 The first installment, despite Sapir's doubts, was published without delay in the Journal de la Société des Américanistes de Paris (Sapir 1913a). The consonant installment was delayed by the outbreak of war in 1914, and Sapir arranged to have this part published in the American Anthropologist as well (1915a). The third installment, which was to have been a treatment of comparative morphology, was never written.
  - 2 This is Sapir's first mention to Kroeber of the "Na-Dene" relationship, a problem of enduring interest to Sapir, and which ultimately led him to undertake fieldwork on several Athabaskan languages.

[106]

Geological Survey

Ottawa, Ontario, June 14th, 1913

Dear Dr. Kroeber,-

Some time ago when you wrote me in regard to Goldenweiser and Mechling as possible candidates for a position within your University, I assumed that the vacancy referred to would be one meant to succeed Waterman's position, as I then imagined he would be coming east. I understand, however, that since then things have taken another turn, and that Waterman is to stay in Berkeley. As what is wanted seems to be one who can take up university extension work in anthropology, it seems to me that Mr. W. H. Mechling would very probably be just the kind of man you want. He has had considerable experience in anthropology, is rather enthusiastic in manner, and is easy to get along with personally. You could certainly get good research work out of him too. He has recently submitted to me a large body of Malecite myths that he has collected, with which I am, as far as I have looked them over, well satisfied.<sup>1</sup> I have suggested to Water-

man that he and Mechling get together in Cambridge or New York and talk things over.<sup>2</sup> I believe that Mr. Mechling would be glad to go out to California. He has recently been appointed University of Pennsylvania Fellow in Mexico, but would doubtless be prepared to resign that position if he could get something more permanent.

Yours very sincerely,

E. Sapir.

1 Mechling 1914.

2 Waterman was in New York at this time, finishing work on his doctorate at Columbia. Mechling was at the Peabody Museum, Harvard.

[107]

[no date--apparently Ottawa, June, 1913]

Numeral Classifiers

Yurok -eli "house, sweat-houses, nests"

Lillooet	-ê	} 1(i)to "houses"
	<u>-e</u>	

Yurok -o "months (=moons); dollars" (i.e. round objects)

Comox -qs "round objects, dollars"

Lillooet -ōca (dit.)

Yurok -ixtali "boats, wagons, conveyances"

Tcil'Quel (Cowitchin group) -aQeīl (i.e. -axwił) "canoes"

Do you suppose Yurok might turn out to be related to Salish? Tillamook is not so awfully far away! Don't tell Boas--he'll think I've gone crazy!

Yours,

E. S.

[108]

Affiliated Colleges

San Francisco, June 20, 1913

Dear Dr. Sapir:

The Yurok resemblances look mighty promising.<sup>1</sup> I would be delighted if you were to follow the thing up, and if you run out of Yurok material, let Waterman or me know. Our joint material is in his hands and is not well organized for finding any particular word, but we would do our best to give you the Yurok equivalent for any term desired. We seem at last to have got Powell's old fifty-eight families on the run, and the farther we can drive them into a heap, the more fun and profit.

I have the carbon copy of your paper,<sup>2</sup> for which my best thanks. I anticipate looking it over with much pleasure, but as I have only just returned from a vacation, have not yet been able to take it up.

Waterman, the last time he was working on Yurok, told me repeatedly that he was strongly impressed with its fundamental similarity in construction to Tshimshian, on which he had done some work with Boas while in New York. I do not believe he had in mind anything further than similarity of general morphological plan.

Sincerely yours,

A. L. Kroeber

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1 Referring to the Yurok-Salishan sets in Letter 107.

2 On Uto-Aztecan; see Letter 105.

[109]

Affiliated Colleges

San Francisco, Calif., June 21, 1913

Dear Dr. Sapir:

Waterman recently wrote me that he was hoping to have a talk with Mechling. However, the appropriation bill providing for University Extension has just suf-

ferred shipwreck, so that the outlook is once more quite dubious. This is an unexpected development in the last few days. The position we have been hoping to fill was to be an entirely new one, although much of the work under it has already been carried by us, especially through Gifford,<sup>1</sup> during the past year. Waterman did expect to leave us for a while early this spring, but I am glad that he finally decided to stay.

I have gone through your manuscript<sup>2</sup> with the greatest of interest. I can only congratulate you on your critical ability and capacity for work. Much of what you have accomplished I am scarcely equipped to do, but now that you have covered the ground, I feel that there are some points which I should certainly have worked out some years ago if I had given it my attention instead of having my head full of other and probably less important things. If anyone can still doubt the general proposition of relationship he is obviously prejudiced. I am glad, however, that you have brought the situation to a point where the question is no longer one of fact as to relationship, but one of working out its circumstances and tracking the history of the individual languages.

Do you plan to publish as a unit what you have sent me, or shall you withhold same until the entire monograph is completed? I ask this question not only because I am greatly interested in the work, but because I am wondering whether you would like me to return the manuscript which you sent, or whether it will be agreeable to you to have me retain it until a second instalment follows, when I should be glad to look it over again in connection with the second batch.

The feeling that is uppermost in my mind is one of regret at our being so small an institution that we could not have provided you with a favorable opening and kept you with us.

With heartiest congratulations, I am,

Sincerely yours,

A. L. Kroeber

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<sup>1</sup> Edward W. Gifford (1887-1959). Trained primarily as a natural historian, Gifford joined the staff at the Museum of Anthropology in 1912 and soon developed a wide range of anthropological interests. He became Curator in 1925, and succeeded Kroeber as Director after the latter's retirement in 1945. He is probably best remembered for his work on California kinship terminologies and Miwok social organization. See Foster 1960.

2 On Uto-Aztecan vowels; see Letter 105.

[110]

Geological Survey

Ottawa, Ontario, June 27th, 1913

Dear Dr. Kroeber,-

I have your letters of June 20th and 21st. I wonder if you are not taking my Salish parallels for Yurok somewhat more seriously than facts warrant. While I do not know either Yurok or Salish very thoroughly, it seems fairly obvious that there are tremendous differences between them. Thus, you state that reduplication is only sparingly used in Yurok,<sup>1</sup> whereas in Salish its use is almost excessive. As far as numeral classifiers themselves are concerned, we get these in Kwakiutl-Nootka and Tsimshian as well as in Salish. But perhaps there is something back of my similarities, after all. The trouble about comparing with Salish is itself so terribly split up into divergent dialects that it would require a great deal of specifically Salish work to determine what was really most fundamental in that stock and comparable with other stocks. As Boas has pointed out several times, there are several remarkable morphological resemblances between Kwakiutl-Nootka and Salish, and I can not help feeling that they may turn out to be genetically related. There are, indeed, at least a few lexical resemblances between Yurok and Tsimshian. Perhaps Waterman did not quite realize that numeral classifiers are not confined to Tsimshian, though they are perhaps more characteristically developed in Tsimshian than in any other West Coast stock.

I am pleased to learn that you thought well of my paper on Uto-Aztekan. I have sent it to Paris to be published in the Journal de la Société des Américanistes de Paris. I have been informed that its first instalment will appear in two successive numbers. This means that the whole paper will be dragged out over a ridiculously long time. If I had known that they were going to split the first instalment, I would probably have preferred to have it published elsewhere. As it is, however, I have committed myself. The carbon copy that I sent you was meant for you to keep. I shall be glad to let you have the carbon of any new

copy on Uto-Aztekan that I may get ready. Within a couple of weeks or so, I expect to be able to send you for publication in your series, should it prove acceptable, a paper entitled "Notes on Chasta Costa Phonology and Morphology." It is turning out to be a better-rounded paper than I had reason to think it would be, and should prove of at least comparative value for Athabaskan linguistics. So far as I know, nothing of any moment has yet been published on Oregon Athabaskan, and perhaps your series could be stretched to include this paper. Chasta Costa will probably turn out to be not very different from Tolowa in extreme northwestern California,

With best wishes,

E. Sapir.

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1 Kroeber had recently published a sketch of Yurok grammar (1911b:414-26).

[111]

Affiliated Colleges

San Francisco, Calif., July 9, 1913

Dear Dr. Sapir:

I have never taken the mere existence of numeral classifiers, or any similar phenomenon, as indicating anything else than inter-influence. When, however, you appear to discover resemblances of form corresponding to resemblances of meaning, it is another story, and it would be most important if you could follow these few cases up with similar data.

As regards your paper on Paiute and Aztec, you may be able to make some use of the list of Papago verb stems by Juan Dolores<sup>1</sup> which our printer is to set up this week, and which should appear from our press within a couple of months. You will be interested at least to find confirmation of the non-existence in this dialect of the vowel "e", of which I regard unrounded u as a substitute.

Your Chasta Costa proposition is mighty interesting, and I am very keen about getting it for our series, as you suggest. I very much doubt, however, whether this will be possible. The University rule is that University publications shall represent the work of the University. This is interpreted to mean work

done by other people on materials acquired for or belonging to the University. If your Oregon notes had been acquired as a by-product of your Yana for us, they would, without any question, be available, but as you probably obtained them while on your Takelma expedition, I am afraid they would not be considered eligible. Our rule may be a bit arbitrary, but experience has shown the necessity of drawing a definite line somewhere on account of the manifold cases which are on the border. I will, however, take up the matter as soon as our Editorial Committee get together and let you know whether anything can be done.

Sincerely yours,

A. L. Kroeber

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1 Juan Dolores (1880-1948) was a Papago Indian who worked intermittently at the Museum of Anthropology as a caretaker (and later a preparator). Kroeber and others (notably Mason) collaborated with Dolores in linguistic studies. A list of Papago verb stems (1913) and a list of noun stems (1923) were published under his name. See Kroeber 1949.

[112]

Geological Survey

Ottawa, Ontario, July 19th, 1913

Dear Dr. Kroeber,

I have your letter of July 9th. I shall be glad to use Dolores' Papago material when it is out. I presume it does not differ much from Pima. So far as I can gather at present. Pima, and with it Papago and Tepecano, are among the least archaic of Uto-Aztekan dialects. They are interesting, however, in that they share with Shoshonean i as regular development of Uto-Aztekan e.

My Chasta Costa material was obtained in 1906 when I was engaged on Takelma work for the Bureau of American Ethnology. However, this material was obtained at odd moments when I was not on regular work. It involved no extra expenditure, and I have always considered myself as entitled to do what I pleased with it without first consulting the Bureau. I thought that you might be interested in publishing it in your series, if only for the reason that it is so closely connected with Goddard's Athabaskan work. In fact, I refer constantly to his

Hupa, Kato, and Chipewyan material in my paper. I am sending it to you so that you can see and judge for yourself. If, however, the University rules prevent you from publishing it, kindly return it at your earliest convenience, so that I may dispose of it otherwise without too great delay.

With best wishes.

Yours very sincerely,

E. Sapir.

[113]

Affiliated Colleges

San Francisco, Calif., July 26, 1913

Dear Dr. Sapir:

I have your letter of July 17, also both those of July 19.<sup>1</sup> The manuscript has also come safely to hand. I am submitting same to the Editorial Committee and will advise you promptly of their decision, though it is likely to be three or perhaps four weeks before they hold a meeting. I should like nothing better than to publish the paper.

Thanks for the Yurok-Wiyot resemblances. They are all the more welcome because I should have found them myself. Thank you even more for the Pomo-Yana similarities, which will prove mighty valuable. Dixon should be here in about a month and we will then try to give our work fairly final form. We appreciate being able to make use of your suggestions.

I have rather forgotten the status of your once projected sketch of Yana ethnology. If I recall correctly, you expected to complete this for the Putnam volume but did not do so and the material has not yet appeared. Since your Yana grammar does not seem to be making much material progress, would it not be possible for you to whip this smaller contribution into shape and let us have it? It would serve to keep your connection with the University fresher in the minds of people here, and perhaps also to keep your interest in the institution warmer.

Sincerely yours,

A. L. Kroeber

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1 Two of these are missing. They presumably contained the Yurok-Wiyot and



Pomo-Yana comparisons noted in the next paragraph. Sapir wrote Radin on July 18: "I have only recently sent Kroeber a few dozen lexical resemblances between Pomo and Yana, which ought to gratify him somewhat" (Sapir to Radin 7/18/13, Archives, Canadian Ethnology Service, Ottawa).

[114]

[San Francisco]

7/30/13

Dear Dr. Sapir

Your trump card wins.<sup>1</sup> I am sure I always thought of Arapaho *bā-* when dealing with Wiyot or Yurok *me-*, *we-*, but never dreamed of anything but a coincidence. After this, when I get three aces, I draw to them. No more discards. The whole moral of our California situation is that it doesn't pay to hang back, provided one is reasonably critical to begin with; and I hope you will profit by my many years' sitting on the lid, and cut loose before somebody else does. Our recent comparisons are going to stimulate others.

The pronouns turn the trick, alone, but the rest looks good. My Yurok is in Waterman's hands, my Arapaho locked up at my office, and all the Wiyot I ever had is in my published sketch. My memory is so poor that I must wait until I have access to the notes; but instead of giving all my Arapaho to Michelson,<sup>2</sup> as I have been planning to do, as soon as I should get settled at my desk once more, I'll hang on to it now until I can follow this discovery up a bit. Meanwhile here are a few tentative suggestions, based, except for Wiyot, on nothing but imperfect recollection:

hand, W. *we*'s, Arapaho *bā* etcet  
 woman's breast, W. *weser*, A. *bāθān-i* (?)  
 belly, W. *tan*, A. *-ot*, *n-ot* (?)  
 bear, add to yours, A. *woxu*  
 wood, " " " , A. *bāθ* (?)  
 earth, W. *patut*, A. *bīta'a<sup>n</sup>wu*  
 rock, Y. *ha'āigon*, A. *haha'ana<sup>n</sup>ka<sup>n</sup>*  
 sit, Y. *tsiyuk*, *atseyuqw*, A. *θi'ok* (?)  
 Wiyot *r < ŋ* = Algonq. *l > n* shift (?)

W. 2, 3, 4, r- (< n-) = Alg. 2, 3, 4, n-, y-

father, W. dar, dan-, A. -eisana<sup>n</sup>

grandfather, W. bitc-otcker, A. -ʔbʌcibʌ

More as I find them. Ad interim, put the thing in print in some shape and nail it. I would run no risk in your place.

With congratulations,

A. L. Kroeber

- 
- 1 This letter (handwritten) is apparently Kroeber's immediate response to Sapir's announcement (in a letter unfortunately not preserved) that he had found conclusive evidence that Wiyot and Yurok were Algonquian languages. A letter from Sapir to Radin, dated 10 days earlier, is preserved, and in it Sapir writes:

The process of slaughter of linguistic families, upon which several of us seem to have embarked of late, is going on apace...I now seriously believe that Wishosk [Wiyot] and Yurok are related to Algonkin. I have been more frank with you in regard to this than with others, to whom I have only given dark hints....The consequences of this latest theory are so great that I am hesitating very considerably, even in my own mind, about committing myself, and want to get more and more evidence before I confess to myself that I am convinced.

In his letter to Kroeber, Sapir seems to have asked for additional Algonquian (Arapaho) and Wiyot-Yurok material.

- 2 Truman Michelson (1879-1938), linguist with the Bureau of American Ethnology. Michelson received his Ph.D. from Harvard in 1904 in Indo-European philology. In 1910 he joined the BAE, where he devoted himself almost entirely to Algonquian work.

[115]

Affiliated Colleges

San Francisco, Calif., August 5, 1913

Dear Dr. Sapir:

My Yurok material is in bad shape for comparisons. I never got vocabularies except at the very outset, and these are incomplete and horribly written. I have however gathered what I can find and subjoin it for your use. Incidentally this being the first time I have ever gone over the whole of these old notes for such a purpose, I find some new and very obvious Wiyot resemblances. My ortho-

graphy being inconsistent, I write q as k, and glottally affected stops as ordinary surd stops. C and S are one sound.

Yurok

man, person	oL (W. gu-wil, di-wil)
man (vir)	<u>pegerk</u>
male animal	<u>upegil</u>
woman	<u>wentsauko</u>
married woman	<u>winoc</u>
unmarried woman	<u>we'yon</u>
female animal	<u>we'yena</u>
girl	ummäi
old woman	uperei
old man	mewimar
child	hukca (W. hetca)
ghost	sä'ä
name	-gwollek
foot	-tska (W. tckatc)
knee	-erkerL
hand	-tsewec (W. we'e)
leg	-Lpil
arm	-cen
tooth	-peL (W. -pt)
tongue	-pil (W. -it)
nail	<u>weLkete</u> (W. -tkan)
ear	-tepegar
nose	-pern
eye	-lin (W. -lir)
beard	-merper
hair	-lep, -lep-täl (W. baL)
head	-moL, moL-kwo (W. wet)
mouth	-lul (Coast Yurok) (W. -lul)
lip	-erwerst
liver	<u>werLkun</u>
bones	<u>werLker</u> (W. -kerat)

heart? breast?	tseikuc
penis	-xkwet (W. -dgat)
vagina	-LpoL (W. -bec)
anus	-metik
faeces	-moloX
belly	-ye
tail	-tpeL (W. -hel)
body part prefix	werL, weL- (W. wat-)
large	pelin, pleli, etc.
brush	käpeL
stream	weroi
ash	pontet
smoke	mera
house	olomeL (W. mol)
indoors	olepek
sweathouse	ergerker
iron	pegemip
tobacco	hoxkum
pipe	rawo (Coast Yurok)
bow	camot; smoxter, cmahater (Coast Yurok) (W. cwat)
arrow	nerkwerc; horau (Coast Yurok)
quiver	cräts
come	nec
see	negwo (=newwo?)
like	ckewok (ckui, good) (W. dicgam)
eat	nep
drink	a'c
sleep	take
month	negor
dark	hoxkoL
what	tini-
where, how	kuo
no	pä

rock	hã'ai, ha'xi-gon (Arapaho, haha'ana <sup>n</sup> ka <sup>n</sup> )
small	tseixkeni
many	teinem
good	ckãi
deer	puuk (W. but-caweti, white deer)
bear	tsieri (W. tsetsgeruL-)
wild cat	temokwer
fox	wergec
coyote	cegep
skunk	wateeL (W. botcwi)
rabbit	herkwer
mouse	negenits
eagle	taxteL (W. di'l)
dog	tsic'a, megokw
ocean	pickãL
ocean, plain	tewolli
stick, wood	kowic, yoLkoitc
board	naxko
earth	LgeL (W. lagerak)
sky	wec'ona
sky	wonoyek (won-, up) (W. wen, wir-)
rain	tenpuL
snow	rarir
wind	rokw
sun, moon	won-uclei (won-, up)
star	hogets
road	layekc
fire	mets (W. mes, wes)

The Algonkin distinction of animate and inanimate is found in the Yurok adjective. Yurok is the only language in California, so far as I know, which has ä of the quality of Arapaho and Fox.

How about Wiyot patut, Arapaho bita'a<sup>n</sup>wu, earth?

Sincerely yours,

A. L. Kroeber

[116]

[Ottawa, Ont.]

Aug. 5, 1913

Dear Dr. Kroeber,

Thank you for your letter of July 30th. Shall write more at length regarding it a bit later. Arapaho ba-θan-i "woman's breast" = W. we-ser < \* -sen is fine, if quoted correctly from memory. I could find no Algonquian cognates for ne-ser before, tho I looked hard. This suggests that Arapaho (and probably Cheyenne) may have very peculiar value of their own in reconstructive work. Their very divergent position within Algonquian now assumes new significance.

Several of your resemblances I had already discovered independently. Am trying to work out phonetic laws, but am hampered by rather small amount of material to work with. Still, it is astonishing that with only quite little W. and Y. I have accumulated as good evidence as, probably better than, for genetic unity of Haida, Tlingit, and Athabaskan. You will be interested to learn that I have some evidence to show that W.-Y. L becomes Algonquian g (or c).

Yours,

E. S.

[117]

Ottawa, Ont.

Aug. 6, 1913

Dear Dr. Kroeber,

A few notes on your Arapaho parallels:-

W. we's "hand" : Ar. b̄h̄et̄c̄et. Ar. b̄h̄-et̄c̄et is evidently cognate with Cree mi-tc̄it̄ciy (? < \* -tc̄iti; t, d become tc, dj before i in Fox, Ojibwa, and doubtless elsewhere). I had hesitated to connect we's with mi-tc̄it̄ciy, particularly as I did not like to separate -tc̄it̄ciy from Ojibwa -nindj(i) "hand". Ar. b̄h̄e-, however, suggests that we deal with b̄ä'e- (Algonquian \*me'e-), which would go excellently with Wiyot we'-. Ar. -t (? < \*-ti), Cree -tc̄iy are probably old suffixed elements. (As further indicated, or at least suggested, by Blackfoot

no-tsi-s "my hand". Uhlenbeck gives no rule as to when ni-, when no- is used. I wonder if it is not really no-'tsi-s; no- regularly before '.) Then W. we's (? < \*we'se-) = Ar. bä'etce-, Cree mitci-. À propos of glottal stops. What are we to do with Blackfoot and Arapaho glottal stops? They occur also, yet not very often (mostly between vowels), in Ojibwa. One problem ahead of Algonkinists is to explain these Western glottal stops, perhaps find their reflex in more eastern dialects. It is not unlikely that W. and Y. ' will turn out to correspond, at least in part, to Blackfoot and Arapaho '. I suspect Cheyenne also has '. This matter needs looking into badly.

W. tān "belly" : Ar. -ot. Ar. -ot is evidently cognate with Ojibwa m-ōdji "belly of an animal" (< \*-ōdi). You may be right, but I had noted Cree m-atāy "ventre" (I use Lacombe for Cree) as plausibly connected.

W. māti "wood" : Ar. bəθ. It is hard to decide on this until we know what becomes of Algonquian st in Ar. If st becomes θ, then original \*mesti-, \*misti- would become bəθ. However, I consider it perhaps more plausible that Ar. bəθ be reconstructed to \*mes (cf. W. weser : Ar. bəθāni) and compared with Blackfoot misau "wood", Ojibwa missan "wood for fuel" (-an is inanimate plur.), mishi "piece of wood for fuel", Natick mishash "wood" (-ash is inanimate plural). This Algonquian \*mis-, \*mes- "wood for fuel" easily falls in line with W. mes "fire", Y metc "fire". (Cf. Takelma p!ī "fire; firewood".)

W. pātūt "earth" : Ar. bīta'ā<sup>n</sup>wu. If correct, this implies that Ar. b does not always go back to m, though perhaps W. p, b sometimes corresponds to Algonquian m (see below under bitcō-tcker).

W. r n : Algonquian l > < n shift. I disagree with you on this. I now believe that l and n are two originally distinct Algonquian consonants, as shown by eastern dialects (such as Delaware and Malecite), where l and n are kept apart, and by Cree, where original l appears as y (in Lacombe's dialect), original n as n. In certain Algonquian dialects n and l fell together into n (as in Ojibwa, Fox, and others). Wishosk<sup>1</sup> r and n are evidently etymologically identical consonants. Though they seem at times to vary with l also, I believe r/n and l are to be considered distinct. W. r/n = Algonquian n; l = Algonquian l. For original l, see, e.g.:

W. welu, wilu "hollow" : Ojibwa wānike "to dig a hole in the ground";

Cree wāyā'iw "creux" (inanimate); Micmac (Rand's material)

wōl-běgádākūn "to be hollow" (wōl- = wal-).

Initial l- seems, however, to have become general Algonquian n- in some cases:

W. lakwet "to cough" : Malecite nēk<sup>cw</sup>-hAmq- "to cough".

How about Y. r and vocalic er?

W. rit(w)- "2", rik(w)- "3", riaw- "4" : Fox nīcw-, nesw-, nyāw-. Could anything be neater? This, of course, was one of my strong pieces. It is laughably obvious. Cf., of course, also Y. ni'- "2" and naxkc- "3". Cree for "3" has nistw- as stem. I wonder if original \*niksw-, \*neksw- would not explain all forms. Cf. also Blackfoot niok(skūm) "3" with its -k- as contrasted with absence of -k- in Fox. How in God's name to explain Cheyenne nāǎ "3" (nan-6e "30", nān-oxtoě "8" = "3 added")? There'll be some pretty problems of phonology to sweat over!

I have ventured also to compare W. gō't-, Y. goo(x)t- "one" with Fox ne-gut-, Ojibwa nin-got- "one".

W. dār-, dān- "father" : Ar. -eisana<sup>n</sup>. Cf. also Abenaki kIt-adán "thy father", ōd-adan-á' "his father". It is not at all unlikely that related to this is also Ojibwa -dān-iss "daughter" and related forms in other Algonquian dialects. -iss is probably diminutive. Note that W. dān- denotes also "son".

W. bitcō-tcker "grandfather" : Ar. -ǎbāci-bǎ. Cognate with Ojibwa -misho-miss and Cree -muso-m, both of which have -o- as in W. I have other cases of W. tc becoming c, s in Algonquian. -tcker evidently suffixed; cf. gō-tcker "grandmother". With this gō- cf. Cree -okku-m- (okkuma "sa grand' mere" suggest -kku- as stem), Ojibwa noko "my grandmother!"

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Well, there's something in it all, i' faith! On another sheet I give some extra good ones, tho by no means all I have. I think you may be right about publishing. There seems to be enough to lift my hypothesis into practical certainty, so why not make people aware? Detailed study can follow.

Yours,

E. Sapir.

1 Wiyot. Kroeber had used "Wishosk" in his early papers, then switched to "Wiyot" as more accurate.



[118]

[Ottawa, Ont.]

Aug. 8, 1913

Dear Dr. Kroeber,

To show how important W. Algonquian dialects may turn out to be, consider:

W. mīw- "to eat (elk)" (see p. 406 of your paper<sup>1</sup>): Fox Amw-; Ojib. amwa "to eat (anim. obj.)". Only this moment I discovered in Mooney's "Cheyenne Indians" (p. 426): miv- "to eat (anim. obj.)". Ch. v = original w. You see, in this case, Ch. actually agrees better with W. than with other Algonquian dialects.

W. m-okèc "singers" : B. m-okétsis "singer" (-is is common as ending in Bl.)

Are these "accidents"? Fiddlesticks!

Yours,

E. S.

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1 "The Languages of the Coast of California North of San Francisco" (1911b).

[119]

Geological Survey

Ottawa, Ontario, Aug. 12, 1913

Dear Dr. Kroeber,

Thanks ever so much for your supplementary Yurok material of Aug. 5th.

It has come in handy for some added cognates. Most interesting of these are:

Yurok werLkun "liver" : Cree oskun "liver" (cf. Yurok werLker "bones" : Cree oskan "bone"; does werL- in both cases regularly become os-? You remember there is reason to believe L becomes Algonquian s. Do you think W. wat- and Y. werL- necessarily correspond? It is true that final -l and -t both seem to become -L in Yurok.

Yurok werges "fox" : Ojibwa wâgosh "fox"

It is satisfying to find that W. and Y. have more resemblances than was at

first apparent. However, some of your comparisons I cannot consider plausible.  
Add to yours:--

Yurok -LpiL "leg" : W. we-liL "foot"

Yurok -pern "nose" : W. m-etere (?)

(It looks as though Y. had p in some cases where no consonant is found elsewhere. Cf., besides -LpiL and -pern above, -piL "tongue" : W. miit.)

Yurok -moloX "faeces" : W. me'l.

In writing up my paper I was thinking of beginning by giving further (chiefly lexical) evidence of genetic relationship of Y. and W. than has been already given,<sup>1</sup> with due acknowledgement, of course, of your added data. Would this interfere in any way with your own plans?

Yours sincerely,

E. Sapir.

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1 Kroeber 1911b:414-26.

[120]

Affiliated Colleges

San Francisco, Calif., August 12, 1913

Dear Dr. Sapir:

I enclose a brief list of Arapaho stems which may be of service to you. On the whole, however, this dialect is so specialized that I imagine you will be able to make comparatively little use of it. At the time I recorded these words I did not know anything about surd vowels. I imagine that every final vowel in this list is really surd, and that every final consonant should be followed by a surd vowel. You will see that I have few, if any, additional resemblances. I am, however, as convinced as ever that you are on the right track. I hope that you can find something in my recent Yurok list.

Sincerely yours,

A. L. Kroeber

[121]

[Ottawa, Ont.]

Aug. 13, 1913

Dear Dr. Kroeber,

How about this?---

W. wetserakw "son-in-law" (< \*wetsenakw) : Natick wussénum  
(i.e. wAsínA-m-)

You will recall that W. tc, ts often appears as Algonquian s. Ojibwa, Kickapoo, Cree, Blackfoot, and Micmac differ completely from Natick on this word.

Yours,

E. S.

[122]

Ottawa, Ont.

Aug. 14, 1913

Dear Dr. Kroeber,

It just struck me yesterday that Yurok r and Wiyot r must be largely distinct in origin. Wiyot r, as you point out, is always n in origin. Yurok r, however, seems often to correspond to original t:-

Y. qoot-, qoxt-, qooxt-, qoxts- "one" : qoor-e<sup>u</sup>

Y. -pern "nose" : W. m-etere \*-etene; Micmac -itn. Y. -p-ern  
= \*-p-etn

Y. horäü "arrow" : Cree atus "arrow"; Oj. mitigw-anwi (mitigw- = "wood"). Cree t and Oj. n not infrequently correspond. I imagine that \*atu-, \*atw- lies back of Y. horäü and Cree atu-s.

Yours,

E. Sapir.

[123]

Affiliated Colleges

San Francisco, Calif., August 14, 1913

Dear Dr. Sapir:

I return herewith the list enclosed with your letter of August 6.

Your suggestion that the prefix to the Arapaho word "hand" is really bā'e- is I think correct. You will notice that nearly all the Arapaho names of body parts which I sent you the other day begin with e or i as I have written them after subtracting the ba.

This same bā- prefix by the way is wa- before stems containing back vowels, for instance wa-not. The other possessive prefixes show a similar alternation between ei and ä on the one hand, and a on the other, according as the stem contains front or back vowels. I do not wish to be understood as saying that this corresponds in any way to the Yurok change of the possessive prefix from e to er, but the shifts are certainly of the same general character.

I threw out the hint as to connection between Yurok r-n shift and Algonquian l-n shift only at random. You are very likely correct that they have nothing to do with each other, just as the occurrence of ä in Yurok and Western Algonquian may fall before critical analysis.

I hope in any event that you will be persuaded to publish soon. We are going to have a flood of similar cases in the next few years, and the prestige of American anthropology will be better maintained if at least some of the claims come with the backing of your critical faculty than if they are all made by people in whom the rank and file of us have less confidence. I think this is an important reason, in addition to the purely personal one of securing for yourself what you have found.

Sincerely yours,

A. L. Kroeber

[124]

Ottawa, Ont.

Aug. 18, 1913

Dear Dr. Kroeber,

Thanks for your Arapaho vocabulary, which has just come to hand. Arapaho may be very divergent, but is evidently none the less thoroughly Algonkin. I doubt if Arapaho hīsis "sun, moon" has anything to do with Y. won-ucl̄ei "sun". hīsis cannot well be separated from Ojibwa gisiss "sun" and its cognates. Very interesting to me is Arapaho hoti "arrow", concerning whose Cree and Ojibwa cognates (also Y. horäü) I wrote you recently. Possibly Arapaho hoti < \*hotwi < \*hatwi, which would go well with Cree atu-, Oj. -anwi. Original form probably with h-; hatw- (?). be-itš̄iθ "tooth" is interesting because of first -i- of stem; -itš̄iθ < \*-ipit (Cree m-ipit). Were your stem \*-pit, "his tooth" in Cree would be \*o-pita, not w-ipita. All this goes well with W. m-èpt, whose long è suggests contraction from \*me-i-. Perhaps Y. -pil should really be -ipiL. It seems Arapaho θ sometimes goes back to t as well as tc, as in -itš̄iθ and -iθan "tongue" (cf. W. m-it; Bl. m-atsinf < \*m-atinf \*m-itani; with western Algonkin -itan cf. Delaware -īlAnō); probably this is when t is preceded by i, which palatalized it to tc, which then regularly became θ.

Yours,

E. Sapir.

[125]

[no date--probably August 1913]

Dear Dr. Kroeber,

Hurrah for Arapaho! If no<sup>n</sup>ku "rabbit" = Oj. wābos (see Michelson), why can't Arapaho nīna<sup>n</sup> "house" go back to \*wīna<sup>n</sup> (cf. Malecit meno'-djiIgwom "house")? And if so, how about:--

Ar. nīna<sup>n</sup> : W. kac-werar "small house" (< -winan)?

I imagine that Ar. w- becomes \*m- before following n or nasalized vowel and that \*m--n is then assimilated to n--n.

Yours,

E. S.

[ 126 ]

Geological Survey

Ottawa, Ontario, Aug. 23, 1913

Dear Dr. Kroeber,

How about this?:--

W. capo "straight" : Natick sampwi "it is straight"

It is curious that I get several Natick cognates that seem to find no parallels in Cree, Ojibwa, or other better known dialects. You remember "son-in-law". Another such case is:--

W. me-lokaL "throat" : Natick mu-nnaonk "throat"

I am writing up my paper now. I find I already have about 75 noun stems that are cognate (some rather doubtful, of course); in all I have 150 or more stems or grammatical elements that correspond. Not bad, is it? Have you any other data as to animate and inanimate in Y. and W. than what you have already given as to Y. adjectives?

Yours,

E. Sapir.

[ 127 ]

Geological Survey

Ottawa, Ontario, Aug. 26, 1913

Dear Dr. Kroeber,

Hurrah for Blackfoot this time! You may remember what I wrote about numeral classifiers. I pointed out Yurok -en "days" : Oj. -gwan, which, if correct,

would probably have to be analyzed as -gwa-n. On looking through Uhlenbeck's<sup>1</sup> latest Dutch pamphlet I find:--

Bl. -ni- "days and nights" (numeral classifier)

Bl. -mi- "(so and so many years) in age, old" (numeral classifier)

Cf. respectively Yurok -en and -emi "times, occasions, years". Isn't it encouraging to find how well Bl., Cheyenne, and Arapaho go with Y. and W.?

Yours,

E. Sapir.

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1 C. C. Uhlenbeck (1866-1951), Dutch linguist who had worked on Blackfoot.-- Trained as a Germanist and Indo-Europeanist, Uhlenbeck became interested in wider genetic and typological perspectives. He took up the study of Basque, Eskimo, and (after 1910) Algonquian. His work on Algonquian included extensive fieldwork on Blackfoot in 1910-11.

[128]

Affiliated Colleges

San Francisco, Calif., September 6, 1913

Dear Dr. Sapir:

It will be impossible for the University to publish your Chasta Costa paper,<sup>1</sup> as you will see from the enclosed copy of the letter from the secretary of the Editorial Committee.

I may add that the committee did not act hastily. The proposition was laid before them and fully explained by me at a previous meeting, but decision postponed until yesterday so that the merits of the case might be examined in- to as fully as possible.

To clear the record I should also state that I had not formally submitted your paper as Mr. Allen seems to intimate, but held the manuscript and sent to the committee only the suggestion of publishing, with a statement of all the relevant facts. You are therefore not in the position of having been turned down.

As I wrote you before, this result was what I expected, but I am none the less sorry that it was necessary.

I am returning your paper under separate cover.

Sincerely yours,

A. L. Kroeber

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1 See Letters 110-112. The paper was published by the University of Pennsylvania (Sapir 1914).

[129]

Port Arthur, Ont.

Sept. 12, 1913

Dear Dr. Kroeber,

I am on my way to Vancouver Island with Mrs. Sapir and heir-apparent. Just before leaving Ottawa I received Dolores' "Papago Verb Stems".<sup>1</sup> I have been reading this with great interest and have landed several interesting Shoshonean-Piman cognates that were new to me. Such are:--Pap. ammoh "to talk loud" : S.P. ampaya- "to talk" (both Pap. -h and S.P. -x- go back to -k-); Pap. vaI "to call" : S.P. pai-; Pap. wō-hPÜ "to run (pl)" < \*p₂- : S.P. pōya- "to run (pl)"; and others. I am particularly delighted to find that Papago keeps rigidly apart o (my ɔ; corresponds to S.P. open o, Ute ü) and u (= S.P. close o, u), as I had rather expected it would. I think it clear now that we have to deal with 5 primary Uto-Aztekan vowels (a, i, o, ɔ, and e or ī). It is clear that original s (and c) became h in Papago (I had already suspected this from what I knew of Pima and Tepecano); cf. Pap. hi'I "urine" with S.P. si'i "to urinate". Where Pap. has s and c, they seem, at least in some cases, to go back to original ts and tc. Cf. Pap. cohcah "to cry" with Nahuatl chocoa. tc, in turn, evidently goes back to t (before u, i, ü). Postvocalic k becomes h; cf. muh "to die" with Hopi moki. Initial p becomes v (w), evidently enough. Pap. p at first puzzled me, but it suddenly dawned on me that it goes back to original kw. E.g. Pap. pah "to swallow" : Nah. qua; Pap. püI "to take" : S.P. qwīi- "to take"; Pap. pahhi "tail" : S.P. q'wā'si- (= qwas'i-) "tail", Cora kwasi; Pap. pihI "manure" : S.P. qwitca- (< \*kwita-), Nah. cuitla-. This change of kw > p seems particularly interesting to me.



It is almost humorous to see how stems at first sight very divergent in appearance turn out to be identical when phonetic laws have been worked out. E.g. Pap. wuhhA "to awaken" (< \*pusa) = Cora -xis in ta-xis "to wake up" (-xis, -his < \*-pus). Phonetic laws have worked destructively in Pima-Papago-Tepecano and have made stems quite diverse in origin look alike or identical. E.g. Pap. tcuI "to grind" (leg. tcuhI ?) < \*tusi (cf. Nah. teci; S.P. t<sup>u</sup>'cu- = tuc'u- < \*tus-i); but tcuI "to put out the fire" (leg. tcuhI ?) < \*tuki (cf. Nah. toqui-a; S.P. tuywi- < \*tuki "fire goes out"). Yes--Uto-Aztekan rests on bedrock. I am planning to write my second instalment (Consonants) in Vancouver Island, if I get time. My address will be Alberni, B.C. I hope you get a chance to drop a line once in a while.

Yours sincerely,

E. Sapir.

P.S. I finished my paper on "Wiyot and Yurok, Algonkin Languages of California" shortly before leaving Ottawa and sent it in to Hodge.<sup>2</sup> I think my evidence looks convincing enough. Some phonetic laws seem to work out beautifully.

1 Dolores 1913; see Letter 111.

2 F. W. Hodge, Editor of the American Anthropologist. The paper appeared in the last issue for 1913 (Sapir 1913b).

[130]

Affiliated Colleges

San Francisco, Calif., September 23, 1913

Dear Dr. Sapir:

I am glad to hear that your Wiyot-Algonquian paper is on its way to publication. I am glad that you interpreted as consent my silence upon your inquiry as to introducing our new Wiyot-Yurok resemblances.<sup>1</sup>

Papago tcuI is tcuI, and not tcuhI. Juan Dolores does admit, however, that certain sub-dialects use the sound "h" more abundantly than his, so that wider knowledge may prove your conjecture right. The final surd I contains,

as always, a tremendous amount of breath, and it is not impossible that in this and some other words it may have "absorbed" a preceding h.

Dolores is now working out a list of suffixes. He has a good body of texts, but I have had him hold these until he should have carried his analysis farther. I shall not be financially able to carry him much longer this year, but he has now progressed so far that it is only a question of time until he will have the language pretty thoroughly on record.

I have similar hopes for Northern Paiute. The Dr. Marsden, on whose account, and in connection with whom Waterman made his phonetic study,<sup>2</sup> died some months ago. Mrs. Marsden has sent me his manuscripts. The grammatical notes were in some confusion, but I have straightened them out, and they are evidently pretty sound because his texts, of which he has more than thirty of moderate length, analyze very readily. Marsden had not got around to correct his early erroneous spelling, but in the end this may not prove the misfortune which I first thought. In November I hope to have a Northern Paiute down here, and expect that Dolores and myself, working on the basis of Waterman's determinations, will probably be able to make a more certain job of the orthography than Marsden himself could have done. The final result will not be very bulky, but I am confident that there will be sufficient to give an intelligent comprehension of the language.

Sincerely yours,

A. L. Kroeber

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1 See the end of Letter 119.

2 Waterman 1911; see Letters 73-74.

[131]

Alberni, B.C.

Sept. 28, 1913

Dear Dr. Kroeber,

If I of tcuI, as you say, "contains, as always, a tremendous amount of surd breath," I find it somewhat hard to understand how one would distinguish tcuI from tcuI. Does Dolores in cases like tuchI hear free breath without definite timbre (= h) followed by breath with i-timbre (= I)? tcuI "put out the fire" (< \*tuki- ; cf. N. toqui-a "put out the fire") is doubtless phonetically and structurally parallel to tcüh "taste" (< \*tüka- ; cf. S.P. tiq·a- "eat"). Is h of tcüh not followed by A? I wonder if Dolores does not more easily hear certain final timbres (particularly I, U, and Ü), which he then writes generally without preceding h, than certain others (A and O), which are then merely implied in final h (I do not necessarily imply, however, that all cases of final h are to be understood as involving some specific vocalic timbre, though I strongly suspect that in several cases -h should be understood as -hA, -hO, -hÜ, and so on; these sounds I would probably write -h<sup>a</sup> or -<sup>a</sup>). I think it is unfortunate that Dolores gives not stems but absolute verb forms. It would have served comparative and general analytical purposes better if he had abstracted verb stems from fuller forms, so that their final elements could be seen in undisturbed form; thus, tcui- or tcuhi-. Similarly, in S.P., it is far more useful to operate with maxa- "to give" than with max· or max·<sup>a</sup> (such forms as maxa- are not really theoretical, cf. narrative past maxa-p·uxa' ; -p·uxa', in turn, should in analytical work always be given as -p·uxai-, cf. -p·uxai-aq'<sup>a</sup> "he in past time").

I am glad of your progress in Shoshonean work. Have been taking down some more Nootka texts. Am used to Nootka phonetics, but it certainly is "the limit."

Yours,

E. Sapir.

[132]

Affiliated Colleges

San Francisco, Calif., October 4, 1913

Dear Dr. Sapir:

I should have been glad to have Dolores' first paper consist of a list of true stems, but did not consider this quite safe, especially as he is a beginner. He is now working at the affixes. There are also some changes of stem between the progressive and completed action forms of the verb about which I am not clear. He will get the stems in time, but is not yet quite ready for the task.

As to the final surd vowels, you are correct in assuming that most of those following consonants are nothing more than a timbre coloring of the explosion of the stop. I did not hear these colorings at first, but he gradually commenced to write them. While I think he is generally correct in noting the difference, they are not very great and did not strike me as being of particular importance. On the other hand, the surd vowels following another vowel are very plain and I am sure could not be omitted without destroying the meaning of the word. The timbre in these cases is very marked. TcuI could not be heard as anything else. According to our orthography, tcuhI would be exactly what you suggest, namely u followed by uncolored h shading into h colored with i timbre. In fact if there was such a word he would probably write it tcuhhI, as the h would affect the preceding vowel and render it aspirated although remaining sonant. However, as I wrote you, I hear the word precisely as he does, and it unquestionably is tcuI, and not tcuhI or tcuhhI.

The spelling uh for an aspirated sonant vowel is not very desirable, but for a reason which you can appreciate, it seemed particularly urgent in this case for me to work out an orthography that would be as simple in appearance as possible. As long as there is no other than a technical objection to the spelling, I think no serious damage is done.

You will get your Papago roots before we get through.

Sincerely yours,

A. L. Kroeber

[133]

Affiliated Colleges

San Francisco, Calif., December 30, 1913

Dear Dr. Sapir:

Before you wrote me about your Algonquian discovery last summer I had in Mr. Hodges's hands for publication in the Anthropologist a brief paper by Dixon and myself on relationships of the Indian families in this state.<sup>1</sup> I have only now had proof of this article. A paragraph in it about Ritwan is of course entirely superseded by your work. I do not however know when or where you expect to publish, nor even whether you are ready to have it stated in print that you will publish on the matter.<sup>2</sup> I am suggesting to Dixon that we revise our paragraph on Ritwan to read as follows:

"Since the unity of Yurok and Wiyot was announced, Dr. Edward Sapir has made the brilliant and startling discovery that the two languages are not only related but are Algonkin. This larger group includes and therefore supersedes the short lived 'Ritwan.' As complete material as is at present available on this relationship will be included in Dr. Sapir's presentation of evidence."

If this statement is agreeable to you, please notify Dixon at once so that he may forward proof of our article to Hodge with his final approval.

If, on the other hand, you would prefer us not to make announcement on your behalf, please notify Dixon to ask Hodge to hold the article; and at the same time kindly advise me of your desires in the matter, so that I may make the passage conform to your wishes. This must be done by myself as Dixon has no Yurok and Wiyot material at hand. If you expect that it will be some time before you publish, and wish us to say nothing about your discovery, I should like to include all the Yurok-Wiyot resemblances which I have.

With best wishes of the season, I am,

Sincerely yours,

A. L. Kroeber

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1 Dixon and Kroeber 1913b.

- 2 Kroeber's wife, Henriette, died late in 1913, of tuberculosis. In the turmoil of these months, Kroeber apparently forgot that Sapir had told him he had finished the paper and submitted it to the American Anthropologist in September (Letter 129, Postscript). Moreover, Kroeber addressed this letter to Ottawa, and not to Vancouver Island where Sapir was spending the winter working on Nootka. Receiving no reply from Sapir before the proof had to be returned, Kroeber did not include the suggested paragraph. (It is curious that Hodge did not inform Kroeber of his receipt of Sapir's paper.) In any event, Dixon and Kroeber's "New Linguistic Families in California" and Sapir's "Wiyot and Yurok, Algonkin Languages of California" appeared side by side in the October-December, 1913 issue of the journal.

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Alberni, B.C.

Jan. 14, 191[4]

Dear Dr. Kroeber,

Your letter of Dec. 30th reached me only today. I have absolutely nothing to object to in your revised paragraph, as I have no desire to keep my discovery secret. I acted on your suggestion and put my material into shape in a fairly long article which is to appear soon in the American Anthropologist.<sup>1</sup> I have recently read proof on it.

I expect to stay here till about March 1st. I believe I have been rather successful this trip, on the whole. The Nootka are intensely interesting, both ethnologically and linguistically. Perhaps the most interesting thing about them in the former respect is the extremely large number of distinct kinds of things that are handed down as family privileges (ranging from hunting rights to the right to give a slave a certain name); it is interesting to note that the Nootka have a very precise native term for all privileges that are reserved for particular lines of descent, the term applying as much to a name or song as to a property right. Structurally, Nootka is much like Kwakiutl and yet there are many important differences. Kwakiutl is far more complex in its development of demonstrative and case suffixes, Nootka far more so in its purely derivative suffixes (there must be over 300 different suffixes of verbal significance alone, not to speak of numerous local, body-part, and other elements). I have taught one of my interpreters<sup>2</sup> to write Indian. He does it

remarkably well (fully as well as George Hunt<sup>3</sup> does Kwakiutl) and has already supplied me with valuable texts that he has written out himself with inter-linear. So interesting are the family legends in their bearing on the history of privileges that I am relatively neglecting myths pure and simple, tho I can doubtless always get more of them from my interpreter. Has Waterman been doing more on Yurok lately?

Yours sincerely,

E. Sapir.

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1 Sapir 1913b.

2 Alex Thomas (d. 1971)--Thomas was the grandson of Tom Sayachapis, Sapir's principal informant. Over the next decade Thomas sent thousands of pages of phonetically transcribed texts to Sapir in Ottawa. In 1934, Sapir arranged for Thomas to spend several months at Yale, to assist him and Morris Swadesh in preparing a volume of Nootka Texts (Sapir and Swadesh 1939). A second volume appeared in 1955. See Thomas and Arima 1970.

3 Boas's chief informant. See Maud 1982:89-95.

[135]

Affiliated Colleges

San Francisco, Calif., January 22, 1914

Dear Dr. Sapir:

Nobody seemed to have any idea that you were still in the field, and Dixon and I had a lively time trying to locate you. I had wanted to dispose of the Ritwan matter in our joint paper in the *Anthropologist* by referring in three lines to your discovery, but failing to get authorization from you in due season, furnished Hodge instead with a list of all my Yurok-Wiyot resemblances, including those which you were kind enough to point out last spring. If Hodge had only said that he had your paper in press, we should have been able to leave the whole matter to your handling.

Is there any chance of your being able to work up Southern Yana with Ishi during our next fiscal year, which begins on July 1st? Waterman is planning to take him back to his own country this summer to get the ethno-geography and

material culture. If you should be in British Columbia again, say a year from now, and could spare the time for a run down here, can you form an estimate of what the total cost to us would be? I am very anxious to have you do this work if you care to, but you know our resources and the necessity we are under of figuring the cost in advance.

You may be interested to know that Seri is Yuman and therefore Hokan. Everybody who has ever taken the trouble to look at the evidence has probably had this impression, in spite of, or I might better say, just because of Hewitt's paper;<sup>1</sup> but on systematically going over only a little of the material recently I was surprised at the strength and abundance of the resemblances, and the readiness with which phonetic equivalences appeared.

Sincerely yours,

A. L. Kroeber

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1 Hewitt 1898; see Letter 98, FN 3.

[136]

Alberni, B.C.

Jan. 28, 1914

Dear Dr. Kroeber,

I am sorry you had trouble about fixing up your Yurok-Wiyot material. Acting on your permission, I had included all your and my Yurok-Wiyot correspondences in Pt.1 of my paper, of which I have just corrected galley. This seemed necessary, as, to prove that Algonkin was related to both Yurok and Wiyot, it was important to show that these latter were indeed related to each other. I have, of course, given due credit. Some supplementary material that Waterman sent me has also been incorporated.

I am very doubtful if I can work up Southern Yana during your next fiscal year. I shall not be out here next summer and do not wish to have another long winter spell just yet, for it is as cheerless as Hell here in the winter, tho wife and baby naturally make amends. I am thinking of continuing Nootka work



at some point N. of here in the summer of 1915, but that would be after your next fiscal year--and I suppose there is no use crossing bridges.

Am glad to learn Seri is Yuman after all. I have, like you, lost my love for an unlimited number of stocks. I forget now whether "Hokan", which you say embraces Yuman, is Yokuts-Maidu-Wintun-Costanoan-Miwok or Shasta-Achomawi-Chimariko-Pomo-Karok-Yana.

Yours sincerely,

E. Sapir.

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Affiliated Colleges

San Francisco, Calif., February 7, 1914

Dear Dr. Sapir:

I think you wrote me that you would be on Vancouver Island until March. Is there any chance of your coming down here for a few weeks of Southern Yana work then? If so, will you estimate the cost? I might still be able to arrange it. I think you would enjoy the work, Mrs. Sapir would be pleased with the change of climate, and we should all be delighted to see you both.

Hokan is the Shasta-Yuman group, Penutian the Wintun-Yokuts.

There was no particular trouble in connection with the Wiyot-Yurok section of your Algonquin paper, but I was anxious to leave the whole matter to you if I could. I suspected that your paper might be appearing in the same number of the Anthropologist as ours. We will now probably both have lists which will largely duplicate one another, but each contain some material not in the other.

Sincerely yours,

A. L. Kroeber

[138]

Alberni, B.C.

Feb. 11, 1914

Dear Dr. Kroeber,

I am afraid that I shall not, after all, be able to get down to San Francisco this year, though I had been rather looking forward, when leaving Ottawa, to spending some time with you and Ishi in California after finishing up here. One reason is that our household goods in Ottawa, which had been stored at a friend's house, were in large part burnt recently, which makes it advisable that we get back to Ottawa as soon as practicable, so as to come to terms with our insurance agents. All my MSS., including all my Yana material, are safe, as they are either with me here or were placed in the Museum before I left. Another and more urgent reason why we would like to get East as soon as we can is that Mrs. Sapir's health of late has been quite bad. She suffers constantly from acute constipation and indigestion and I feel that she must get a thorough examination at the hands of a competent doctor in New York at the earliest opportunity.--I hope we can get together, though, some time before the Millenium! Distances are frightful in America. I gather that you have some more Wiyot-Yurok correspondences now. This is, of course, as it should be. There is one interesting one that I failed to make a precise note of before leaving Ottawa and so was unable to add to my paper when reading proof. "Drink" in Wiyot is weratc- ; in looking through Powers' vocabularies rather hurriedly I noticed that "drink" in one Yurok dialect is something like werckw- (I really don't remember just what it is). This case is valuable because of Algonkin minikwe (Oj.), Arap. bänä.--Yuman as related to Yana is of course doubly interesting to me. Possibly of some significance here may be this:--As I remember your paper on Noun Composition, qualified precedes qualifying noun in Mohave.<sup>1</sup> In Yana qualifying noun precedes, as so commonly, qualified; thus: au-ha "fire-water, whisky". However, with body-part compounds, as person's heart, deer-liver, Yana is like Yuman. Thus, hauyau-ba "fat (of

deer". I am looking forward with much interest to your and Dixon's new paper on Californian languages.<sup>2</sup> How is Waterman getting on with Yurok?

Yours sincerely,

E. Sapir.

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1 Kroeber 1910a:208.

2 Dixon and Kroeber 1913b.

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Affiliated Colleges

San Francisco, Calif., February 21, 1914

Dear Dr. Sapir:

I am very sorry to hear that Mrs. Sapir has not been well. Under the circumstances, and the added one of a fire in your home, I can well imagine that you must be anxious to return East as soon as possible, where I hope everything will adjust itself to your satisfaction. I do hope, however, that we shall be able, before too long a time has elapsed, to make an arrangement enabling you to get together with our Yana friend. You would find him a most communicative informant. Is there any likelihood of your making the trip here in connection with the Exposition?<sup>1</sup>

I am interested in what you say about the order of elements in noun compounds in Yuman and Yana. I have made several attempts to get a clear idea on this point, thinking that a distinctive feature of the group might emerge, but the available material is so slight and confusing, that I have only been perplexed. Composition is obviously a bad business to say much about until structure is pretty well known. My essay on noun incorporation<sup>2</sup> would be sufficient to demonstrate this, though I have always been exceptionally well

satisfied with this attempt of mine on account of its provoking you into finding the solution.<sup>3</sup>

With best regards, I am

Sincerely yours,

A. L. Kroeber

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1 The Panama-Pacific Exposition, San Francisco, 1915.

2 Kroeber 1910b.

3 See Letter 71.

[140]

Ottawa, May 28th, 1914

Dear Dr. Kroeber,-

Under another cover I am sending you a copy of the last draft of the phonetic report.<sup>1</sup> Practically all the new features and changes in it are based on recent meetings that I have had with Dr. Boas and Dr. Goddard in New York. You will be pleased to see that small caps have been adopted in the meaning that you give them in Dolores' "Papago Stems", also that Greek characters have been preserved and are specifically recommended for various classes of consonants.

What you will probably least like about the report is the method, that all three of us are favoring, of indicating c-sounds and sibilant affricatives. We threshed both matters out pretty fully and became thoroughly convinced that we are recommending what seems most practical. I do not think that it is advisable to indicate affricatives as simple characters. If you carry out such a method logically, you are soon landed in hopeless complexity. Moreover, after all is said and done, whether affricative consonants are psychologically single acts or not, they are certainly not so analytically.

Where I think we have made the most important progress is in the adoption of symbols for various vowels. There is really no reason why we should not

work on the basis of the well thought out scheme of Sweet's and adapt it to our own purposes. Our present amateurishness on this point does us no credit.

You will, of course, not be frightened by the rather elaborate consonantal scheme that I am appending to the report. Of course you realize that many of these consonants are of hardly more than theoretic value. But it seemed worth while to make the scheme as inclusive as possible, if only for the purpose of showing the general workings of the system, and, incidentally perhaps, of teaching a little phonetics. You may have your doubts as to the existence of some of the consonants pigeon-holed in the scheme, particularly in the case of glottalized spirants and laterals. As a matter of fact though, I have recently convinced myself by hearing Louis Shotrige in Philadelphia, that s, ʃ, x, and ɬ, pronounced with simultaneous glottal closure and subsequent release of glottis, are not only possible, but actually exist in Chilcat.<sup>2</sup>

Dr. Boas suggests that we ought to draw up a set of supplementary suggestions recommending the use of more easily available substitutory characters for such symbols as might not be readily available, say g or p. Would you therefore kindly send me at your earliest opportunity a numbered list referring to the various articles of the report, providing for such alternative characters?

We find that Harrington can not be considered a member of the committee for the simple reason that he is not a member of the American Anthropological Association. However, I shall send him a copy of the report and invite further discussion on his part. His personal preferences, however, will necessarily not have direct weight in forming the policy of the committee. I am very eager that you should agree to all or most of the provisions at present outlined, as I should like to see our committee get somewhere. I should be much obliged to you if you could write me in regard to the report at your earliest opportunity, as there will not be any too much time in which to get everything ready in final shape for the Congress.<sup>3</sup>

Yours very sincerely,

E. Sapir.

P.S. I understood that you are coming east this summer to teach in the summer school at Columbia University. We would be delighted if you could stop over in Ottawa on your way coming or going and stay at our house for a while.

- 1 See Letters 77-93, and Appendix I.
- 2 On April 24, 1914, Sapir worked briefly with Louis Shotridge, the Tlingit speaker with whom Boas worked extensively a few months later. Shotridge was from Kluckwan on the Chilkat River and had been on the staff of the University of Pennsylvania Museum since 1912 (Boas 1917:5).
- 3 The 19th International Congress of Americanists, which was originally scheduled to be held in Washington, D.C., late in 1914. Due to the war in Europe, the Congress was postponed until December 27-31, 1915. The report of the Committee on Phonetic Transcription was published as Boas et al. 1916.

[141]

Affiliated Colleges

San Francisco, Calif., June 8, 1914

Dear Sapir:

I have yours of May 28, but the report has not yet come to hand. I leave for the East in ten days, but if possible will advise concerning the report before my departure. I am afraid Ottawa is entirely out of the question. I shall be able to stop in Chicago, Washington, and Philadelphia going, but coming home shall have to go by the most direct route in order to get back at all for the opening of our fall term. I wish there were a chance for you to drop down to New York this summer.

Waterman and I have just returned from three weeks in his old home with Ishi. We did not try to get much except geography, but, considering the tediousness of this line of work, were very successful. We were also hampered by the fact that he knows a larger tract than we anticipated or were able to cover. We may be able to complete the job some other time, but have enough to serve as a framework for myths and history. Let me know in time if you see any prospect of a trip to California during the coming twelve months. Our

funds have been whittled down a bit for next year and it may be necessary for me to have ample notice if we are to arrange anything. If for either reason this does not prove feasible, I will try to get some texts properly written out and forward them to you. Ishi's English is gradually picking up, but it will be some time before he can attempt even a rudimentary explanation. I tried him on a short text the other day, which he promptly translated into his own language.

With best regards, I am

Sincerely yours,

A. L. Kroeber

[142]

[San Francisco, Calif.]

June 13, 1914

Dear Sapir:

For its kind, I like your phonetic report, though I should have preferred to adhere to my suggestion of a year ago that we make recommendations only on points on which we are all heartily in accord, even though our report came out piecemeal. If the present draft is what the rest of you want, you can put me down as concurring. For a compromise patchwork I think we have done better than would have been expected. I think, however, you should know how I feel about the method implied in the scheme.

What this last draft has particularly impressed on me is the fact that it will be well over the heads of most of the people for whom it is intended. There is no use devising an elaborate orthography for people who cannot hear or produce many of the sounds in question, and who at best have a hazy comprehension of the descriptions of the sounds. The only good that our report would do this element, and I believe it includes the majority of American anthropologists working in linguistics, and all who are not, is that it might call their attention to certain classes of sound formation with which they were unfamiliar. This, however, can be done much more clearly and convincingly

by making the report in the main a description of such sounds and adding orthographies only incidentally, and as lightly stressed recommendations. I am convinced that the report as it stands will be probably intelligible to and usable by only three or four anthropologists in the country besides yourself, and that everyone else, even with the best intentions, will be baffled by most of it--I am not even sure that it will evoke the good will of many of them.

Take for instance your system of vowel transcriptions. If Sweet's classification is the one we want to follow, I believe we should first of all give an exact account of this, either by quoting same in full, or better yet, re-writing it in condensed and improved form, or at least mentioning its fundamental importance and giving an exact reference to the original work and pages in which it will be found. What good are a lot of terms like "mid-back-high" to people who have never given any special attention to phonetics and will not have the inclination to give the subject the necessary time, unless you convince them that it is indispensable? In the same way, referring to a sound in Portugese which they have never heard, and perhaps have no convenient opportunity of hearing, is going to amount to very little. I believe that our present report on the vowels, which evidently was written by yourself, will in actuality amount to a recommendation to two people--one of them being yourself, who will follow the recommendation whether it is made or not; and the other being Harrington, who is going to disagree with you whatever you recommend. I am afraid you have not fully realized the prevailing unfamiliarity with even the principles and fundamentals of phonetics, with the consequence that too much over-refinement and emphasis on the side of transcription, rather than sound formation, will leave the report without appreciable effect.

Take again the matter of glottalized stops. The general nature of these may be sufficiently familiar to most of the public you have in mind, but it is clear that they can take a considerable number of different forms, as Harrington pointed out in one of his notes in the Anthropologist a year or so ago.<sup>1</sup> It is of course impracticable to devise a symbol for each of these varieties, but the time is near when we should begin to take note of these differences. If Harrington is correct in what he says, I would favor quoting him in essentials. If he is in error, you should write a paragraph or two that were better. At



the very least, I favor an exact reference to his article. The most important thing we can do is to make everyone realize that there are glottalized stops, and what they are like. Next most important is impressing on as many of our working anthropologists as possible that there are different kinds of glottalized stops, so that some of them may ultimately bring themselves to a point where they can distinguish them. Third in importance is to have everyone recognize the validity of certain principles in transcribing glottalized stops, such as that in theory at least the apostrophe or exclamation point should come over or under the letter, rather than before or after it. Of last and least consequence is the symbol which they actually use in writing, but this is the point about which most of the discussion in our report now revolves. Even should you think it would take us too far afield to go into the sounds themselves, and should insist on our confining our attention to transcriptions, I should not like the part of the report in question. We recommend, in effect, nothing more than that an apostrophe should be written over the character for the stop, but that if anyone absolutely wishes to follow his old habits, he can use the same character followed by a mark of exclamation. What I should like is a re-statement of the general principle that the only correct thing is to put the diacritical mark over or under the character; that the prevailing tendency is to apply the apostrophe as the sign of glottalization; and the mention, without comment, that the subsequent apostrophe and subsequent exclamation have in the past been more or less employed in America. No one will follow our recommendation merely because we make it. Certainly no one will follow it who has any rooted aversion towards strange and unusual characters, or who is confronted by considerations of typographical simplicity. In short, we cannot, to any appreciable extent, in the near future make them write or print differently from what they are writing and printing, but we can make them realize that no matter what they do actually write, certain orthographies are preferable to others.

If this is true of orthography, it is even truer as regards the importance of sound understanding over sound transcription.

If our final report is as learned and elaborate as its present form, I do not believe it will have any practical influence. If we can bring it down

more to elementary generalities, with the subtleties omitted or conspicuously relegated to a secondary place, I am sure that the work will have some good influence immediately, and a great deal in the end. If you and Goddard and Boas are a unit in favor of presenting the draft which you have just forwarded, I do not wish to stand in the way, and you can put me down as concurring, but I should very much regret this event as a lost opportunity. As chairman of the committee, and as the best equipped man in the subject, I am sure you have the chance of preparing something much more far-reaching.

Sincerely yours,

A. L. Kroeber

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1 Harrington (1912:187) identified at least five phonetically distinct types of glottalized stops.

[143]

Geological Survey

Ottawa, June 23rd, 1914

Dear Dr. Kroeber,-

Your letter of June 13th in regard to my phonetic report is not very encouraging, but I think that you are probably right. What I have begun to think we really need more than a report on sound transcription is a general phonetic manual, which makes special reference to such sounds as are peculiar to Indian languages. Until there is more general familiarity with the phonetic basis which should underlie a consistent and well thought out scheme of transcription, there is relatively little use in providing for the latter. As far as the Bureau alphabet is concerned, there is no doubt that the better informed students of American linguistics are more and more inclined to drift away from it and use other systems which have wider scientific currency. To write a general phonetic manual adapted to the special use of Americanist students would be a rather bold undertaking at the present time, when so much of importance in regard to the phonetics of most Indian languages is still unknown, but

I have an idea that in a few years from now it will be quite feasible to do something useful in that line, even if only in a preliminary way. Such things as glottal stops, glottalized consonants, broken vowels, voiceless vowels, intermediate consonants, strongly aspirated surds, velar resonance, and certain other matters that one could easily think of but which are not generally found treated at all adequately in most of the manuals of phonetics, need to be systematically formulated and illustrated from various American languages. I felt this from the very start, but did not think it was within the scope of a report on transcriptions to go into phonetics as such, but rather tacitly to assume these things.

I have spent a good deal of time on the two drafts of the report, and should not like to have all the labour go to nothing. Perhaps the best thing that might be done at present is to have you and Goddard get together in New York and make up your minds definitely between you as to which of the specific points raised in the report are of too detailed a nature to be of much value to most people. In this way you could make up your minds as to what should be eliminated and what preserved. To a certain extent, I must confess that I believe that it is a good thing for a report of this nature to be a trifle over the heads, as you call it, of most students, for it is precisely owing to that fact that it may act as a sort of stimulant. The great trouble with a meagre and poorly wrought out system is that when one, in the course of field work or otherwise, actually comes across certain sounds not provided for in the system, he is often inclined to make light of it and to assimilate the new sounds to some other stock sounds for which symbols lie ready to hand. I feel convinced that the old Smithsonian alphabet, and for that matter the Jesup system,<sup>1</sup> has done a lot of mischief in just this way. Still, if sugar-coated methods seem to be more effective, I shall be perfectly willing to meet their supporters' more than half way.

As regards the short article on glottalized stops that you credit to Harrington, I may as well tell you that I was much surprised when it came out, as it sounded very much to me like a quotation from a somewhat lengthy letter I had written Harrington on the same subject. In this I quoted from various languages that I had had experience with, illustrating the different types of

glottalized stops. The glottalized stop in which release of the glottal closure is simultaneous with release of the oral closure is, of course, quite distinct from the misnamed "fortis", but I believe is quite characteristic in America. It occurs not only in Shoshonean but also, if I am not mistaken, in several Algonkin languages.

Yours sincerely,

E. Sapir.

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1 The orthography used by Boas and his collaborators in the reports of the Morris K. Jesup Expedition to the Northwest Coast (1896-99).

[144]

Geological Survey

Ottawa, July 13th, 1914

Dear Dr. Kroeber,-

Within a month I expect to finish Part II of my Uto-Aztekan paper, which deals with consonants.<sup>1</sup> There will then be left the third part, dealing with morphology. Here I shall naturally feel the want of adequate material more keenly than ever. I am wondering whether it would not be possible for you to put Sparkman's manuscript grammar of Luiseño<sup>2</sup> in my hands for some time, so that I may get some idea of what Shoshonean morphology looks like outside of Ute and Southern Paiute.

With best wishes,

Yours sincerely,

E. Sapir.

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1 Published as Sapir 1915a.

2 Philip Stedman Sparkman, a storekeeper in San Diego County. He had begun compiling a Luiseño dictionary and grammar around 1900, receiving encouragement from Kroeber, who saw to it that a preliminary sketch of the grammar was published (Sparkman 1905). When Sparkman was murdered in 1907, his manuscripts were turned over to Kroeber. See Kroeber and Grace 1960.

[145]

N.Y. 7/14/14

Dear Sapir:

I will let you know what I can do about Sparkman when I get back to San Francisco. The material is bulky and physically mixed. Sparkman had 3 revisions of his grammar, I added a card catalogue, tablature, notes, etc. I could not well direct from here the selection of what you might want. His orthography is vile; I have rewritten, I think, all his stems and elements, but many only once, in the card catalogue; therefore his typewritten grammar is not in corrected shape ready to use. I suppose a list of morphological elements would not do you any good? I could easily let you have that. I have also a typewritten list of verb stems, I forget whether in preliminary or finally corrected spelling. Did I ever send it to you? In essence the trouble is that you want me to go on the street without my pants on; you want to use a work which is only half done, and is likely to take as long as your Yana morphology to complete. When home I will see how much is in shape to use, advise you, and you can let me know whether any of that will help you. Perhaps I may be able to, and should, prepare a brief sketch for publication, which you could refer to without waiting for its appearance from the press. As the whole job is a fairly lengthy one, and before being pushed much faster will require more work with an Indian on some problems I have already reached since last having a Luiseño, and as at any age I realize more and more keenly the uncertainty of life and the future, such an "esquisse" seems called for, but I hesitate because too much of my work has been of this nature for my own professional good, and because the completion of the "abstract" would, I fear, remove the sharpest spur of conscience impelling me to finish the whole.

Yours,

A. L. Kroeber

[146]

Geological Survey

Ottawa, Sept. 1st, 1914

Dear Dr. Kroeber,-

In pursuance of your recent letter in regard to Sparkman's Luiseño material, I should be much obliged to you if you could let me have whatever morphologic data you conveniently can, even if they do not seem to you to do justice to the subject. Even a list of grammatical elements would be far better than nothing.

In view of the postponement of the International Congress at Washington, there will not be such a great hurry about licking our phonetic report into shape. I have recently had a rather lengthy communication from Dr. Boas in regard to it, and as I agree with most of his strictures, I think there is every likelihood of our all coming to an agreement, and finally licking the report into some kind of shape. I think it could stand a good deal of simplification with profit.

With best wishes,

Yours very sincerely,

E. Sapir.

[147]

Affiliated Colleges

San Francisco, Calif., September 8, 1914

Dear Dr. Sapir:

I have some doubts as to the advisability of furnishing preliminary material for publication, even with the careful and important handling which you will give it. I think this applies with particular force to myself. There are altogether too many direct and implied references in my publications to materials which I have in hand, but which are not yet worked over. However, I will do what I can. As I wrote you from New York, I have a feeling that the

apparent remoteness of the time when I shall be able to bring out the whole of the Sparkman data makes it incumbent on me to bring out a concise sketch covering those points of the language which seem to be certainly established.<sup>1</sup> If I find I can complete such a sketch in the near future, I will at once undertake it, and will then be glad to send you a carbon copy for use, irrespective of the date of publication here.

I have just gone over Seri and Tequistlatecan, and find that Brinton was absolutely right in considering them to be Yuman.<sup>2</sup> They are therefore Hokan, and this family now stretches from Oaxaca to Oregon. I should not be surprised if it were to grow far north and east also, or we may discover new relatives in Mexico. I have gone over Waikuri, but there is so little left that it is impossible to decide anything. We shall have to leave it as distinct until the comparative analysis of Hokan has progressed so far that we are in a position to really match this big body of material against the few inadequate words of Waikuri.

I have just taken stock and find that the eighty-two families given in 1911 on the combined maps of the Handbook,<sup>3</sup> and in the Thomas and Swanton article on Mexico,<sup>4</sup> have already shrunk to sixty-four. I believe it will be a very few years only before we are positively down to half that number. I very much wish you could take a few evenings off and dispose of Beothuk. I happened recently to come across once more what Latham said about its affinity to Algonkin in 1856,<sup>5</sup> and it looks pretty good. It strikes me as a nice thing if you were to dispose of this isolated Atlantic group to balance your Wiyot and Yurok discovery. I should do it myself but you know far more Algonkin, and besides you will be believed and I shall not. I expect that it will be ten years before the majority of our colleagues get over thinking of me as having suddenly developed a streak of craziness in uniting families.

Sincerely yours,

A. L. Kroeber

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1 Never written. A full presentation of Sparkman's Luiseño did not appear until 1960 (Kroeber and Grace 1960).

- 2 Brinton 1891:147-8; see Letter 93, and Kroeber's announcement in Science for Sept. 25, 1914 (Kroeber 1914). A longer study appeared the following year in the UCFAAE series (Kroeber 1915a).
- 3 Boas 1911 (map in pocket). This was a slightly revised version of the map that accompanied Powell 1891.
- 4 A BAE Bulletin (Thomas and Swanton 1911).
- 5 Robert G. Latham (1812-1888), in the Transactions of the Philological Society of London (Latham 1856).

[148]

Geological Survey

Ottawa, Sept. 9th, 1914

Dear Dr. Kroeber,-

You have doubtless read Michelson's criticism of my paper in Wiyot and Yurok.<sup>1</sup> I expect to reply to it before long,<sup>2</sup> and do not think that I shall have any serious trouble in pointing out several weak points in his arguments. He may, of course, be right in discarding a few of the particular comparisons of morphologic elements. Thus, I think he is very likely correct in his point that Yurok -m "thou" is not to be compared to Ojibwa -m "you". His narrowness of outlook, however, is quite apparent, I believe. I am particularly surprised to see that he makes such an excessive use of what I would consider purely negative evidence. He says himself, "It is perfectly true that many of the above objections are negative, that is, that thus far the phenomena listed have not been reported. It is possible that further investigation may reveal some of them, but it is not likely that a skilled investigator like Dr. Kroeber would have overlooked the majority of them." And further on, "Dr. Sapir thinks that as some Yurok adjectives distinguish animate and inanimate, other evidence will show that such a distinction exists elsewhere in the language. If that were the case, Dr. Kroeber probably would have recorded it, as this feature is particularly easy to determine." I attach particularly little importance to Michelson's feeling in this matter, as he has himself never had to approach a language with complete ignorance of its structure and set before himself the



task of working out from the bottom up its fundamental grammatical and phonetic traits. He has never done anything outside of Algonkin and there he found the main structure already completed. For this reason he does not realize how long it often takes to get wind of even elementary points of structure in a new language. He would probably be surprised if I informed him that it was not until I was more than half way through with my Takelma field work that I discovered that certain verb forms that were more or less familiar to me were really passives and not actives, or that when I first took up Ute I did not realize that the 3rd personal pronoun distinguished between visibility and invisibility, a fundamental point that dawned on me only when I supplemented my work later on with Southern Paiute. I speak of all this to you because I feel that it will be necessary for me to make it clear that the Wiyot material published by you is, after all, the result of only a small amount of field work, and that it is quite futile to build upon negative evidence under such circumstances. Would you care to give me a statement as to the total length of time spent by you in gathering your Wiyot data, also as to your own feeling in regard to the argumentative value of such negative evidence as Michelson appeals to?

With best wishes,

Yours very sincerely,

E. Sapir.

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- 1 "Two Alleged Algonquian Languages of California" (Michelson 1914), in the April-June, 1914, number of the American Anthropologist.
  - 2 "Algonkin Languages of California: A Reply" (Sapir 1915c), to which Michelson responded with a "Rejoinder" (Michelson 1915), to which Sapir in turn replied in an "Epilogue" (Sapir 1915d). This exchange was printed in the American Anthropologist for January-March, 1915.

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Affiliated Colleges

San Francisco, Calif., September 15, 1914

Dear Sapir:

Michelson's review strikes me as puritanical.<sup>1</sup> I have never had any doubt of the validity of your union of Wiyot and Yurok with Algonkin. I am afraid I cannot give you an estimate of the amount of time I spent in securing the material I have published on Wiyot and Yurok. My linguistic notes were thoroughly intertwined with ethnological ones. I have spent much time collecting Yurok texts and have an excellent series, but the translations are less satisfactory. Owing to the latter fact, as well as lack of time, I have made no serious attempt to analyze the material, contenting myself for the present with pointing out certain features which came to the surface of themselves. My Wiyot material speaks for itself.<sup>2</sup> I believe I have stated that it represents substantially everything which I have collected. I do not consider the texts very good, nor did I find any satisfactory informant in the time at my disposal. The whole sketch is avowedly a slim preliminary treatment.

If I may advise you in the matter it would be to the effect that I hardly consider it worth while seriously to refute Michelson. His attitude speaks for itself as hypercritical and negative. I believe that if you point this out and content yourself with a renewed note on the utter inadequacy of my Wiyot and Yurok materials, which statement I shall be glad to have you make as strong as you like, public opinion will be better able to judge of the merits of the case than if you enter upon a technical discussion, which will be regarded as a quarrel between experts of whom one is as likely to be right as the other. I take the liberty of making this suggestion, because, while I regard the case in point so one-sided as to be already conclusively settled, I nevertheless feel strongly, as you know, on the general issues and point of view involved, and am therefore anxious to see a question of the kind presented in such shape that

the average anthropologist in this country will be able to form his own opinion.

Sincerely yours,

A. L. Kroeber

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- 1 Kroeber expressed himself directly to Michelson, in a letter dated Oct. 13, 1914 (copy preserved in the Kroeber Papers, Bancroft Library, University of California, Berkeley):

...There is certainly a great deal to be said on your side of the Yurok question, but I am afraid you have no case. As soon as sufficient information is available on this language and Wiyot, you will find the evidence overwhelming. If Sapir has not presented more data, it is not his fault but mine for not collecting or publishing more.

- 2 Kroeber 1911b:384-413.

[150]

Affiliated Colleges

San Francisco, Calif., October 10, 1914

Dear Dr. Sapir:

My Luiseño notes were all out and I had begun on them when I had a chance to have a Northern Paiute from Nevada here, and I shall have to shelve the Luiseño for a-while. I had the informant come down to revise the texts of the late Dr. Marsden.<sup>1</sup> This I fear will prove impossible, as the present informant's pronunciation is quite different from that of the Nevada Northern Paiute we had here some years ago when Marsden was with us. It was on the pronunciation of this former man that Waterman worked out his Northern Paiute paper.<sup>2</sup> My present informant declares that Dick Mahwee belongs to the same dialect as himself, but has come to speak differently. The speech I am now working on is much nearer to Shoshoni and Southern Paiute phonetically. Many final, and some medial vowels, are entirely surd. Of this there was absolutely no trace in Dick Mahwee's speech, nor according to Marsden, in the talk of his Oregon Paiutes. The sporadic e of Dick Mahwee, as in ego, tongue, which I

could never account for, although he spoke the sound plainly, becomes *i* in the mouth of my present informant. Many of Dick Mahwee's *i*'s are *ü* in the present dialect. Most important of all, there are perhaps two organic classes of stops. I have not yet had time to hear enough of the language to describe these accurately, but I am reasonably suspicious of their presence. In short, this branch of Northern Paiute is very much more like Plateau Shoshonean in general than the speech of Dick Mahwee, and according to Marsden, that of the Oregon group seems to be worn down and simplified. We certainly worked very hard both with the ear and with the machine on the question of Dick Mahwee's stops. It may be that he really spoke two classes and we missed them, but if he did, they are unusually alike.

In this connection, I am interested to learn from Mason that he and you have proved by comparisons the existence of two sets of stops in Papago.<sup>3</sup> There also the difference is not very great; in fact Dolores and I did not discover it until his work was done. I think if I ever have opportunity to put him back to work he will have no difficulty discriminating between the two sets.

Your S. P. law of accentuation according to morae, and of surdness as per accentuation,<sup>4</sup> holds partly true in this form of N. P. Thus *tā'ma*, tooth, but *patsa'*, kill. You see however that in dissyllabic words the second vowel does not here become voiceless, nor do all final vowels lose voice. There are some such: *pū'pI*, blood, *hū'pI*, wood; but cf. *tūpi'*, rock, and *ta'ma*, not *tā'mA*, tooth.

Here are other points:

	<i>kuma'</i>	husband
	<i>ü'kuma'</i>	your husband
	<i>okuma'</i>	her husband
but:	<i>i'guma'</i>	my husband

so: *sūda'*, bad (d with brief occlusion, almost untrilled r), but *sūta'yü*

Accent is apparently not drawn from its original place in stem (except by reduplication), no matter what prefixes or suffixes are used.

I wish the consonantal portions of your S. P.-Nahua paper<sup>5</sup> were out. Will you not let me have a summarization of the rules of S. P. as to (1) organic classes of stops, and (2) modification of these according to (a) accentuation, (b) length, (c) sonancy, of adjacent vowels? I should be greatly obliged. I really have no business to be bothering with this matter at all just now; but with a good informant, who even if he cannot improve the Marsden texts, can learn to write good ones of his own, it is a pity to miss the chance; and anything you can do to help me give him, as soon as possible, a correct orthography, will greatly relieve me and enable me to get back so much earlier to Luiseño, which will be my reciprocation.

Sincerely yours,

A. L. Kroeber

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1 See Letter 130.

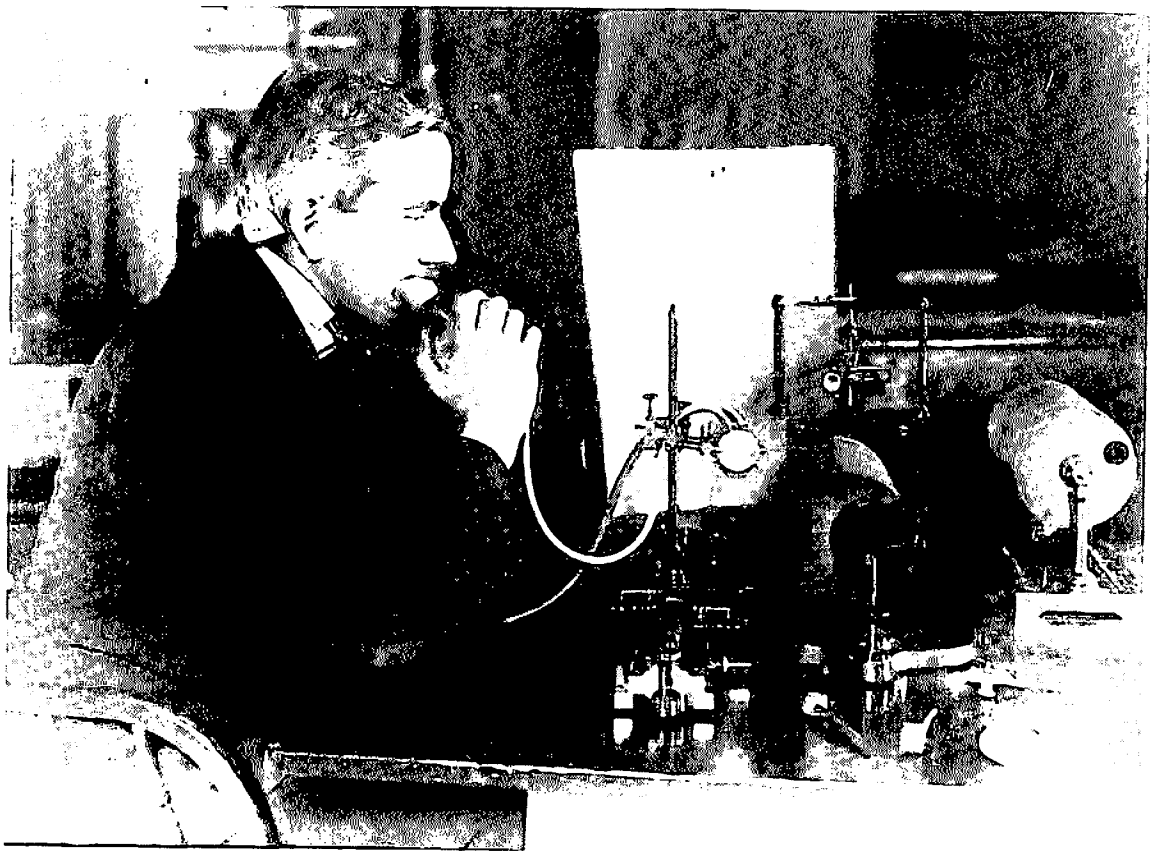
2 Waterman 1911; see Letters 73-4.

3 After taking his Ph.D. under Kroeber in 1911, Mason worked for two years in Mexico and there continued his Uto-Aztecan linguistic work (begun while a student of Sapir's) with a study of Tepecano (Mason 1917). Although spoken in Jalisco, hundreds of miles to the south of Pima and Papago, Tepecano is a language of the Piman subgroup.

4 Described in Sapir 1913a (385-6):

....Every short vowel counts for one mora, or unit of length. Every long vowel or diphthong counts for two morae.... The second mora normally bears the main stress.... All final vowels lose their voice... [and] all vowels standing in weak [unaccented] syllables (or rather morae) and immediately followed by a long (geminated) stopped consonant or by s or š (which also are normally long after vowels) become voiceless.

5 "Southern Paiute and Nahuatl: A Study in Uto-Aztecan, Part II" (Sapir 1915a).



T. T. Waterman demonstrating the Rousselot apparatus,  
Berkeley, about 1914

(Courtesy of the Lowie Museum of Anthropology.)

[151]

Geological Survey

Ottawa, Oct. 19th, 1914

Dear Dr. Kroeber,-

I have your letter of October 10th in regard to Northern Paiute. Under another (registered) cover I am sending you Part II of my Uto-Aztekan paper<sup>1</sup> which was sent to Paris quite some time ago, but I have not yet received word as to its arrival. There seems small chance of anything being done about it in the present state of affairs. Since I finished Part II, I have had some correspondence with Mason relative to Papago and Tepecano, in which some points have come to light that would make it necessary for me to revise several passages in the paper.

As regards the two series of stops in Papago and Tepecano, the matter is clear enough, but is not after all of great significance for Uto-Aztekan. The point simply is that original Uto-Aztekan kw, w, and y became respectively b, g and d. These three consonants are apparently in some positions sonant, in others intermediate. Dolores has evidently confused them with original p, k and t respectively. Inasmuch as original kw, w and y are preserved as such in Southern Paiute and, generally speaking, in Plateau Shoshonean, the point raised in Papago and Tepecano is of purely dialectic interest. As to Southern Paiute, you will see from my paper that there are three types of stopped consonants or their developments, aside from aspirated (or voiceless) and unaspirated (or voiced). One of these, the nasalized set, is demonstrably of secondary origin. The geminated series, which occurs after vowels, as sharply distinct from the ungeminated or spirantized series, is quite obscure in origin as yet. It is clear from Waterman's sketch<sup>2</sup> that they exist also in Northern Paiute. So far I have not succeeded in definitely connecting the geminated stops with anything distinctive in Piman or Nahuatl.

When you are through with the copy of my paper, I shall be much obliged to you if you could return it, as there is every reason to fear that the original

copy sent to Paris may have gone astray. I shall be very glad to get whatever Luiseño material you can let me have.

With best wishes,

Yours sincerely,

E. Sapir.

P.S. I am getting to be a real paterfamilias. My second is a daughter, who arrived Oct. 1st.<sup>3</sup>

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1 Sapir 1915a.

2 Waterman 1911.

3 Helen Sapir.

[152]

Affiliated Colleges

San Francisco, Calif., October 27, 1914

Dear Sapir:

I will return your Southern Paiute paper under separate cover as soon as I shall have been able to extract the memoranda I need. It is a mighty good piece of work, and I am delighted to have had the chance to use it.

I am particularly pleased that it corroborates the conclusion to which I had come while our correspondence was on the way. What at first had seemed to me to be two classes of stops are only one, but a stop in a syllable which is followed by a lengthened consonant seems to be affected by its position, and to be pronounced with somewhat more aspiration. The other point that bothered me was when the stops in medial position were lengthened and when they became sonants, or as in your dialect, spirants. I assumed that there must be some obvious rule covering this point, and broke my head trying to connect the phenomenon with accents, length of vowel, loss of voice of vowel, nearby consonants, etc., but without success. Now that I know that you with greater experience in your dialect have not been able to formulate a rule, I shall not feel so cheap to leave the matter alone. My present guess would be that the



vowels are of two classes, aspirated and unaspirated, and that the length is a product of the former quality in the preceding vowel. Do you think this is possible? I could plainly hear a marked aspiration as the first part of lengthened s in Northern Paiute. At any rate I agree with you that the phenomenon is a fundamental one.<sup>1</sup>

As to Luiseño, will you let me know what you would particularly like? I have thought of preparing a grammatical outline, but it occurs to me that a list of stems or common words might be of more service.

Sincerely yours,

A. L. Kroeber

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1 Sapir's eventual explanation of these consonant alternations in Southern Paiute (1930-31:62-3; also 1933:48-52) has become a phonological classic. See Chomsky and Halle 1968:344-9.

[153]

[no date--apparently San Francisco,  
late October, 1914]

Dear Sapir:

When you issue Part II of your S.P.-U.A.,<sup>1</sup> will you send a copy for Dolores? It will encourage him to keep on. I have never been able to employ him regularly, have had nothing for him for a year, and no opening in sight. His texts are excellent and he may be able to handle the translations and grammar. I want very much to keep his courage up.

My S. Calif. etc. b, d, g in "Shosh. Dialects"<sup>2</sup> of course stand for "intermediates". I did not then realize the fact, and was merely trying to distinguish from English p, t, k, and never implied they were fully sonant.

Working with N.P.<sup>3</sup> has reinforced old sentiments as to orthography. (·) for length makes trouble in ms. and (: ) more. I have my informant using a standard typewriter. He wrote u for both u and ü, only occasionally adding an umlaut later. I was forced to give him e for ü--which has at least historical justification. So too he omitted gemination; fortunately position

will distinguish p and p' & p:, so one character will do. Diacritical marks are easy for you; they would ruin his work, as they did Marsden's. Both of them tend to give ñ (= ŋ) as n. Cost prevents a new set of characters for each worker. Capitals for surds, on the other hand, are easily mastered. Exactness is fine where you can get it, but with most men, zu scharf schneidet nicht. When you come to work more through untrained or half-trained hands, you will value simplicity more highly than now, and it is precisely they and not you that need a key or system.

A. L. K.

- 1 "Southern Paiute and Nahuatl: A Study in Uto-Aztekan, Part II" (Sapir 1915a).
- 2 "Notes on Shoshonean Dialects of Southern California" (Kroeber 1909a).
- 3 Northern Paiute.

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Geological Survey

Ottawa, November 2nd, 1914.

Dear Dr. Kroeber,-

I have your letter of October 27th, and I am very glad to learn that you found Part II of my Uto-Aztekan paper of some use to you. The kind of material that I should like in regard to Luiseño is grammatical rather than lexical, as I want it to serve as material for part III of my paper. If you could let me have a list of grammatical elements, I should be much obliged.

I should be very glad to send you an extra copy of Part II whenever it appears. However, there is no immediate prospect of anything happening, as I have not yet obtained galley. If this present uncertainty keeps up much longer, I may finally decide to send Part II to the American Anthropologist and have them finish the series.

Yours sincerely,

E. Sapir.

[155]

Pacific Division  
American Association for the Advancement  
of Science  
Department of Anthropology, University of  
California  
November 24, 1914

Dear Dr. Sapir:

In conformity with the general plan for the meeting of the American Association in San Francisco during the first week next August, the committee in charge of the anthropological half of the program of Section H, the Anthropological Association and the Folk-Lore Society, has decided to attempt to relate contributions as closely as possible to problems of the Pacific Area-- Eastern Asia, Western America, Australia, and Oceania. Three sessions have been planned, to be devoted to the following subjects:

1. Race in the Pacific Area, with Special Reference to the Origin of the American Indians:

Antiquity of Human Occupation  
Racial Relationships and Descent  
The Influence of Environment  
Origin and Changes of Type

2. The History of Civilization in the Pacific Area, with Special Reference to Relations between Asia and the New World:

Sources and Diffusion of Cultures  
Effect of Environment  
Correlations in Time

3. The Social Aspects of Race Factors in the Pacific Area:

Mixture and its Significance  
Movements of Population as Causes of Cultural Changes  
Race Differences and Values

The Committee is pleased to invite you to present a paper of somewhat more than the customary, say thirty to forty minutes, on the topic "Correlations in Time" at the second of the above sessions.

It is not desired to limit the freedom of choice of contributors, and papers of usual length on any subject will be welcome. But the Committee feels

it to be desirable that on a more or less special occasion such as is furnished by this meeting, there should be at least a ground work of coherently directed program, and would therefore welcome your disposition to take part as suggested.

An early reply, even if only tentative, will be much appreciated, since it is hoped to present a preliminary outline of the program at the winter meetings next month.<sup>1</sup>

Very Sincerely,

A. L. Kroeber

Chairman.

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1 For the program of this meeting, and abstracts of papers presented, see Science 42:541-6 (Oct. 15, 1915).

[156]

Geological Survey

Ottawa, December 2nd, 1914

Dear Dr. Kroeber,-

In reply to your flattering request of November 24th that I take part in the special program that has been planned for the August meeting of the American Anthropological Association, I may say that while not necessarily indisposed to take part in it, I have difficulty in gaining a clear idea of just what is expected from a paper on "Correlations in Time". I find this title about the most difficult to grasp of all that you have listed. Before, therefore, I definitely accept or refuse to undertake the preparation of a paper, I should be much obliged to you if you could tell me in some detail just what the committee had in mind in selecting this subject. I have, of course, a vague idea of what it might mean, but would prefer to hear further from you before committing myself.

With best wishes,

Yours sincerely,

E. Sapir.

[157]

## Affiliated Colleges

San Francisco, Calif., December 7, 1914

Dear Sapir:

The title is not a happy one, but I wanted something broad enough to allow of wide range. What we had in mind was an attempt to grapple with the accumulating but unorganized evidence on the time element in the history of the American race and civilization. In Asia, as well as in Oceania, there are historical records and more or less trustworthy traditions running back some distance. On the American side we have now reached a pretty thorough understanding of the several local types of culture and their interrelations. This may make possible an attempt to estimate the length of time involved in these local developments, as well as in such general cultural traits as may be specifically American. These results either might or might not then be connectable with the determinations for the other side of the Pacific. There are dozens of considerations that might be brought in, such as the probable date of the closing of the quaternary, the indicated antiquity of Maya civilization as a local development, a problem of cultural connection between Peru and Polynesia, the rate of racial differentiation, the period at which the civilization of East Asia took shape, possible indications of an earlier and more general culture underlying the recent provincial cultures of America, etc. etc.

I am afraid that multiplying examples may obscure rather than clear up, so will state that I was responsible for the topic being adopted by the Committee; and what I had in mind was any treatment of the ethnological material in question which would emphasize its historical bearings more than has been customary. We in this country have been particularly remiss and unimaginative. The average American anthropologist, even if he is an archaeologist, treats his data with the most scrupulous reference to geography, but with an almost punctilious avoidance of the factor of time. It seemed to me that you and Laufer<sup>1</sup> were the two men specifically qualified to break through our tradition and inject a new point of view. I am sure that the Committee would not want to have you feel yourself in any way limited, but they did agree with me not

that you were better able to handle this problem than other problems but that you would handle it better than most other men would be able to.

Sincerely yours,

A. L. Kroeber

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- 1 Berthold Laufer (1874-1934), Sinologist and anthropologist. Trained in oriental languages at Berlin and Leipzig, Laufer came to the United States shortly after the turn of the century, and from 1908 was associated with the Field Museum of Natural History in Chicago. He gained considerable repute as a field anthropologist after several expeditions to China and Tibet, and after 1915 was Curator of Anthropology at the Field Museum. (See Hummel 1936).

[158]

Geological Survey

Ottawa, December 14th, 1914

Dear Dr. Kroeber,-

I have your letter of December 7th I regard to the title of the paper that you wish me to prepare for you. I am not at all certain that I shall be able to come, as, if I do, it will have to be at Government expense, and economy is evidently the slogan around here for next fiscal year. Should I decide to write on the subject, I presume that it will be possible to forward the paper and have it read for me. I have read the paper of Wissler's that you referred to<sup>1</sup> with very great interest, and think it is one of the most suggestive things that have appeared in our midst for some little time. I think you are right in your remark about our timidity in grappling with the time element in the history of culture. I think it is historically due to the fact that so many of our men are trained rather in descriptive or psychological comparison rather than in strictly historical comparative work. We do not, for instance, most of us fully realize the importance of eliminating many ethnological and linguistic features, even if extraordinarily complex, when it can be shown that they are probably of quite secondary origin. In relating one people or area to another, for instance, from the point of view of historical reconstruction,

what good is it to use a feature or group of features which are demonstrably of secondary origin? Pointing out analogies is not in itself reconstructing history.

However, I am not at all certain that I shall be prepared to tackle the very important and difficult problems that you refer to. I think that you are perhaps inclined to overemphasize the matter of absolute time. In the absence of the necessary preliminary work in America in marking out culture strata relatively to each other, it seems hopeless to correlate types or elements of culture in Asia or Oceania. It seems to me that such a paper as you ask for could hardly be expected to do more than open up the general question of time perspective in American ethnology, with illustrations here and there based on our specific ethnographic information. I think that one of the most important things to realize at present is that all our culture areas, despite the fact that we, in supposed contrast to many English ethnologists, pride ourselves on a historical sense, are, to a large extent, after all, of a purely descriptive rather than historical value. It is perfectly true, for instance, that a characteristic element in Plains culture is dependence on the buffalo, and that this fact has given a characteristic tone to all of Plains culture. Suppose, however, one demonstrates that, relatively to some other comparatively less important feature, such as type of house or even type of game, this highly important feature is chronologically secondary, then what becomes of it as a historic criterion? Looked at from a purely descriptive or psychological standpoint, it would be perfectly just to emphasize it, and from that point of view it might be shown to differentiate Plains culture more markedly than anything else from neighbouring cultures. If there were reason, however, for believing that some type of house or other lesser feature connected the Plains Indians with some neighbouring group with which it is not customary to associate our Plains Indians, would it not be absolutely necessary, from the point of view of reconstructive history, to entirely ignore the whole buffalo culture with all that it involves, and to lay stress on such apparently lesser ethnological features as criteria of cultural classification?

This type of reasoning is, of course, perfectly familiar to us in linguistic work. In comparing various Indo-germanic languages among themselves,

we are not moved entirely by consideration of descriptive resemblances and differences, but we evaluate these historically at every step. In English, for instance, we have practically lost grammatical gender. This, however important it may seem from a purely descriptive or psychological standpoint, is really not of the slightest importance in assigning English its proper historic place. The fact, for instance, that Armenian is also an Indo-germanic language and has also lost grammatical gender does not move us in the slightest degree. We have plenty of evidence, it so happens, to see that this loss of gender in English is quite a secondary feature, and that we have every right for this reason to ignore it absolutely, and in making out our genetic scheme we just calmly assume that English has a fully developed system of grammatical gender, or if it has not, that it might as well have.

The elimination that I refer to works, of course, not only in the evaluation of negative features, but even more in that of positive features. A very simple example will occur to you immediately. The fact that English is flooded with words of Latin and French origin does not influence us in the slightest in assigning English its historical place. Knowing that these words came in subsequently to the time at which English had already exhibited its Germanic characteristics, we just simply ignore all these loan words as if they never existed. This wholesale elimination of important descriptive features is something that, of course, requires a good deal of tact and insight, and I do not think that it would be at all easy or perhaps even possible to do it in American ethnology today. It is of course always more easy to do this in linguistics than in culture anyway. However, I think that the methodological necessity of such ruthless elimination should be made clearly manifest. There are, of course, other matters of theoretic interest that should be taken up, but you can see from my remarks just what my present attitude towards your topic is. I am afraid it would have to be largely first principles rather than specific applications, though perhaps by the time that the paper is ready for presentation, a number of such applications that are of value may have occurred to me. Please let me know whether the type of discussion that I



refer to is in line with the idea that you had in mind in making up the program.

With best wishes,

Yours very sincerely,

E. Sapir.

- 
- 1 Clark Wissler, "Influence of the Horse in the Development of Plains Culture" (1914). At first a student of psychology in which he received his Ph.D. from Columbia in 1901, Wissler (1870-1947) came under the influence of Boas about 1903 and soon switched to anthropology. His career was centered on the American Museum of Natural History, where in 1906 he succeeded Boas-- who had resigned in a dispute over research priorities--as Curator of Ethnology. Wissler's fieldwork concentrated on northern Plains groups, but his 1917 monograph, The American Indian, was the definitive statement of general North American ethnology for the period. See Murdock 1948; Lowie 1949; and Freed and Freed 1983.

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Affiliated Colleges

San Francisco, Calif., December 22, 1914

Dear Dr. Sapir:

Your scheme of treatment, as per your letter of the 14th, answers exactly what I had in mind. You will understand of course that we did not wish to prescribe but were only endeavoring to secure papers which would make a coherent program. If it is as yet too early to attempt actual correlations in time, there is all the more need of indicating the problems of approach that must first be solved, and the methods which it will be necessary to follow. I hope you will prepare your paper and that you may be able to attend the meeting.

Sincerely yours,

A. L. Kroeber

[160]

Geological Survey

Ottawa, January 8th, 1915

Dear Dr. Kroeber,-

In reply to your letter of December 22nd, I may say that I am willing to commit myself to writing the paper that you have been so kind as to ask me to prepare. I do this with considerable misgiving, but I shall make the effort nevertheless. I can not, however, promise to be present at the meeting, as that depends on whether or not the authorities here are willing to pay my travelling expenses.

Yours sincerely,

E. Sapir.

[161]

Geological Survey

Ottawa, February 3rd, 1915

Dear Dr. Kroeber,-

Can you find out for me what "salmon flies" are? I obtained the term when working in California, and it may be of purely local use in California. It seems to refer to a kind of small crustacean. As far as I can make out they are washed ashore and remain stuck on willows. They are then gathered by the Indians and cooked for food.

Yours sincerely,

E. Sapir.

[162]

Affiliated Colleges

San Francisco, Calif., February 9, 1915

Dear Sapir:

A salmon fly is an insect. I saw many of them when with Ishi last May. It is a large animal with a body perhaps two inches or more long of a pale salmon or yellowish color, with large loose transparent wings. They fly over the streams apparently to deposit their eggs, and are excellent bait for trout. I do not know whether they derive their name from their color or from the fact that the salmon feed on them. The general appearance suggests that they belong to the same order as the May flies, or one-day flies. If the matter is of much consequence I can probably obtain the specific name for you without much trouble.

Sincerely yours,

A. L. Kroeber

[163]

Geological Survey

Ottawa, February 18th, 1915

Dear Dr. Kroeber,-

Thank you for your letter of February 9th in regard to salmon flies. I am glad that I asked you, as I did not know it was actually a large insect. The reference as I originally worded it in my paper, in course of preparation, was as follows: "A species of small crustacean popularly known as "salmon fly" (tc'í'na) is washed in great numbers from the river on to the willows along the bank ('ilau- "salmon flies are washed on to shore"), on which they remain stuck; when found on willows, they are gathered, cooked, and used as food." Of course this will have to be corrected in accordance with your letter. I do not quite understand, however, whether the insects themselves,

or perhaps more likely the larvae, were used for food. Perhaps you can get corroborative testimony from Ishi.

With best wishes,

Yours sincerely,

E. Sapir.

[164]

Affiliated Colleges

San Francisco, Calif., March 13, 1915

Dear Sapir:

Ishi calls the salmon fly *helmtcen-ti*. The *-ti* is not part of the word, but the stem for "say" which he adds to many words given singly. I cannot get him to pronounce this word without it, so cannot be sure that the word properly is *helmtcena*. It appears to be the larvae that are eaten, as he speaks of getting them in the water, where they cling to the rocks. They seem to have been crushed, salted, and cooked.

Sincerely yours,

A. L. Kroeber

[165]

Geological Survey

Ottawa, March 15th, 1915

Dear Dr. Kroeber,-

You have probably been wondering what has happened to our phonetics committee. As a matter of fact, I put in a third, and I hope final, draft with Boas and Goddard quite a long time ago, but Boas has been so busy that it is not until today that I have heard definitely from him in regard to it. The draft, as I am sending it to you under registered cover, has been definitely approved by Boas and Goddard without reservations of any kind. I hope that

you will be able to follow them in this approval. It is our intention to have the report published either in the American Anthropologist or in the first number of a new linguistic journal that they are talking of inaugurating. Such publication will make the report accessible to all. You may keep for your own reference the copy that I am sending to you. However, I must ask you to return the tables of vowels and consonants, as the technical labour required in preparing these is so great that I did not think it worth while to have four copies made of each. I am therefore sending you the only copy I have for your final approval, should they meet with such.

I have read your recently published paper on Chontal, Seri and Hokan with very great interest.<sup>1</sup> I think there can be no doubt that Chontal and Seri are connected with Hokan, though I would be rather skeptical about the validity of some of the lexical parallels given. Of course, we will be forced to grope about somewhat in the dark until we know more exactly just what the extent of your new Hokan stock is, what its major divisions and subdivisions are, and what phonetic laws operate in specific dialects. The one thing that impresses me as perhaps most characteristic of Hokan is the prevalence of stems of the form vowel plus consonant plus vowel (plus consonant). From this type, however, Pomo (except S. and S.W. dialects) and Yana seem to have diverged, inasmuch as the first vowel seem to be regularly, or generally, lost. (Cf. Yana s'ans'i-, Pomo sima "to sleep" : Shasta itsmi; Yana mal'yu < \*(i)smal'-? Pomo cima "ear" : Mohave ismaly-ka.) As regards Yana, while I am quite favorably disposed towards considering it a member of the Hokan linguistic stock, it is becoming increasingly evident to me that it is a decidedly divergent member of it. I should not be at all surprised if it turned out, for instance, that it was further removed from say Shasta and Yuman than even Seri and Chontal are. I have recently gone over Dixon's Chimariko material<sup>2</sup> with a view to finding further Yana cognates, if possible. I believe I have hit upon several points of interest which I, when I get time, shall let you know about later on. Meanwhile, I am very eager to see your full Hokan evidence published.

I think it is one of the greatest misfortunes for American linguistics that Chimariko could not have been studied far more fully and precisely. As you have noted yourself, it seems to be decidedly more typical than Shasta,

Karok, Yana or Pomo, and I strongly suspect that it and Yuman will turn out to be the Hokan standard-bearers, as it were. If it is at all possible to get anything done at this very late date, I should think that there would be no more pressing task for your department than the rescue of what Chimariko material is still available.

There is one phonetic problem that bothers me at present, and that we shall have to tackle very seriously later on. In Yana we have three organically distinct series of stopped consonants--the intermediates, the aspirated surds, and the glottalized surds. It seems fairly evident that these exist also for Pomo. I am practically certain that this applies also to Chimariko, except that here Dixon's faulty phonetics leave us in considerable doubt as to specific details. From Dixon's rendering of Yana, I know that he is not able easily to distinguish the three series. In particular, he almost never hears glottalized consonants, except in the case of glottalized k; and here, curiously enough, he misinterprets the sound as a velar k and almost regularly writes it q. Thus, his Achomawi haq "two" is, as I happen to know from personal experience in the field, really hak!. I wish to warn you emphatically on this last point, because I believe that many points which would otherwise seem obscure receive light. Thus, his Chimariko word for "stone", qa'a, is probably to be read kla'a, thus agreeing better with Yana klai- "stone".<sup>3</sup> Now the difficulty that I am getting at is that in Yuman, if we may judge from your Mohave and Diegueño reports,<sup>4</sup> there seem to be no aspirated surds and glottalized stops as organically distinct from intermediates or unaspirated surds. Hence, the Northern Hokan (Shasta-Achomawi, Chimariko, Yana, Pomo; how about Esselen and Iskoman?) three series must either have developed under certain phonetic circumstances, that remain to be ascertained, from an originally single series; or there are other factors involved that we do not know yet.

With best wishes,

Yours very sincerely,

E. Sapir.

P.S. How important really accurate phonetics is before we can do much with phonetic laws, may be shown by Dixon's Achomawi ē'isat "ear". I recorded this

as is's'at (s· I then used for s ~ c); evidently Hokan \*isma- assimilated to issa-, cf. Dixon's own Okwanuchu is'sa-wak. Another example of disturbance of true insight due to faulty phonetics: Achomawi i'tsa "teeth" was recorded by me as its!a, which goes far better with Yana k'i-ts·lau-; cf. Dixon's own Atsugewi i'tsau.

- 
- 1 Kroeber 1914 (or possibly 1915a).
  - 2 Dixon 1910.
  - 3 Sapir had made some of these points in his published review of Dixon's Chimariko monograph (Sapir 1911a).
  - 4 "Phonetic Elements of the Mohave Language" (Kroeber 1911a), and "Phonetic Elements of the Diegueño Language" (Kroeber and Harrington 1914).

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Affiliated Colleges

San Francisco, March 20, 1915

Dear Sapir:

I think you ought to let up on Dixon. He doesn't pretend to be anything he isn't. Nobody could have done much more with the Chimariko informants left. Goddard found the woman decrepit, and I know the man was out of his head. They must long since be dead or entirely past use.

The Hokan stops have bothered me, but sufficient unto the day! I may be able to do something during next year, when I'm off. Just now spring is here and I want to forget and be away.

Is there any chance of your combining coming to the August meeting with work with Ishi?

Sincerely yours,

A. L. Kroeber

[167]

Affiliated Colleges

San Francisco, March 20, 1915

Dear Sapir:

The phonetic report came too late in the day for me to return the tables by registered mail. It may be a week before I return to the office and can send them.

Recommendation A is fine, and I subscribe enthusiastically.

"B" I have not had time to go over as thoroughly as it deserves, but you know more about what is involved than the rest of us, so count me in as voting aye. If I differ on any details, I will advise you later.

I note that in "A" there is no recommendation for sonant palatal fricative. Was gamma ( $\gamma$ ) omitted through an oversight? The sound is frequent enough to need being represented.

You have done a good job.

Sincerely,

A. L. Kroeber

[168]

[no date--late March, 1915?]

Dear Dr. Kroeber,

I am getting vastly interested in Hokan. Under registered cover I am sending my preliminary set of N. Yana stems.<sup>1</sup> It is by no means complete and, owing to colorlessness of many initial stems, I may in some cases have combined distinct elements under one stem. Perhaps one or two further Hokan cognates may be suggested to you. When through, please return. Please note that syllabically final m and n become p and t in N. Yana. Hence, stems like k'ip- should be read k'im- for C. and S. Yana (other things being equal, of course). But p<sup>ε</sup> and t<sup>ε</sup> remain as such for C. Yana.



You once asked me, if I remember rightly, if wáwi "house" occurs also as wa. I believe -wi is collective (as in i'wi "firewood", 'is·iwi "men), wa- being thus identical with Mohave ava, Chimar. áwa, a- being dropped in Yana as in ha- "water" < aha- (cf. Mohave and Pomo). Yana -wa occurs also, e.g. mát!adju-wa "sweat-house", probably from mát!adju(i)- "to be winter", hence "winter-house".

Yours,

E. Sapir.

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1 This list was one of those that were ultimately published as "The Fundamental Elements of Northern Yana" (Sapir 1922b). See Letter 299.

[169]

[no date--late March, 1915?]

Dear Dr. Kroeber,

Is yufi "nose", which you have recently quoted, Karok?<sup>1</sup> I regret very much you did not indicate under "Other Hokan Languages" which language was referred to in each case. I think Esselen aur (la Pérouse) also belongs here. Frenchmen often hear (or write) x as (uvular) r; read aur as aux (cf. Dieg. -axu).

In recently looking over Hewitt's Seri-Yuman paper,<sup>2</sup> I found what seem to be two more Seri-Hokan correspondences:

Seri tahj "sun" (i.e. tāx) : Pomo -daka (see Barrett)

Seri ko-polt "black" (i.e. -poł; ko- is apparently adjective prefix)  
: Yana p'al- "black"

Yours sincerely,

E. Sapir.

P.S. Am preparing a preliminary paper on Na-dene (Haida, Tlingit, and Athabaskan) for Am. Anthro.,<sup>3</sup> also a supplement to your Hokan stock, which I shall send you before very long!<sup>4</sup>

P.P.S. What the hell does Waterman know about Mexican archaeology? And why such insistence on British failings?<sup>5</sup>

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- 1 In "Serian, Tequistlatecan, and Hokan" (Kroeber 1915a).
  - 2 Hewitt 1898.
  - 3 Sapir 1915b.
  - 4 Apparently "The Hokan and Coahuiltecan Languages" (Sapir 1920a). See Letter 173.
  - 5 A reference to Waterman's review of Thomas A. Joyce, Mexican Archaeology, which appeared in the January-March, 1915, number of the American Anthropologist (Waterman 1915).

[170]

[no date--late March, 1915?]

Dear Dr. Kroeber,

Esselen aur was a lapse on my part. It means "teeth", not "nose". Esselen hocis "nose" (h-oci-s) looks quite Hokan-like; cf. Chim. h-oxu. Esselen c, s : Hokan h, x as in asa-nax "water" < Hokan axa-; ciefe "stone" : Pomo xabe. I have recently had occasion to look into your Chumash material,<sup>1</sup> and begin to feel it is decidedly part of Hokan.<sup>2</sup> Its key seems to be recognition of certain prefixed elements, not only t-, but also n- and l-. With Mohave ime "foot" cf. Chumash t-em, n-ime-l; with Chimariko -oxu "nose" cf. Chumash n-oX(c). With Mohave ammaya "sky" cf. Chumash a-l-apa(ya), Salinan l-èm(o). With Chimariko asi "day", Esselen aci, asi "sun" cf. Chumash a-l-ica, a-l-aca, icau "sun".

Yours,

E.S.

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- 1 The section on Chumash in "The Chumash and Costanoan Languages" (Kroeber 1910c:264-71).
  - 2 J.P. Harrington had already announced the relationship of Yuman and Chumash, although he presented no data (Harrington 1913).

[171]

[no date--late March, 1915?]

Dear Dr. Kroeber,

À propos of Seri wat, Mohave ahwata "blood", cf. Yana wát' duwi "blood". Again a- is lost (was it ever there?). wát' duwi consists of collective -wi with infixed plural -t'- (cf. bambamút' giwi "flies" < stem \*bambamugi), leaving \*wadu- < \*wat- (before w, as regularly) as stem. This analysis is confirmed by derivatives: da-wát'-sa- "brownish red" (color adjectives are formed from stem da- and with suffix -sa- "off, away"; cf. da-tsigai-sa- "blue, green"), wat'wá' i "redbud" (-'i "tree, bush"). For "blood" : "red" cf. terms in Yuman for these given by Hewitt,<sup>1</sup> e.g.

26. a-whăt "blood" : whūt "red"

Incidentally, this shows that Hokan xw (hw) became w in Yana. There are no labialized k-sounds in Yana.

Yours sincerely,

E. Sapir.

P.S. With Pomo tcadīn, tcadū "to see" (see Barrett)<sup>2</sup> cf. Yana djadji- "to (sit) with eyes drooping".

1 Hewitt 1898:334\*.

2 "The Ethno-Geography of the Pomo and Neighboring Indians" (Barrett 1908). The form cited is from a vocabulary on p. 67.

[172]

Affiliated Colleges

San Francisco, 3/30/15

Dear Sapir:

Thanks for your Yana stems. I will return same as soon as I can make a copy. I shall not draw on them for publication without your consent.

I am pleased you think well of the wider Hokan group, and that we weight points much alike. I don't much care if there are obvious omissions and even a few errors in my Seri comparisons. The natural doubters would cavil anyhow. Others will be convinced as to the fact of relationship, and if they want the "how", that is another story for later, or let them work it out themselves.

I am however in a quandary about Hokan and Penutian. In the first flush of conversion from disbelief, I wrote out a complete paper, which I soon supplemented and revised, and which, barring a few lists and loose cards, was even clean-copied, as two extracts enclosed show (ms. pp. 66-68, 44-46, covering some of the points in your recent note; pencil memoranda are recent).

Meanwhile, however--all this was two or more years ago--our ideas developed, the point of view changed, and a lot of the manuscript is already ultra-conservative and wasted. The obvious thing to do was to work out the sound shifts. A few were plain, but to get the rest--I mean enough to give a working basis for fruitful comparisons--is no slight job, and I have not got around to it. The little paper in the *Anthropologist*<sup>1</sup> we put in to gain time and hold our claim. It's not enough to convince anybody, of course, but it seems foolish to publish more without doing the whole job up brown. To issue material of the character of the enclosed tables, would be misleading, as everyone would interpret it as meaning we had nothing better to offer. In these two years of apparent inaction, however, a lot has soaked in; my feeling for the evidence is quite different. I was glad of the opportunity to express this point, and backhandedly help along the Hokan cause in an interim way without doing anything more professedly preliminary, by issuing my Seri paper. Penutian, though a more narrowly framed problem, is equally interesting. I see the germs of some extraordinary developments. But the task of handling the material in the time available is the same. If we choose some of the lines of least resistance, and let it go for the present at bringing out the easy but imperfect material lying along them, people like yourself will be the first to jump us;--cf. p. 2 of your typewritten letter of March 15.<sup>2</sup> Will you promise to commend instead of belaboring us if we make some fraction of the evidence available?

And do stop jumping on Dixon. He does the best he can, and about as well as I--certainly at least as well as I used to. We can't all have your ear, or Goddard's eighteen years of experience with one group.

Waterman fought Mexico till he got interested. He has a paper on day-signs, which only a congestion in our printing office has prevented from appearing to date.<sup>3</sup>

Yours,

A. L. Kroeber

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1 "New Linguistic Families in California" (Dixon and Kroeber 1913b).

2 Letter 165, second paragraph.

3 A reply to the second postscript of Letter 169.

[173]

Geological Survey

Ottawa, April 3rd 1915

Dear Dr. Kroeber,

Under another cover I am sending you the carbon copy of a paper I have recently prepared on Hokan and Coahuiltecan.<sup>1</sup> You might return it when you are through with it, accompanied by your opinion as to whether you consider the demonstration satisfactory.

With best wishes,

Yours very sincerely,

E. Sapir.

P.S. I am afraid you misunderstood the tenor of my remarks regarding Dixon. My object is not to point out Dixon's linguistic shortcomings as such, but merely to give a tip as to how certain of his orthographies were to be understood, so that you might perhaps make better use of Chimariko in comparative

Hokan work. Thus, I believe it to be distinctly helpful to know that Dixon's q really means k!.

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- 1 Published as Sapir 1920a. Swanton (1915) had recently grouped several extinct languages of southern Texas and north-eastern Mexico (Coahuilteco, Comecrudo, Karankawa, Cotoname, and Tonkawa) in a "Coahuiltecan" stock, suggesting a further connection with Atakapa. Sapir now linked all of these to Hokan.

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Geological Survey

Ottawa, April 7th, 1915

Dear Dr. Kroeber,-

Thank you for letting me have access to part of your Hokan manuscript.<sup>1</sup> I have looked this over and made some use of it, also added notes here and there, which I hope you won't mind.

You can make what use you like of my Yana verb stems, drawing on them for publication if you care to. I am now engaged in carding all Yana resemblances to Hokan stems and elements that I can collect, with a view to trying to work out later on just what the status of Yana is from the point of view of phonology.<sup>2</sup> I shall send you these cards later on, so that you may make what use you like of them.

I should like very much to have you and Dixon publish enough of the Hokan material to put the theory on a sounder basis than it is as yet, even if most of the details of sound relationships have not yet been worked out. The material as such ought to be accessible anyway, revised up to date, of course, as much as it can be, though in matters of this kind we inevitably soon become somewhat antiquated. Of course, one real difficulty in the way of establishing the phonetic laws at present is that by far the greater part of the comparative material available is not accurately enough recorded. It is irksome, for instance, in handling a Pomo k not to know whether to take it as an aspirated k, an intermediate k-g, or a glottalized k. By the way, I think it too bad

that you so often simplify original orthographies in your lists. I do not see how you can avoid in this manner unwittingly eliminating those very features that may ultimately turn out to be most valuable in working out phonetic comparisons. Not infrequently one can read behind the original orthography to the true sound value. Another small point that I do not quite like is the indefinite character of some of your references. To give three or four un-specific forms of "Shastan", for instance, does not mean much to any one who wants to try to see what sound processes have taken place in Shasta, Achomawi, and other dialects. This applies also to Pomo. It is quite evident, for instance, that certain specific dialects of Pomo have very definite phonetic peculiarities of their own, as distinct from Pomo generally. I emphatically do not believe that the necessity of cutting down printing expenses is a valid excuse.

I rather feel that there will turn out to be a further intrinsic difficulty in the working out satisfactorily of Hokan phonology, and that is that the Hokan sound system to begin with was probably not a particularly simple one. This means that a much larger mass of material to work with should, sooner or later, be made available than there seems to be now.

With best wishes,

Yours very sincerely,

E. Sapir.

P.S. While it seems rather too much like a caucus understanding for me to promise to "commend" instead of "belaboring",<sup>3</sup> I do not mind expressing myself as favorably disposed to your Hokan hypothesis whenever opportunity presents itself. Indeed, I believe I have made it evident that I work on that theory as a valid one. I would not be inclined to lay stress on shortcomings in estimating the value of a bit of pioneer work.--You asked me some time ago if I could manage to get to San Francisco to work with Ishi. I really don't see, to tell the truth, that there is any reasonable chance of it, particularly as economy is our slogan here just now. I couldn't afford it on my own expenses. I don't see why you or Waterman don't work out S. Yana quite independently of my N. and C. Yana material. Any help I can give at any time I shall be delighted to put at your disposal.

P.P.S. How about Washo? I have a lurking feeling it will turn out to be a very divergent Hokan language! Washo d- as in Chumash and Salinan < Hokan \*da "that, the" (see my Coahuiltecan paper).<sup>4</sup> How about -l as indefinite noun ending (cf. Pomo -l, Chim. -l, -r, Dieg. -ly, Yana -na, Esselen -nax)? d-aja-l < \*d-ama-l(a) looks curiously Hokan-like. I have noted other Hokan points of resemblance. Do you want them?

- 1 See Letter 172.
- 2 This is the genesis of "The Position of Yana in the Hokan Stock" (Sapir 1917a), one of Sapir's most important statements on Hokan.
- 3 A reply to Kroeber's remarks at the end of the fourth paragraph of Letter 172.
- 4 The manuscript that Sapir had recently sent to Kroeber on "Hokan and Coahuiltecan" (Sapir 1920a).

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[Ottawa]

April 15, 1915

Dear Dr. Kroeber,

Did I ever write you I had evidence to show Takelma and Coos are related?<sup>1</sup> Not very closely, however. Tak. is much more synthetic in structure. Coos is quite analytic in type. Now, I begin seriously to suspect both Coos (perhaps eventually also Siuslaw, Alsea, and Kalapuya?) and Tak. are North Penutian, cut loose from S. Penutian by northern intrusion (Shasta-Ach., Chim., Karok, Yana, Pomo) of Hokan languages, which seem to gravitate south. I wonder if you care to look seriously into this matter. Note, e.g., Coos ma "people" : Yokuts mai; Coos yipsEn "three" : Tak. xibini : Yokuts copin, Cost. kapxan; Tak. xi "water" : Cost. ci (also Coos ci "to drink"). One thing I know, and that is that Coos & Tak. are certainly not Hokan. Hokan, as far as I can see at present, stops at Shasta. If you eliminate Athabaskan and Shoshonean, which clearly moved in S. and N.W. respectively, you can almost read California history from linguistic geography. Tak. is doubly isolated. More recently it



was cut loose from its N. Penutian relatives (e.g. Coos) by Ath. intrusion; further back from S. Pen. relatives by Hokan intrusion. No wonder it has developed along very special lines. I incline at present to think that its greater synthetic character was built up on more analytic basis exhibited by Coos and Yokuts. Of course there are tremendous morphological differences between Coos, Tak., and say Yokuts or Maidu, but we must look at these things not with inflexible descriptive eyes but historically. Let me point out at least one very important feature. Coos, Tak., and Yokuts have as one of their most common types of stem:

c v c<sub>1</sub> v -

i.e., disyllabic with repeated vowel. If you look through Frachtenberg's Coos vocabulary<sup>2</sup> you will be struck by immense numbers of noun stems of this type (cf. Tak. noun stems & particularly verb aorists); they look strangely Yokuts-like. Another thing. Tak. has 2 peculiar types of reduplication that have close parallels in Yokuts:

Tak. 1. hegwehagw-

2. t!omom-

Note also ablaut in Tak., Coos & Yokuts. Would you not care to look into this? Maybe I'm wrong. But maybe not! I find this sort of thing so fascinating that it keeps me from more orthodox work.

Yours,

E.S.

P.S. Cf. Yokuts t·üñü- "nose" with Tak. s·in-, Coos tcūñ.

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1 Frachtenberg's grammar of Coos, one of the components of the second volume of Boas's Handbook of American Indian Languages (1922), was issued in paper covers in 1914. As Sapir later wrote: "On the appearance of Frachtenberg's Coos grammar it soon became clear to me that the morphological and lexical resemblances between Takelma and Coos were too numerous and fundamental to be explained away by accident or plausibly accounted for by borrowing" (1921a:58). Frachtenberg, independently, came to the same conclusion, and published a short paper on "Comparative Studies in Takelman, Kalapuyan and Chinookan Lexicography" in 1918, three years before Sapir's paper on wider Penutian relationships ("A Characteristic Penutian Form of Stem", 1921a). Since Frachtenberg seems to have begun his comparative work no earlier than

1916, the real priority for the "Northern Penutian" hypothesis belongs to Sapir. See Letter 233.

2 In his Coos Texts (Frachtenberg 1913).

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Geological Survey

Ottawa, April 21st, 1915

Dear Dr. Kroeber,-

I sent you a little note some time ago in regard to Coos and Takelma. Though I said that I did not feel like looking into the matter myself, I found the idea so fascinating that I could not resist doing something with what seemed a promising line to me. The enclosed 145 resemblances between Coos and Takelma on the one hand and Penutian on the other is the result.<sup>1</sup> It is hard to believe that at least some of these resemblances are without significance. It is particularly gratifying to find such a large number of important grammatical elements among them. See particularly numbers 24, 25, 26, 27, 46, 50, 54, 55, 61, 62, 64, 65, 87, 119, 120, 121, 123, 127, 129, 130, 131, 142, 143, and 145.

One difficulty is, of course, that there are no case suffixes in Takelma, and that there are only two or three local case suffixes in Coos. However, I suspect that the "noun characteristics" that are discussed at length in my Takelma grammar may be specialized case endings, -an being an old possessive, -i and -a being old objectives, and -u being an old locative. If this is correct, the resemblance between Takelma and Yokuts becomes rather striking on this point. I think the resemblance in general feeling between Takelma on the one hand and Yokuts and Miwok on the other is quite striking. I remember distinctly now that when I read your Yokuts grammar years ago, this resemblance struck me. The similarity between the pronominal systems of Takelma and Miwok is certainly remarkable.

I am sending you also a selected list of words from the seven languages involved, illustrating a characteristic type of stem with repeated vowel. The

examples might be very easily multiplied for any of the languages. I believe that types of stem are a very important criterion of relationship, and much more far-reaching and significant than we are apt to imagine.

If you have looked into this matter more fully on your own account, I shall be very glad to have you use my material in any way you may see fit. If you prefer, we could collaborate on a paper which would show that Takelma and Coos are members of the Penutian stock. On the other hand, if you do not care to commit yourself to this new development or have not time to go into it more fully, I shall be willing to assume sole responsibility. Would your University care to publish a paper on the subject in such an event? I may point out incidentally that the material that I am sending you does not by any means include all the evidence at my disposal tending to connect Coos with Takelma. I have included only such material as was of interest for comparison with the Californian languages.

In any event, please let me know what you think of the evidence. With best wishes,

Yours very sincerely,

E. Sapir.

P.S. It is particularly gratifying to me that this last development has cleared up 3 important phonetic problems within Takelma itself. 1st, there is no tc (ts) in Takelma, which seems strange in view of Tak. tc!, which is very common. However, Penutian and Coos show clearly that c, s has become Tak. x (e.g. Tak xà "water" : Cost. si "water", Maidu sē-wi "river"), whereas Tak. c (s) goes back to tc (ts), whether this latter is original or developed from t· (cf. Tak. s·in- "nose" : Yokuts t·üfük). Hence Penutian c : tc becomes Tak. x : c. 2nd, in Tak. c (s) becomes "strengthened" to tc! just as b to p!, g to k!, d to t! (e.g. aorist t!omom- : non-aorist dō<sup>u</sup>m-, like aorist ts·!elel-amd- : non-aorist s·e<sup>e</sup>l-amd-). Evidently older tc : tc!, which is intelligible, has shifted to c : tc!. This, again, confirms our phonetic law of c < tc. 3rd, in Tak. theoretical -tx- always appears as -c- (-s-). This, of course is O.K., as theoretical -tx- was really -t-c- (old c > x) which had to develop to -c-. Everything is now in order. All

this goes to show that even purely descriptive studies may contain many obscure features when not helped out by comparative evidence.

P.P.S. I don't like "Penutian". In view of Cost. ama, Yokuts mai, Maidu mai-, Coos mā, I would suggest "Mai" as stock name. Bother -an! Mai stock would be good enough.

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1 This 14 page manuscript is now in the Franz Boas Collection of the Library of the American Philosophical Society (catalogue no. 2950). In its present state it contains 152 lexical sets, with marginal notes and other indications of having been somewhat reworked after April, 1915. This manuscript was edited and published by Morris Swadesh as "Coos-Takelma-Penutian Comparisons" (Sapir and Swadesh 1953.)

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Affiliated Colleges

San Francisco, Calif., April 26, 1915

Dear Sapir:

For weeks I have been waiting for an answer from you to a letter which in consulting my files I now find was never sent. This is the proposition: Can and will you combine attendance at the August meeting here<sup>1</sup> with a stay of four, six, or ten weeks working on Southern Yana? If so, please advise me what time you would have available and what you think the cost would come to. I shall know in a couple of weeks what our allowances for the next fiscal year will be, so that if anything mutually feasible is in sight, we shall be able to arrange for it before my departure for Zuñi and the East on May 19.

I am anxious to have you get hold of Ishi as soon as possible. About Christmas he was ill and several of the men in the hospital diagnosed him as tuberculous. Those who denied were apparently right, for he has come around into good shape. But the affair gave me some alarm, and the moral is to get

from him what we can while he is well instead of trusting that he will last indefinitely.

Sincerely,

A. L. Kroeber

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1 The special summer meeting of the American Anthropological Association and the AAAS, for which Sapir had already agreed to prepare a paper. See Letter 155.

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[Telegram]

May 6, 1915

Dr. A. L. Kroeber  
Affiliated Colleges  
San Francisco, Cal.

Can get leave of absence for three months including trip. Would be willing to accept your offer of April 26th on payment of \$400 out of which I would pay all my expenses. Could probably leave June, certainly July. Please answer by wire if this is suitable.

Sapir.

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[Telegram]

5/7/15

Dr. E. Sapir

Our budget will not be adopted until next week but if we receive same appropriations as this year we shall be able and glad to make arrangement outlined and I do not expect cut in appropriation. Will wire as soon as official.

A. L. Kroeber

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[Telegram]

San Francisco Calif May 13-15

Dr E Sapir

Ottawa Ont

Clerical discrepancy renders budget uncertain hope advise you few days

Kroeber

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[Telegram]

San Francisco, Calif. May 15th-1915

Dr E Sapir,

Geological Survey, Ottawa-

Everything arranged, we accept proposition as outlined letter follows.

Kroeber

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Affiliated Colleges

San Francisco, Calif., May 17, 1915

Dear Sapir:

As I have already wired you, we shall be able to make the arrangement which you outlined in your recent telegram. I am very much pleased. You will find Ishi bursting with mythological, ethnological, tribal, and geographical information, which he is delighted to impart, but he may need a little training before he will dictate connected texts slowly enough for writing. All that we have satisfactorily got of this sort has been through the wearisome process of having him repeat, word by word, speeches previously delivered into the phonograph.

I presume you will want to stay in Berkeley, possibly at the Faculty Club. In the latter case, you may find the enclosed card of interest, and I would suggest your asking Waterman to make the necessary arrangements.

Please also advise me of your intentions on this point. If you wish to live and do your work in Berkeley, there will be a number of little problems to meet before we can have Ishi at your daily disposal. They do not amount to much, but some of them are of an administrative nature and will have to be given attention before your arrival.

We can pay you the four hundred dollars in either of two ways: First, we can secure an advance, probably not of the whole amount, but in two or three installments, and keep you supplied as the work progresses. This would entail your submitting an itemized statement, with vouchers, for expenses aggregating four hundred dollars, and the expenditures would have to be according to the rules of our business office. The other way would be substantially that we contract with you for three months' services at the rate of one hundred and thirty three dollars and thirty three cents monthly, payable at the end of each month. In that case you would be exempt from accounts and vouchers. Please advise me as soon as possible which plan you prefer to follow, since I leave the last of the month.

I may add that our fiscal year begins on July 1 but that if you should wish to follow the first of the above plans we could have a bill for an advance put in in June and check ready for you within a few days after the first of July. I have also on hand a balance of about seventy-five or one hundred dollars which I could turn over to you under this first plan, provided you left Ottawa before our new fiscal year begins on July 1.

Wallis<sup>1</sup> is to take my place here next year, and we have a fellowship which I hope Mason will accept.<sup>2</sup> Between these accessions to our staff, the meeting in August, and visitors to the Exposition at odd times, you will not be lacking in anthropological contact while in California.

I have deferred answering your letters about linguistic relationships until I should have time to think things over in quiet. I have had much to

attend to in anticipation of being away, but will write you fully before I leave.

Sincerely yours,

A. L. Kroeber

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1 W.D. Wallis. See Letter 81, FN.

2 J. Alden Mason did accept this fellowship, and spent the calendar year 1916 in California.

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Affiliated Colleges

San Francisco, Calif., May 18, 1915

Dear Sapir:

If you can send me a copy of one of your reprints of your "Southern Paiute and Nahuatl: Part II," from the January 1915 number of the Anthropologist, I shall be greatly obliged.

Sincerely yours,

A. L. Kroeber

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[Telegram]

May 25th, 1915

A. L. Kroeber

Affiliated Colleges,

San Francisco, California

Would prefer second of two plans of payment outlined in your letter of May 17th. Traveling time must be included in three months for which salary is paid. Wire me if this is O.K. Will probably leave about June 15th with



family. Would prefer to work in Berkeley. Am writing Waterman about securing accomodations.

Sapir.

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[Telegram]

5/27/15

Dr. E. Sapir

Arrangement fine and stands. Will provide for payment of one third of amount to you middle of July, August and September. Good luck to you and a pleasant time. Wish I could be with you.

A. L. Kroeber

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Affiliated Colleges

San Francisco, Calif., May 29, 1915

Dear Sapir:

For lack of time due to preparation for my year's get-away, I have not been able to go over your evidence as to new Hokan and Penutian affinities with the care that I should like to give your material. I am, however, entirely confident that you have sufficient proof on each point.

I should like nothing better than to collaborate with you on a paper connecting Coos and Takelma with Penutian, but it is out of the question. I shall not have the time for six months, and could not promise then. I feel that in any event the evidence for Penutian should be brought out first. No one is more keenly aware than I that however good our case is in this matter, we have not yet submitted the proof. I am sending my notes East, and hope to get at the job there.

I should be very glad to have you keep in touch with me as to any progress you are making, so that we may avoid duplication of efforts or partial repetition of matter, as in the case of Yurok and Wiyot. I shall advise you as soon as Dixon and I are at work again.

I should be delighted if we could have a paper from you on either of these two subjects (viz., Coahuiltecan and Coos-Takelma), as you suggest, but the rules of our Editorial Committee provide that the University can publish only work of members of the University, or papers relating to material belonging to the University. The interpretation of the rules, however, is liberal, and it is possible that there is some way around. I would suggest your discussing the situation with Waterman this summer.

I should like to hear from you what you propose to do in regard to names for the enlarging families. I should like to arrive at an understanding with you as to a definite policy to be pursued, not only now but in future. The point is not altogether without importance, as wrong names will lead to ever recurring confusion. My Hokan and Penutian, although arbitrarily coined, had perfect definiteness in their favor, but you have now brought on a new status under which the terms might be used in two senses.

I am thoroughly satisfied that our arrangement for the summer has been consummated. I only regret that I shall be away. Waterman will look out for anything that you wish. I leave the day after to-morrow for six to eight weeks at Zúñi, and then go on to New York. I hope your summer will be as pleasant as I know it will be profitable. Please remember me to Mrs. Sapir. I am hoping that my opportunity to become acquainted with her, as well as the two newcomers in your family, is only deferred.

Sincerely yours,

A. L. Kroeber

P.S. I am returning your two manuscripts under separate cover.

[The following handwritten note, on smaller sheets of stationery, is apparently a postscript to the preceding letter.]

As to S. Yana, Waterman and I have agreed to call the group by their own name for person, "Yahi", at least on title pages. I hope you join in.

I have some pretty analyses of music, ms. nearly completed; I may be able to have this sent to N.Y. next winter and finish it up. Waterman has some first rate geography. I have turned over to him my data, with map. All else we have is scattering and fragmentary, except possibly for some 50 or 100 pages of text of the Duck myth that W. wrote out from the Indian's own repronunciation of a phonograph dictation in the first days he was with us.<sup>1</sup> The text thus is straight Yana, but Batwi's English to it is nonsense.

There is a pile of good ethnogeography to be got out of Ishi, as you will see on looking over the data in W.'s care. I have talked with him so frequently about other tribes that he is interested, and ready to be pumped all day. I hope you get some of this information in text; I don't know enough of the language to have got all the points he has given me. He knows some Maidu, Wintun, and Hat Creek words, and some geography.

I'm quite depressed at not being able to keep up with the developments in linguistic relationship. I'm not very fast at work of that kind, my memory being limited, but my real trouble is being loaded up with too many other things. I can do many things in spare moments, but not these comparisons. Well, I'll do the California end of Hokan and Penutian one of these days; and if I haven't the fun of having a hand in anything else, I'll at least know you'll work it out more thoroughly and more accurately. My only equipment is feeling and perhaps a sense of judgment; the fine analysis fascinates me, but I'm too slow and clumsy to attain it in practice.

I wish you'd line up Beothuk with Algonkin. The case is clear, and with a day's work you would establish it. It would take me a week or two of mulling around with Algonkin dictionaries.

Arapaho is off my liver--in press here, or at least in cold storage for the press--such as it is: one of my scrappy papers.<sup>2</sup> I've got just far enough to see the tremendous interest of Algonkin, and to define some problems--which I hope you'll answer, if Michelson doesn't.

A.L.K.

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1 These wax cylinder recordings (and many others of a similar nature) are preserved in the Lowie Museum of Anthropology, University of California,

Berkeley. They were transferred to magnetic tape in 1973-74 and are available for study.

- 2 "Arapaho Dialects" (Kroeber 1916a). See Letter 203.

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Geological Survey

Ottawa, September 23rd, 1915

Dear Dr. Kroeber,-

I have refrained from writing to you earlier than this because I wished to be done with my work with Ishi before committing myself one way or the other in regard to progress or lack of it. Now that I am safely back in Ottawa, I may say that I sincerely believe that I have succeeded in getting material of value from him. At first the task seemed perfectly hopeless, for reasons which you know better than any one else. I despaired for a while of being able to get text from him at all, but found before very long that he could be made to dictate reasonably good texts. The difficulty was not so much in writing down Ishi's words, as in getting him to interpret them. As a matter of fact, what success I have had is due almost entirely to brute memory of stems and grammatical elements familiar to me from Northern and Central Yana. A considerable amount of grammatical material was obtained from Ishi also by suggesting Central Yana forms and getting his Southern Yana equivalents, where possible.

The body of text material secured is far from abundant, but considering the circumstances, the output is not so bad after all. There are a considerable number of grammatical points that are still obscure to me, and quite a few words here and there that I could not satisfactorily unravel, but there are real results nevertheless. I think I may safely say that my work with Ishi is by far the most time-consuming and nerve-racking that I have ever undertaken. Ishi's imperturbable good humour alone made the work possible, though it also added at times to my exasperation.



A. L. Kroeber and Ishi, 1911  
(Courtesy of the Bancroft Library.)



Left to right: T. T. Waterman, Paul Radin, Ishi, Robert Lowie, and Edward Sapir. Taken in Berkeley during a special summer meeting of the American Anthropological Association, August 3-5, 1915. (Courtesy of Philip Sapir.)

One of the gratifying results of the work has been a clearer insight on my part into the phonological problems of Yana as a whole. On the whole, Southern Yana is somewhat more archaic in phonetic respects than the other dialects, so that valuable light is thrown on matters which would otherwise have remained somewhat in obscurity. The lexical and grammatical differences are not great, though the divergence from Central Yana seems greater than that of Central from Northern Yana. I am very much surprised to learn from Waterman that Ishi had difficulty in understanding Sam Batwi, the more so as Ishi seemed to be well acquainted with Central Yana forms as distinct from those of his own dialect. Perhaps the most interesting thing that I was able to do with Ishi was to get a fairly complete set of terms of relationship from him. In fact, I think that the set that I secured from Ishi is rather more complete than that which I had obtained from Betty Brown, my Northern Yana informant. The method that I used in getting this material from Ishi was to employ different colored paper-fasteners as counters, one set standing for males, the other for females. By arranging these on the table somewhat in the manner of a genealogical tree, and by long-winded and round-about explanations of what was wanted in each case, I succeeded in getting results which have every internal evidence of being correct, and which chime well with results obtained from Northern Yana, with some very interesting differences of evident significance. As a matter of random curiosity, you may be interested to learn that it took Ishi some little time to remember his words for father and mother. He had evidently not used them for many years.<sup>1</sup>

There is a sad side to this recital that you have probably been informed about by this time. Some time before leaving Berkeley, Ishi became decidedly ill, and had to be removed to the University hospital in San Francisco. This illness of Ishi's and the time taken up by the anthropological meeting cut down the period of my working time somewhat. I am rather afraid that Ishi has tuberculosis, and the prospect of a cure seems far from certain. I sincerely hope, however, that he will be able to pull through. If so, arrangements should certainly be made to have him provided for in some more suitable climate.<sup>2</sup>

I understand that you have had excellent success among the Zuzi, and I hope that you can find time to drop me a note in regard to your experiences.

With best wishes,

Yours very sincerely,

E. Sapir.

P.S. Enclosed I am sending you a list of ten place names that I incidentally secured at one time from Ishi. As I understand that you and Waterman have made a rather thorough study of Southern Yana ethnogeography, I am placing these at your disposal, so that you may make whatever use you like of them. Several of the names were familiar to me from data obtained further north.

- 1 Sapir used this data in a paper on "Yana Terms of Relationship" (1918). In another paper (Sapir 1923a) Sapir gives further details of his work with Ishi:

"Throughout the period of my work with him, he was gentle and patient to a degree. Had he been surly, like several of the more northern Yana I have known, progress in this most nerve-racking of researches would have been impossible. It should be remembered by anyone who makes a study of [the Yahi text in] this paper and who may be inclined to feel annoyance at the gaps in my analysis that Ishi's English was of the crudest. "Him's no good" did duty for "He (or it) is bad" or "That is not correct," while "sista" might mean equally "sister" or "brother." Ishi was perfectly willing to dictate and to interpret; the difficulties followed unavoidably from the circumstances. In going over his texts for interlinear translations--and it proved a difficult task to hold Ishi in leash in the matter of speed of dictation--I endeavored to use every tittle of evidence that I could muster, Ishi's "explanation" of the single words, his accompanying gestures, the context of the myth itself, and, most important of all, the analogies of the northern dialects. Had Yahi proved to be less closely related to these dialects than it is, it is difficult to believe that it would have been feasible to secure from Ishi more than merely lexical information." (Sapir 1923a:264).

- 2 Ishi died early in the following year. His medical history was described in some detail by Dr. Saxton T. Pope (1920). Signs of Ishi's final illness first appeared in 1915:

"He had been ailing all winter and showed an increased disinclination to shoot his bow or bestir himself. In the summer vacation of this year Ishi was moved to Berkeley, where he was studied by Dr. E. Sapir, and very valuable data were obtained on the subject of his language and mythology. For three months he was in constant communication with Dr. Sapir, living at Professor T. T. Waterman's home



under the most hygienic conditions with plenty of outdoor recreation, sleeping, proper food, and diversion. Nevertheless, his health suddenly began to fail, and on August 22 he was returned to the University Hospital in San Francisco, where he remained for six weeks.... At the expiration of his stay in the hospital, in October, Ishi was removed to temporary quarters arranged in the museum,... In March, 1916, when his weakness progressed to an extreme degree, we moved him back to the hospital where he could receive better nursing and alimentary feeding. Shortly after re-entering the hospital he had a very large pulmonary hemorrhage.... He died soon after, at 12:20 P.M., March 25, 1916." (Pope 1920:198-206).

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Holland-America Line, S.S. Ryndam

November 17, 1915

Dear Sapir:

I am giving the ocean hours not wasted in chess to the preparation of a paper on the Tribes of the Pacific Coast, which I promised Holmes for the Christmas Congress at Washington months ago but forgot all about until homeward bound.<sup>1</sup> I find it necessary to take some cognizance of the newer enlarged linguistic families as evidencing movements of population; and would like to ask whether you would be willing to have me mention you as believing\* in the relation of Coahuiltecan to Hokan, and of your Oregon languages with Penutian. My doing so is not necessary to the proving of my case, but the paper must largely be a review of the present state of knowledge, and I should be glad to be able to include everything pertinent that is likely to be definitely on record in the near future.

Shall we see you at Washington?

My address for the winter will be care of the American Museum, New York.

Sincerely,

A. L. Kroeber

\* or "conjecturing same as possible"

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1 Kroeber (who was on sabbatical leave during 1915-16) was returning from Europe. (For his impressions of wartime Germany and England, see Kroeber

1916b). His paper on "Tribes of the Pacific Coast of North America" (Kroeber 1917a) was for the 19th International Congress of Americanists, held in Washington, D.C., in December, 1915.--W.H. Holmes (1846-1933) had succeeded Powell in 1902 as Chief of the Bureau of American Ethnology, serving until 1909. At this period he was Head Curator of the U.S. National Museum, the host institution for the Congress. See Hough 1933.

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Geological Survey

Ottawa, November 25th, 1915

Dear Dr. Kroeber,-

I am in receipt of your letter of November 17th. I would not at all object to your referring to my newer and as yet unpublished linguistic combinations. The Hokan-Coahuiltecan stock that I now recognize, you may refer to as being, in my opinion, "very probable". (My paper on this subject is now in Radin's hands, for his new Southwestern Society.)<sup>1</sup> These words would also seem appropriate to me in connection with Penutian and Coos-Takelma. I have little doubt that Siuslaw and Alsea will turn out to be more or less closely connected with Coos, but as there is no published material on these as yet to speak of, it would be safest to limit ourselves to Coos and Takelma. Since I last wrote you about these matters, my preliminary report on Haida, Tlingit, and Athabaskan has appeared in the American Anthropologist.<sup>2</sup> You may have seen it by this time. By the way, have you any ideas in regard to the linguistic position of Zuñi?

I do not know whether I shall be able to attend the meeting at Washington or not, though it seems somewhat likely at present. I am down for a short paper on "Terms of Relationship and the Levirate",<sup>3</sup> in which I am to use material obtained from Ishi.

With best wishes,

Yours sincerely,

E. Sapir.

- 1 Radin, though still employed by the Anthropological Survey of Canada, was in Santa Fe at this time, owing to his wife's health. He seems to have become active in anthropological circles in Santa Fe and Taos, and helped set up the Southwestern Anthropological Association. A series of SWAS Papers was initiated in 1915 with Radin's "The Winnebago Myth of the Twins", but it soon foundered. Sapir's paper on Hokan-Coahuiltecan was published in IJAL several years later (Sapir 1920a).
- 2 "The Nadene Languages: A Preliminary Report" (Sapir 1915b).
- 3 Sapir 1916a.

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[New York, N. Y.]

November 28, 1915

Dear Sapir:

Thanks. Your opinion and the approximate location of the languages is all I need refer to as I am only concerned with indications of movement. I had a thorough and amiable discussion of relationship with Boas last night. I tried to make him see that no one would quarrel with his stand if he did not feel it necessary to antagonize ours.

I have always thought Zuñi was Siouan, but don't think it will hold. I can find just as much evidence--and that mighty little--pointing to Hokan. It is not Uto-Aztecan or Athabascan or Algonkin. I have even tried Muskogean. I know it is something, but have neglected my American linguistics too much of late years to be able to trust an instinct very far. I am going to go through your Na-Dene carefully.

I hope you get to Washington. I want to see you, both generally and specially. Do you know it is seven years and a half since we have set eyes on each other? You have risen in the world since then. Also I want to scrap with you. As a neutral I scrap with all partisans in the war, barring those really in it.

I am wondering whether Radin has really got something going, or is still shoestringing like a crazy gambler. Last spring I was his only supporter besides Farrand.<sup>1</sup> Since then he hasn't peeped to me.

Yours,

A. L. Kroeber

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1 Livingston Farrand (1867-1939), a physician and psychologist who turned to anthropology and was Boas's colleague at Columbia from 1901 to 1914. In 1914 Farrand became President of the University of Colorado, and from 1921 to 1937 he was President of Cornell.

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American Museum of Natural History

New York City, December 8, 1915

Dear Sapir:

I have just read the sketch of Coos<sup>1</sup> the first time. It is a vehemently Penutian language in structure, at least as much as Maidu. I hope you will cover this side thoroughly in your demonstration, or allow me to bring out a paper on it during the winter.

As regards the material of the language, I have no doubt you have the requisite evidence, though the phonetic system has certainly covered it superficially, and I have not looked further.

Between you and me, and not for the official file, Frachtenberg has got good stuff on Coos, but done a poor job. I have done as well on less and worse material.

Yours,

A. L. Kroeber

---

1 Frachtenberg 1922a (available as a separate publication in 1914). See Letter 175.

[192]

Ottawa, Ont.

December 9, 1915

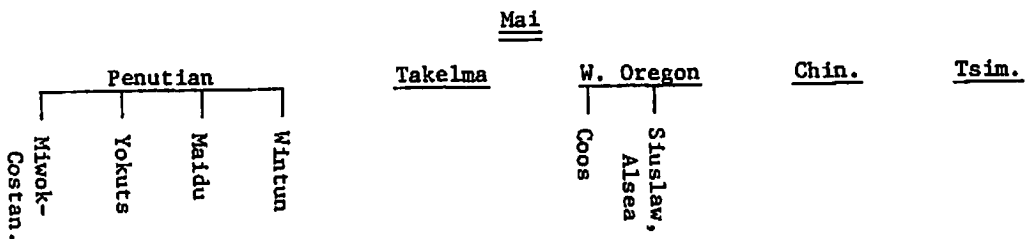
Dear Dr. Kroeber,

I have been looking through Frachtenberg's Lower Umpqua (Siuslaw) material<sup>1</sup> and find that Lower Umpqua is not only related to Coos but is, indeed, much more closely related to it than Takelma is. There is no reasonable doubt of this whatever. You may, therefore, quote me as maintaining that your Penutian stock is continued in Oregon by Takelma, Coos, and Lower Umpqua (Siuslaw). Frachtenberg's recently announced Siuslaw "stock" is only a joke, even if he doesn't know it.<sup>2</sup> It seems certain, from what I am able to gather here and there, that Siuslaw is also closely related, relatively speaking, to Alsea (Yakonan). In fact, I strongly surmise that Coos, Siuslaw, and Alsea form a fairly close unit as contrasted with Takelma and with Penutian proper. However, we have no right to say anything about Alsea yet, as hardly any material is available.

To you personally (but for Heaven's sake, don't quote me to Boas, Goddard, or anyone else as yet) I don't mind saying that I now believe this enlarged Penutian stock to travel still further north and to include, though you may blink with incredulity, Chinook! This Chinookan language is greatly specialized, it is true, but I can explain most of its peculiarities as secondary developments. In its adverbs there are even very clear remnants of old case endings found further south (I am not, of course, referring to recently borrowed Sahaptin postpositions found in Upper Chinook). The verbal prefixes (tense, pronominal, and prepositional) are originally independent particles that have built up a new synthesis. I'd like to talk some problems over with you--it takes too long to write about them.

And now (don't faint!), I think Tsimshian is the most northern outlying member of the stock. Again greatly specialized, but still exhibiting many startling features in common (e.g. Nass River nê-gî "not", nē "not?": Lower Chinook nê-ket "not", Wishram nā-qxi : Coos īn "not", ǎn "not thou", nī "not I" < n-nī : Takelma ánī "not", nī<sup>1</sup> "not?"; Tsimshian prefixed and infixed l-,

-l- for plural verbs, e.g. pl. ayaluwa "to shout" < ayawa : Chinook infixes and suffixed -l-, -(a)l, -l...-l for frequentatives, e.g. -pculit < -pcut "to hide" : Takelma -al- for continuatives and -iha for frequentatives : Yokuts -l-, -d- for frequentatives). Of Chinook and Tsimshian I am not as sure as of Lower Umpqua, Coos, and Takelma, but I think my evidence will grow as I work on it. How to group these languages I do not yet know, of course. I would suggest:



However, this is premature. We do not even know if Penutian, as first defined, is really a unit. Takelma may turn out to be coordinate with say Yokuts, not Penutian as such. My "W. Oregon" may eventually have to be grouped with Chinook, though this hardly seems likely. I doubt if Takelma, W. Oregon, Chinook, and Tsimshian form a northern unit as contrasted with your southern one.

Naturally, I am looking forward to Frachtenberg's Alsea and Kalapuya material. If these two fall in line, we have a continuum from Yokuts to Chinook broken only by obviously intrusive Athabaskan between Takelma and Coos and doubtless also intrusive Shasta between Takelma and Wintun. Only Tsimshian would be really much apart geographically. If its inclusion proves O.K., the Wakashan-Salish-Chemakuan group form an interesting problem from the point of view of movements of population.

Enough for the present.

Yours sincerely,

E. Sapir.

1 Lower Umpqua Texts (Frachtenberg 1914). His grammar (1922b) was probably also available to Sapir, in a preliminary paper-cover publication.

- 2 J. Owen Dorsey, who did extensive field work on the languages of the Oregon coast in 1884, concluded that Lower Umpqua (and Siuslaw) belonged to the same linguistic stock as Alsea. This stock, which he called "Yakonon", was included in the Powell-Henshaw classification (Powell 1891). Frachtenberg, however, in the Introduction to his Siuslaw (Lower Umpqua) grammar, announced that "a closer study... proves conclusively that Siuslaw and Lower Umpqua form a distinct family, which I propose to call the Siuslawan linguistic stock" (1922b:437).

[193]

Ottawa, Ont.

December 10, 1915

Dear Dr. Kroeber,

Please go ahead and write your Coos-Penutian paper. In fact I should be glad not to be alone in this new series of developments. My original idea was to write a morphological and phonologic sketch showing that Coos and Takelma are related. Before I got around to actually writing this paper I discovered, as I wrote you before you left California, that both were Penutian, which fact naturally made a specific Coos-Takelma demonstration rather out of focus unless one could show that Coos and Takelma form a unit as contrasted with Penutian proper. This, however, I see no reason to believe. On some points Coos is more in accord with Penutian than Takelma, in others Takelma may be in closer accord, though there seem to be fewer such points. Now I feel that we cannot get quite the right perspective until Frachtenberg publishes his Lower Umpqua and Alsea material.

However, it may be as well to proceed gradually and to clinch the main point, i. e., show that Penutian has Oregonian cognates, by restricting yourself to Coos and Penutian. In that case would you mind stating that I have independently found that Coos has both morphologic and lexical analogies with Penutian? Or we might collaborate. Write out your Coos-Penutian sketch and I'll add whatever I have that you've missed--morphology and lexical material. I really oughtn't to go on with so much new stuff, as I have a staggering load of descriptive material on my hands that I ought to get out (only yesterday my

Nootka interpreter sent me 288 pp. of text and interlinear--foolscap size-- dealing with details of his marriage, including speeches at various stages of proceedings!); but these new vistas are too fascinating to resist.

I agree with you on Frachtenberg, but in justice to him it should be remembered that Coos was his first job. His stuff is good, as you say, but his presentation suffers from lack of power of analysis. For instance, he says it is impossible to suggest analysis of irregular noun plurals. Now, as a matter of fact, there are two main types involved, both of which are of far-reaching importance. You have -n(e) as suffix (e.g. mē-n < mā "person" = Penutian mai), which finds its analogue in Takelma -(h)an (e.g. yaplahan "persons") and certain Californian suffixes that I cannot quote from memory (my books are not accessible just now). Then you have vocalic suffix -e, -a with internal change (e.g. Coos k·nes "hunchback" : pl. k·enēyese); these have startling parallels in Yokuts and, I may now add, also in Tsimshian, where there are some survivals. In other words, Fr. has been absolutely silent on two points of tremendous importance. Then again, as I once pointed out to you, he does not speak of stem forms c<sub>1</sub> v c<sub>1</sub> v (e.g. wit<sub>in</sub> "blood"), which is decidedly peculiar to Takelma and all Penutian languages; I found over 100 Coos examples. He has also obscured his presentation of case endings (various local -tc suffixes) needlessly (incidentally you'll be interested to learn of Lower Umpqua -ū locatives, cf. Yokuts and possible survivals in Takelma and Chinook). All through obvious things have missed him. Thus, his particle yūL is compounded of ī and ūL.

It's remarkable how prognostic general impressions may be. Both Tsimshian and Takelma, to some extent also Chinook, had always struck me as differing from most languages in America by being more analytic and irregular in rather subtle features; they suggested English more easily than say Algonkian or Kwakiutl. This feeling is now borne out by Yokuts and Coos.

Yours sincerely,

E. Sapir.



[194]

American Museum of Natural History  
New York City, Jan. 7, 1916

Dear Sapir:

I am engulfed in the basic map of my California handbook.<sup>1</sup> One of the points of resistance is Yana. Will you tell me if the following is correct?

1910 <u>Classification</u>	1916 <u>Designations</u>	<u>Territory</u>
"Northern" (gari'i)	Northeastern	Montgomery & Cedar Cr.
"Central" (gata'i)	Northwestern	Cow Cr.
"Southern"	Central	Battle & Antelope Cr.
---	Southern (Yahi)	Mill & Deer Cr.

I do not suppose you have any forms from the (new) Central dialect; but have you information or belief that it exists, or did Ishi's people take in all the Battle Creek territory? I think it very unlikely, though he knows the country. I am not even sure there were four dialects, though Sam's muddled talk gave me the impression that there was a group between his people and Ishi's.

How about designations? I shall be grateful for any suggestions. As Gari'i and Gata'i seem to contain the stem for "speak", I presume they refer to dialect only, and not to people, so should be avoided as "tribal" names.

Do you know whether Nozi was applied to all the Yana, or only to one or several divisions?

My map will be rotten in spots, but useful and partly new.

Sincerely,

A. L. Kroeber

<sup>1</sup> Handbook of the Indians of California (Kroeber 1925a).

[195]

Geological Survey

Ottawa, January 10th, 1915 [read: 1916]

Dear Kroeber,-

I think it would be poor policy to change the nomenclature of Yana tribes that I have already established. It would be particularly confusing to have what was originally called "Central" now termed "Northwestern", and what was originally "Southern" now termed "Central". Moreover, although it is quite likely on general principles that there are dialectic differences between Ishi's dialect and Southern Yana of Battle and Antelope Creeks, I myself have no tangible evidence of this. Of course, if you have yourself obtained such evidence from Sam Batwi and Ishi, you would be justified in mapping these two dialects as distinct. Should you find it necessary to carry this out, I would suggest that you speak of "Northern Yana", "Central Yana", and "Southern Yana" or "Yahi", subdividing this last into "Northern Yahi" and "Southern Yahi" or "Kombo". This last term, you may remember, is taken from Powers, who calls the other three groups Yana. I do not know if I have ever told you before, but during my work at Montgomery Creek in 1907, I obtained quite a few forms from Betty Brown of Yana as spoken around Antelope Creek. These are evidently much more closely related to Ishi's forms than to those of either Northern or Central Yana. Thus, this dialect has the -hi ending, where the two northern dialects have -na. It is for this reason that I believe that "Yahi" is a justifiable term for both the Antelope Creek and Ishi's dialects. I should avoid the terms "Gari'i" and "Gat'ā'i", as they refer to dialect, and are moreover variable in their application. Thus, while Betty Brown and Sam Batwi used the former for the Montgomery Creek dialect and the latter for the other two, Ishi used the former for my Northern and Central Yana and the latter for his own. It is clear that the terms have no strict tribal or local reference.

I do not know if the term Nozi was applied to any of the Yana south of Battle Creek, but it evidently included both Northern and Central Yana. It was not applied by Powers to Ishi's people. Our knowledge is of course weakest in regard to the people between Battle Creek and Middle Creek. I may

say, though, that I feel pretty certain that the dialectic difference, if any, between Northern Yahi and Southern Yahi or Kombo is slighter than that which we know to exist between Northern and Central Yana. My own tendency in the present state of our knowledge would be to ignore the possible difference altogether, as far as linguistics is concerned.<sup>1</sup>

Yours sincerely,

E. Sapir.

Yana

N. Yana

C. Yana

S. Yana (Yahi)

N. Yahi

S. Yahi (Kombo)

---

1 This classification differs from the one Sapir later outlined in "Text Analyses of Three Yana Dialects" (Sapir 1923a:263). In the latter, Northern and Central Yana are described as being relatively close, probably mutually intelligible; Ishi's Yahi is characterized as "archaic" and quite divergent from the other dialects; and Southern Yana (insofar as Sapir had data on it from Sam Batwi--Betty Brown is not mentioned as a source of Southern Yana data) is identified as a "link" between the two northern dialects and Yahi. Spier (in Sapir and Spier 1943:243-45) attempts to reconcile Sapir's various interpretations of Yana dialect diversity. See also Swadesh's discussion in the Yana Dictionary (Sapir and Swadesh 1960: 13-15).

[196]

[Ottawa]

March 16, 1916

Dear Kroeber,

I am preparing for your Univ. of Cal. series a paper on "The Position of Yana in the Hokan Stock".<sup>1</sup> I give all the lexical evidence I have, which is rather surprisingly considerable. My main aim is to show as clearly as can be

done at present that even the weakest claimant for Hokan honors (aside from Salinan and Chumash) is quite unmistakably such nevertheless.

In a manuscript you once loaned me<sup>2</sup> you had Shastan: es, etca, etc-ni "to sing". Which of these forms is Shasta, Achomawi, or Atsugewi? I don't like to quote forms vaguely.

Yours,

E. S.

P.S. And what language does isi-duk belong to? Karok?

P.P.S. I find it hard to reconcile Mohave is-var and su-var with these forms, because of Pomo xe-, ke-, ih-, which point to Hokan \*exe-. Shasta-Ach. and Karok have s, tc < Hokan x, but Hokan x remains such in Yuman; e.g. "axa "water".

P.P.P.S. If Radin finds it impracticable to publish my Hokan-Coahuiltecan paper, would you care to have it for your series?<sup>3</sup>

Yours,

E. S.

---

1 Sapir 1917a.

2 See Letter 172.

3 See Letter 189.

[197]

[New York, N.Y.]

3-19-16

Dear Sapir:

Enclosed are the "sing" words. I will look up asmak. I slipped up on Mohave "ear". The one I gave in the earlier work is wrong. I think it means "leaf". The later one is right. I will try to verify this also.

I shall be delighted to get and print your Yana-Hokan paper. We can do this because it is based on University material, in the sense that the institution was footing the bill when you secured the information. The same rule,

however, prevents our taking the Coahuiltecan ms.: the statute would apply automatically, and the Committee would not even vote.

Washo-Hokan I fear would be in the same category, but as you say the paper would be brief, there may be no great difficulty finding a vehicle for it.<sup>1</sup> I shall be very glad if you can settle the status of Washo. You know my own feeling as to it; but I am too slow to work out the soundshifts in spare moments and would probably never get around to soaking in enough to begin to guess them. By the way, my Washo phonetics are particularly rotten. I never heard the language but a few days, and didn't know one sound from another in principle even at the time.

In general, go ahead on the business as much as you like. You can do it better; and it will relieve me of responsibility that presses. Dixon has agreed that at the first opportunity we will print Part I, consisting of historical introduction and the raw vocabularies. This will give you what you want; and if you write Part II, the interrelations and classification, before we get to it, confidentially, I shall be much relieved. I'd love to play with the stuff, especially Penutian; but there are many other pleasant things I expect to have to forego; and I'm not as greedy of hopes as I used to be.

As to Yuki, I don't know. It strikes me something as a Hokan Yana made you feel at first blush, but I'm open to anything.

If you can get the Washo settled soon, I shall be able to get it properly into my California book. By the way, the scope of this requires the elimination of everything linguistic that has no ethnic or historical bearing; but I am working out some good relations of language to topography on maps.

Yours,

A. L. Kroeber

Sing

Karok	pakuri
Chimariko	-tak-
Achomawi	-es-
Atsugewi	-etca-
Shasta	-etcni-
McCloud Shasta (Okwanuchu)	-isiduk

- 1 Kroeber evidently refers here to the MS of "The Status of Washo", which Sapir must have offered him in a letter not preserved. The paper--a brief note--was published in the American Anthropologist the following year (Sapir 1917b), following an announcement by J. P. Harrington that he had discovered a relationship between Chumash and Washo (Harrington 1917). Sapir also contributed "Data on Washo and Hoka" to the final published version of (Dixon and) Kroeber's paper on California linguistic classification (Dixon and Kroeber 1919:108-112); see Letters 237-9. See also Sapir 1921b:72.

[198]

[New York, N.Y.]

3-24-16

Dear Sapir:

asmak is Atsugewi.Karok is itiv.

As regards Mohave, I can give no new verification away from any notes. You had better stick to note 8 on table facing page 282 in volume 11 of our series.<sup>1</sup>

Sincerely,

A. L. Kroeber

---

1 "Serian, Tequistlatecan, and Hoka" (Kroeber 1915a).

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[199]

American Museum of Natural History  
New York City, April 13, 1916

Dear Sapir:

It looks good to me.<sup>1</sup> I hope you go on with it. Every nail of proof you drive in absolves me from so much that I haven't got around to doing. I'm sure you have a sound case.

If I could get this book<sup>2</sup> off my shoulders, I'd see at once that our general comparative vocabulary was put in shape and published, whether we ever added analysis or not, but I cannot fairly let myself be distracted.

Yours,

A. L. Kroeber

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1 Presumably the draft of (a portion of) "The Position of Yana in the Hokan Stock". Sapir completed the MS the following month (see Letter 201).

2 Handbook of the Indians of California (Kroeber 1925a).

[200]

[New York, N.Y.]

5-9-16

Dear Sapir:

	<u>Tail</u>	<u>Sugar Pine</u>	<u>Sleep</u>
Karok	apuvi	uc-ip	kivit
Chimariko	aqūye	haqewina	-po-
Achomawi	ipi	asauyo	
Atsugewi	t'ipwin	atcawop	-itsmi-
Shasta	ihiwa	atsahu	-itsmas-
Okwanuchu*	ip <sup>h</sup> ñwa [sic]	atsañihu	-itsiwi

\* Proper name of "McCloud Shasta"

I am sorry these got overlooked. Are there any others outstanding?

Yours,

A. L. Kroeber

[201]

Geological Survey

Ottawa, May 25th, 1916

Dear Kroeber,-

Under registered cover I am sending you my paper on "The Position of Yana in the Hoka Stock".<sup>1</sup> I trust you will find it acceptable for your series. Altogether the evidence is perhaps more extensive and convincing than might have been supposed in the present state of our knowledge. Incidentally the paper may have some value in giving further preliminary



information on Yana grammar, though such information is, of course, only incidental and rather implied than definitely imparted.

With best wishes,

Yours sincerely,

E. Sapir.

P.S. In this paper I naturally dare surmise nothing in regard to Washo being Hokan, yet several of my entries would have gained in force if I could have included Washo. Thus, for no. 166, cf. Washo -uk, -buk "toward the speaker" (for -u, -bu, cf. -ue, -bue "from the speaker").

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1 Sapir 1917a.

[202]

Affiliated Colleges

San Francisco, Cal., August 22, 1916

Dear Sapir:

I am today submitting your paper on Yana to the University Press.

Waterman still thinks that there should be a key giving the values of the symbols used. I presume you regard such a key as unnecessary because the paper is comparative and not descriptive and the orthographies in the material available to you are too variable. If I hear nothing from you on the matter, I shall conclude that you wish to stand pat. If you are willing to add a note giving a key or explaining why none is furnished, I can always add it to the paper without affecting its status in the University Press.

The printer told me yesterday that he is handling material with reasonable promptness at the present time.

My work of Zufi will appear as soon as the New York Museum puts it through the press.<sup>1</sup> I am forwarding the manuscript to Wissler to-day. There is no question that the clan in the Southwest is a very different thing from what has been believed, and I hold a strong conviction that to a large degree, the condition which I have found there will be seen to exist elsewhere, also as

soon as the subject is approached with another attitude than the old conventional one.

Things are in excellent shape with us here and with reasonable luck, we should have a successful year. Let me know what you and your people are doing.

Sincerely yours,

A. L. Kroeber

---

1 "Zuñi Potsherds" (Kroeber 1916c) and "Zuñi Kin and Clan" (Kroeber 1917b).

[203]

Geological Survey

Ottawa, August 28th, 1916

Dear Kroeber,-

I see that you are back in the foggy atmosphere of Affiliated Colleges. I did not append a phonetic note to my Yana paper for the reason, as you yourself suggest, that I was quoting from various authorities that used different orthographies. I have no doubt that those that are able and interested enough to follow the paper will have no difficulty with matters of orthography. However, if you wish, you might append the following note:

"The phonetic orthographies of the various authorities cited have been left unchanged, and are explained in the sources. Stress accents, however, have been omitted. The system used for Yana in this paper is identical with that employed in my 'Yana Texts', except that the glottal catch is rendered by an apostrophe ('')." "

We have not been doing quite as much as usual in field work this summer owing to the war. Barbeau has collected a lot of new French Canadian folklore, not only stories this time, but a great number of folk songs. Waugh<sup>1</sup> has been studying the material culture of the Ojibwa of the region of Lake Nipigon.

I take this opportunity of telling you that I read your paper on Arapaho<sup>2</sup> with very real interest. What impressed itself upon me was that once the

phonetic laws which differentiate Arapaho are worked out, the divergence of Arapaho from other Algonkin dialects will be seen to be very much less than generally believed.<sup>3</sup> I believe that there are quite a number of points of comparative interest that have escaped you. As a matter of fact, I have been flirting with the idea of writing a more or less formal study of Arapaho comparative phonology. One impression which I have gained is that Arapaho and Cheyenne do belong, after all, to a single subdivision of Algonkin. There seem to be some rather important special points of resemblance.

With best wishes,

Yours sincerely,

E. Sapir.

- 
- 1 F.W. Waugh, an anthropologist on Sapir's staff.
  - 2 "Arapaho Dialects" (Kroeber 1916a).
  - 3 Sapir goes into further detail in a letter to Lowie, dated August 12, 1916: "Thus, it is possible to show with 'mathematical' certainty that the Arapaho word for 'leg', wa'a<sup>n</sup>θi goes back to Algonkin miskāti and is exactly cognate to Cree miskāt" (Sapir 1965:19).

[204]

Affiliated Colleges

San Francisco, August 29, 1916

Dear Sapir:

Our Librarian, who is a man of business, is much disturbed at not having received from you either the key to the Anthropological Seminar Room in the Library or any reply to his request for same. Will you not kindly attend to the matter and help us preserve our standing with the Library?

Sincerely yours,

A. L. Kroeber

[205]

Affiliated Colleges

San Francisco, September 2nd, 1916

Dear Sapir:-

Will you tell me whether you think it advisable to publish a couple of hundred pages of Northern Paiute texts of the quality of the attached sample page? They are the Marsden materials.<sup>1</sup> The typewriting is an exact copy from his manuscript and the red ink indicates the approximate degree of correction which I can add. I have had two Indians here with the idea of whipping these texts into shape but it is impossible to make revisions without the original informant, and considerations of time as well as money do not render it feasible to bring him here. I have learned enough about Northern Paiute to know that Marsden's texts are excellent specimens of the language, that the translations are satisfactory and that his orthography is execrable. I am sure that by the time he died he had learned to hear the sounds accurately because he spoke them correctly but many years of going over and over the ground had fixed certain visual images of how words looked on paper so firmly in his mind that he seemed quite incapable of making even the most obvious alterations in spelling.

The fundamental defect of his writing is a failure to distinguish between the change from surd to sonant and vice versa. As two of the three classes have coincided in Northern Paiute the matter is comparatively simple. But yet I find that I could not put the texts into correct shape on this score without an infinitude of work. My man from Pyramid Lake learned to write very quickly and quite correctly, in all other respects, but absolutely got stuck at this point. The principal other defect of the texts is that Marsden commenced to write either "i" or "u" for "ü" and never got over the habit. I have enough material of my own to correct him for the most common words and suffixes.

The other changes are largely of a mechanical nature.

I am particularly desirous of having your opinion because, for the present at least, you would make more use of these texts than anyone else. Their disposition has hung fire now for something like three years and I feel that the only alternatives that remain are either to bring them out as quickly as

possible along the lines of my sample, or to return them to Mrs. Marsden as unublishable.

I will only add that the red ink corrections on the sample are entirely from memory and that I might be able to do a little better after I get into the work and my memory becomes refreshed from my own notes. But I do not think that this will be a very considerable factor and prefer to receive your opinion on the basis of the sample as it stands.

Sincerely yours,

A. L. Kroeber

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1 Dr. W.L. Marsden. See Letters 130 and 150. A small selection of these texts was eventually published (Marsden 1923).

[206]

Affiliated Colleges

San Francisco, September 5th, 1916

Dear Sapir:-

I should have liked to do something real with Arapaho, but in the face of a wide spread group of languages it seemed absurd to try to do so with Michelson and you in the field. I had a sense of duty about bringing out something on the subject after all the time I sunk into it in former years, but my chief sense when the paper was done was one of relief at having extracted myself from a bad and old mess. Apart from such service as the material itself may be, I consider the only point of value in the work to be the definition of the interpretation of the Algonkin verb stem. I am very earnestly convinced that the essential nature of these languages will never be understood until our understanding of this point is clear. I am also of the opinion that this job will have to be done by you. Michelson has the knowledge, but is either afraid to go to the heart of the issue or incapable of doing so, and Uhlenbeck promises even less. It is extremely unfortunate that with all our detailed knowledge we should be, from a broad point of view,

so thoroughly in the dark as to the nature of a group of languages that are perhaps the most characteristic in North America.

I shall append to your paper the foot-note you suggest.

Gifford has showed me your letter of the 25th to him.<sup>1</sup> I agree with you that the Wintun have extended northward. I think, however, that you should use Gifford's avowedly tentative classifications with caution. As I have pointed out to him, his classification of our kinship systems is substantially on the basis of only two or three characteristics out of a dozen or more that might be employed as Criteria. I have just gone over the ground on the basis of my own material collected at various times in the last fifteen years and conclude that the primary types of systems are three, which correspond fairly closely with the three culture areas which we have always recognized in the state. The central type is fairly close to that of the plateau. Within this larger central region there has been a specialization which has reached its extreme among the Wintun, but has also affected the Miwok and Pomo. I suspect that Gifford's classification reflects primarily the difference between this specialized central sub-type of the system, as he first became familiar with it among the Miwok, and the more generalized central sub-type as it presented itself to him among the Mono. I trust that within a year we shall have all the evidence necessary to answer these problems.<sup>2</sup>

The apparent coincidence of types of kinship systems with types of culture interests me exceedingly. The Wintun specialization in terms of relationship is exactly paralleled by their role in religion and other matters. There do not seem to be any distinctive types of social organization corresponding with the culture areas. I infer, therefore, that [it] is the cultural color or setting or attitude of mind that has chiefly shaped the kinship systems. I think this finding will commend itself as reasonable to anyone who is an Ethnologist and not an Evolutionist.

Sincerely yours,

Kroeber

[PS] Forgive the typist--she's brand new.

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1 Sapir had earlier written to Lowie:

I am glad to learn from Gifford that he finds Wintun [kinship terminology] to go with Miwok and Yokuts, while Maidu belongs to the other class... [This] neatly corroborates what I had calculated on linguistic grounds. Though Wintun reaches further north than Maidu, I thought it possible to infer from certain points of morphology that Maidu had been in longer contact with Hokan-speaking peoples. I ventured to conclude that Wintun had moved up north after Maidu, which chimes in well with Gifford's evidence. (Sapir 1965:19).

- 2 Gifford's first work on California kinship terminology was on Central Miwok (Gifford 1916), followed shortly by a study of the Tubatulabal and Kawaiisu systems (Gifford 1917). Encouraged by Kroeber, he then began a kinship survey of the entire state, published in 1922. Kroeber, meanwhile, published his own general treatment of California terminologies in 1917 (Kroeber 1917c).

[207]

Ottawa, Ont.

Sept. 6, 1916

Dear Kroeber,

Thinking that you might be somewhat interested in the mental pathology of your department, I take the liberty of enclosing an effusion that has just reached me. I am highly gratified to learn that my merely philological essay is being subjected to a keenly critical analysis worthy of the masterpieces of "the best authors". I had long known that Waterman was an "ass", to use his favorite word, but had never fully realized the delightful compound of "pedantry" and intolerance (masking as overflowing humor) which seems to be his chief claim to the possession of a personality.

Yours sincerely,

E. Sapir.

[Enclosure]

University of California  
Berkeley, California  
Aug. 31, 1916

Dear Eddie:

I have resigned my editorial labors to the Boss, and now, in connection with the suggested changes in your recently submitted Ms., I want in my private capacity to call your attention to the fact that you are an ass. You ought to reflect that the future-perfect does not exist in the best English. It is a pedantry. When worked into a subordinate clause it becomes doubly awkward. The use of the first personal pronoun is always more or less objectionable. You won't find that common in the best authors either. A phrase such as "I hope to have dispelled" strikes me as absolutely inane. Your beloved "of doubtful inclusion" also adds a touch of melancholy to a style which is none too good at best. Finally, a man in your position ought to have more sense than to scatter references to one footnote all the way through a paper. The Old Man has written you I believe about some other matters that are standing over.

Well, how is everything? We are having a gay time in Berkeley,

Yours,

T.T.W[aterman]

[208]

Geological Survey

Ottawa, September 8th, 1916

Dear Kroeber,-

I should say, in reply to your letter of September 2nd, that if the University has the money it could not go far wrong in publishing Marsden's "Northern Paiute Texts", inferior as they may be in phonetic respects. For one thing, the mythology that they include should be made accessible; for another, the texts, while not perfect, are clearly usable for comparative and other purposes. Of what comparative interest they are likely to prove, you may gather from a list of comparisons that I have instituted offhand with Southern Paiute cognates.

You say that your man from Pyramid Lake learned to write Northern Paiute very quickly and quite accurately in all respects except differentiation of surds and sonants. Of course, I do not know what instructions he received on



this point, but I hope you will pardon my frankness if I suggest that the trouble may not have been entirely his own. Practically anyone starting with an English-speaking bias would take great pains to make clear to such an informant the phonetic difference between the initial p of such a word as pabi'i "elder brother" and the medial b of the same word. As a matter of fact, however, the native informant would be sure to be puzzled, as the difference between the two, while real from an analytical standpoint, is a purely secondary consequence of mechanical factors. In other words, to the native consciousness, the two sounds are identical. If a Northern Paiute invented an alphabet of his own to record his own language, he would undoubtedly use the same character for both sounds. On the other hand, there is, as you know, a series of geminated surd stops, which occur medially. These, I have not the slightest doubt, could be easily differentiated by a native from the medial sonants. To our ears these geminated stops sound pretty much like the initial ungeminated stops. Hence, we tend to write with a p what is, organically speaking, both a special form of one consonant and a totally different consonant, and to write with b another special form of the former consonant. No wonder the native would get confused. Evidently the proper thing to do is either to have him write consistently b for all forms of the first consonant (e.g. babi'i), and p for the geminated stop (e.g. tüpi- "stone"), or p for all forms of the first consonant (e.g. papi'i) and pp or some other modification of p for the second (e.g. tüppi-). I am almost certain that if this method were adopted you would find that the native would catch on surprisingly readily. I have had enough experience with teaching Indians to write their own language to know that there is nothing simpler if one has only mastered the organically significant types of sounds. An exhaustive knowledge of all phonetic nuances by itself is apt to be more of a hindrance than a help in the teaching of orthography. I would go so far as to say that if one finds that he can make no progress with a native in the matter of teaching him to record a particular class of sounds, that the reason will nearly always, in the wash, be found to be a lack of adequate analysis on the part of the instructor. I am a firm believer in the consciousness, or if you like the sub-consciousness, in naive speakers of the organic (differentially significant) phonetic elements of their language.<sup>1</sup>

I started out to answer your question, and I find that I have written a treatise. I hope you will forgive my "pednatry".

Yours very sincerely,

E. Sapir.

- 
- 1 This is Sapir's earliest statement of the "phonemic principle". See the published version of this Southern Paiute example in Sapir 1933:48-52. See also Letters 150-52.

[209]

Affiliated Colleges

San Francisco, September 12, 1916

Dear Sapir:-

I am sorry Waterman got under your skin but I cannot view the matter as a department affair. You no doubt noted that his letter is specifically personal. He told me that he would write you about as he did. I received the information without other comment than that he was free to do anything he wished that did not involve the institution. My own opinion is that the only way to handle Waterman in a case of this sort is to keep your temper and give him some of his own rough-house. Nevertheless I am sorry if you were annoyed.

Sincerely yours,

A. L. Kroeber

[210]

Affiliated Colleges

San Francisco, September 14, 1916

Dear Sapir:

Thanks for your opinion on the Marsden texts. I shall write Mrs. Marsden and if I obtain her approval shall proceed with the editing.

Your point about orthography strikes me as excellent, only I do not see any theoretical reason why the lengthened p should be more distinct from the initial sound than is the shortened sound which I have written b. The latter has its occlusion so much abbreviated as to be very nearly a fricative. It is quite likely that the Paiute would feel in this matter as you predict and I am eager to try out your guess at the first opportunity, but it seems to me only a guess.

Sincerely,

A. L. Kroeber

[211]

Geological Survey

Ottawa, September 26th, 1916

Dear Kroeber,-

I have your two letters of September 12th and your letter of September 14th. All I know about Nass River social organization, insofar as my information is of independent origin, is contained in the Bulletin that you referred to.<sup>1</sup> Since that was published a couple of Nass River Chiefs have visited Ottawa and have been closely questioned by Mr. Barbeau, who had made a very intensive study of the social organization of the Tsimshian proper. He should be able to give you the statistical data that you require. I am, therefore, turning over your letter to him. Presumably you will hear from him before very long.

With best wishes,

Yours very sincerely,

E. Sapir.

P.S. I imagine you rather misunderstand my letter regarding Waterman. I wrote you as I did, not seriously intending to push "the matter as a department affair" but simply because I thought you might be amused to get another document for your archives on the type Homo Watermanensis. As for the "personal" part of it all, you are at perfect liberty to show Waterman my letter regarding

him, also this P.S. Nothing is further from my thoughts than "to handle Waterman" nor was I aware of the fact that I had failed to keep my temper. At any rate, I do not propose to chop future perfects with your esteemed professor!

E. S.

- 
- 1 A Sketch of the Social Organization of the Nass River Indians (Sapir 1915e). Kroeber apparently referred to this in a letter of September 12th that has not been preserved.

[212]

Affiliated Colleges

San Francisco, November 4, 1916

Dear Sapir:

The manager of the University Press in looking over your manuscript has found the following cases of words spelled in two ways. For some of the variants, I presume there is a reason, but others seem to be due to the omission of diacritical marks. Will you annotate the attached sheets? I will then make any necessary alterations in the manuscript. I assume that you hold a carbon copy.

Sincerely yours,

A. L. Kroeber

[213]

Affiliated Colleges

San Francisco, December 28, 1916

Dear Sapir:

Radin writes me that you can do nothing more for him after the end of March and asks for the fellowship that Mason has been holding. I can probably swing this his way, and superficially the arrangement looks like a good bargain

for the University. It does not, however, strike me as being sound policy. However content Radin may be now to receive a pittance, he cannot permanently get along on \$40 or \$50 a month. He would have to divide his time, which is always unsatisfactory on both sides; and if anything better came his way, we could not refuse to let him go in the middle of whatever he was undertaking for us. I am furthermore convinced that Radin is at a thorough crisis in his affairs and that either a definite place must be found for him in anthropology or it will be only a question of time until he has to take up something else. If the latter is the case I am confident that it will be best for Radin to meet the inevitable promptly.

I very much wish you would see your way to take the case up frankly with Laufer, Wissler, Goddard, Boas, and Dixon, and ascertain what they feel inclined to do.<sup>1</sup> There is no doubt, I think, but that Radin is pretty generally discredited--always excepting yourself--in spite of the recognition of his ability. If no one in authority wishes to touch him, there is no use in our half starving him for a year or two only to have him go under anyhow at last. It seems to me that if any institution is willing to take him on its permanent staff--and he can be had at bargain rates, of course--this is the time in justice to the man to do so. If all of them have their faces resolutely set against him, he on the other hand should know the situation.

If it were that you or anyone else were eager to secure Radin and unable to do so at the present, but reasonably sure of having an opening for him in a year or two, I shall be very glad to do my level best to take care of him in the interim. It seems to me that the way the general situation is now shaping itself, there is no doubt but that in two years the present obstacle to Radin's continuance will have disappeared. As to the situation of your bureau and your own personal attitude, you yourself must however speak.

I feel extremely sorry for Radin, for I recognize both his ability as a worker, and his fine traits of temperament. At the same time I cannot but feel that his troubles are ultimately wholly of his own making. He has a certain indecisiveness of character that arouses a sense of distrust or repugnance in most people.

Incidentally, I would like to remind you of a cripple for whom I already feel a certain moral responsibility and whose case makes me reluctant to take on Radin if it is not essential to the latter's preservation. I mean Wallis.<sup>2</sup> He knew, of course, that he was taking unfavorable chances as to permanance when he came here, but a mistake in judgment is no reason why an excellent man should be in danger of being lost to the science. So far as income is concerned, Wallis is very comfortably off at his high school in Fresno, but the work is strenuous and I fear altogether too heavy for his sensitive personality to carry very long.

I wish you would let me know what you believe as to the prospects of both men.

Allow me to extend my congratulations on number three, whose arrival I learned from your recent letter to Gifford.<sup>3</sup>

The latter, by-the-way, is making some remarkable discoveries in southern California.

With best wishes for 1917 and my regards to Mrs. Sapir, whom I would like you to tell that I have not yet given up hope of meeting her in person some day, I am,

Sincerely yours,

A. L. Kroeber

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1 These were the men who (together with Kroeber and Sapir) were in effective control of the anthropological institutions of the United States and Canada. Laufer was Curator at the Field Museum; Wissler was head of the Anthropology Department at the American Museum of Natural History; Goddard was editor of the American Anthropologist; Boas in charge of teaching and research at Columbia; and Dixon was Professor at Harvard and Curator at the Peabody Museum. Only the Bureau of American Ethnology is omitted--an institution from which Radin had already been dismissed.

2 W. D. Wallis, who had taught at Berkeley during Kroeber's sabbatical in 1915-16.

3 Philip Sapir, the Sapirs' third child.

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Affiliated Colleges

San Francisco, December 30, 1916

Dear Sapir:

One of our graduate students, Leona Cope, is making a very promising study of native calendar systems north of Mexico. The literature is immensely scattered and most of the printed material is inadequately brief and frequently vague on the essential points, but certain conclusions are beginning to emerge. If you possess any unpublished notes which you would be willing to put at her disposal, I am sure that they would increase the value of her study. I enclose a brief list of topics on which information is most desirable.

Sincerely,

A. L. Kroeber

[Enclosure]

## Information desired on Indian Calendrical Systems

1. Tribe
2. The sequence of months in the year, with the meanings of the names.
3. Is this sequence ever recited differently?
4. Is there definite agreement as to the month that begins the series?
5. When in our year does this beginning fall?
6. Since there are approximately twelve and one third lunations in the solar year, neither twelve moons, nor as some tribes reckon, thirteen, will make even a rough calendar without some periodic adjustment. Are the Indians aware of this discrepancy, and if so how are adjustments made?
7. Is there any recognition of the solstices, a name for them, and do they enter into the reckoning of time? Any information on this point promises to be especially valuable.
8. By what method of observation, if any, are the solstices determined?
9. Is there any name for "month" other than the name for moon?
10. What are the names of the seasons with their meanings and what natural events are reckoned as marking their beginning and end? Is it certain that the names of all the seasons are aboriginal or is there a possibility that the Indians have taken on a modern set of designations based on our own? What is the literal meaning of the words for year, season, month, division of the month (if any), or any other period of time?

[215]

Geological Survey

Ottawa, January 9th, 1917

Dear Kroeber,-

I received your letter of December 28th only yesterday on my return to Ottawa from the New York meeting of the American Anthropological Association. While in New York, I learned from Goddard that you were writing him in the same vein. I must confess, I was rather astonished to learn from Goddard how much prejudice there was against Radin. I had never realized that Wissler and Goddard would, under no circumstances, recommend him for a museum post. With Dixon I did not have the opportunity to discuss the matter. As for Laufer, I had myself written him some time ago recommending Radin for a position that I understand he will be able to offer before very long. Before I learned that Radin could not be continued here, I had recommended Mechling for the same position, but I made it clear to Laufer later on that Radin was much the better man. I gathered from Laufer in New York, however, that he thought that Radin would not be happy in the kind of position that he was thinking of. The work required is not so much scientific as preparatory in character. Most of the work would have to be installation and label writing. Under those circumstances Radin would probably feel much more at home in Timbuctoo. I am very sorry indeed that it seems so difficult for Radin to get a permanent position. I thought I practically had the matter clinched here, but the war, with the consequent necessity of economising expenditures, also the unsympathetic attitude of our Deputy Minister towards anthropological matters in general, have knocked my plans sky high. As far as I myself am concerned, I shall always be most willing to exert whatever influence I can to have the permanent position that was originally intended for Radin declared; but it is impossible at the present time to tell whether or not it will be feasible after the war to do anything of the kind. The economy spell may well last a good many years after the war. As it is, our anthropological field work is now reduced to practically zero.



Of course, it does make one sick to realize that so many men of quite inferior ability are comfortably fixed, some of them in anthropological positions of considerable responsibility, when a man of the highest quality, like Radin, has to go begging for a livelihood. As usual in such cases, I doubt if anyone in particular is to blame. He simply has the misfortune to possess what in America is a moral crime--that is, temperament. If he were an intellectual machine, he would get along much better. As to whether it would be advisable for you to let him have the Fellowship so as to tide him over to some more favourable position later on, it would be impertinent for me to suggest anything. All I can say is that I shall continue to try, as opportunity presents itself, to get him a really satisfactory berth. After all, my own good will in the matter is the only thing of value that I can offer. Advice is rarely wanted, and experience teaches me that it is rather a useless undertaking to try to influence people who are prepossessed one way or the other.

With best wishes,

Yours sincerely,

E. Sapir.

P.S. One other point. I hear people talk of Radin's erratic ways and financial unreliability. As to the latter, I must be allowed quite uncompromisingly to state that our own experience flatly contradicts the hearsay. In all our dealings with Radin (and they cover a considerable number of years) we have had no financial misunderstanding whatever. Our treasurer, a most punctilious man, has told me more than once that Radin's accounts were always business-like and satisfactory! This, entre nous, does not seem to have been altogether the case with Goldenweiser. I would attach little or no importance to rumors about people's failings. They travel and gain volume quite without genuine knowledge of the pertinent facts.

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Affiliated Colleges

San Francisco, January 16, 1917

Dear Sapir:

I am very glad for Radin's sake that he has your entire good-will. I really believe that this is the only anchor that may save him from complete wreck so far as anthropology is concerned. I frankly would much rather not have him here. I will try to tide him over if it is necessary, but I do not wish to be the goat. If he is going to go under anyhow, I should feel much kinder giving him the finishing stroke than nursing him along in misery, not to mention our discomfort, for a year or two. I believe you are mistaken when you ascribe the prejudice against Radin to his having temperament. He has that, but his troubles are due to instability of character. Something is lacking which seems to make him quite incapable of dealing with people without inspiring distrust in them. I am not paying any attention to hearsay reports about integrity. I do think, however, that when a man inspires such a sentiment quite generally, there is a foundation for it; and even if there were not, the effect is the same. Radin certainly messes everything he touches. He is all the time breaking in where he is not wanted, and whether by indiscreet acts, by peddling gossip, or by some other means, he is always putting his foot into some unpleasantness. Limited as my contact with him has been, I can cite half a dozen instances.

I believe Radin to be a good hearted person and confess to considerable liking for him, but I am absolutely sure that I should much rather have no dealings with him of any but a purely social kind. I mention this so that you may understand why I asked you what I did in my last letter.

Sincerely yours,

A. L. Kroeber

[217]

Geological Survey

Ottawa, February 24th, 1917

Dear Kroeber,-

Under registered cover I am sending you the revised galley of my Hokan paper.<sup>1</sup> There are quite a number of Salinan, Karok, Shasta, and Achomawi inserts, based on manuscript material of Mason and Gifford. These inserts, I believe, materially increase the value of the paper, so that I should not like to have them ruled out if it is possible to keep them.

With best wishes,

Yours sincerely,

E. Sapir.

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1 "The Position of Yana in the Hokan Stock" (Sapir 1917a).

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Affiliated Colleges

San Francisco, March 1, 1917

Dear Sapir:

Under an exact interpretation of our editorial rules additions to matter in proof are counted as corrections and any excess over fifteen per cent of the cost of composition is charged to the author. I have not yet had your proof but if the quantity of corrections is considerable will try to have them exempted from the operation of this rule on the ground that they are necessitated by the material which has become available only since the submission of your paper.

I sent you sometime ago a brief questionnaire as to calendrical data in connection with the work of a graduate student who is making a promising study of this much neglected subject. If you believe Barbeau has anything that would be of interest and that he might be ready to spare I should be very glad

if you would forward the questions to him. There are endless lists of month names but exact and usable data as to the method of correction and the observation of fixed points such as the solstices, are very rare.

Sincerely yours,

A. L. Kroeber

[219]

Geological Survey

Ottawa, March 8th, 1917

Dear Kroeber,-

I am sending you a few calendric notes from my Nootka data, in answer to your request. I have other data in my notes for my second season of Nootka field work, but as I have not sorted out all of these notes as yet they are not so readily accessible. However, I shall look them up immediately and let you have them soon.

Yours very sincerely,

E. Sapir.

[220]

Geological Survey

Ottawa, March 9th, 1917

Dear Kroeber,-

The data that I am sending you enclosed were obtained later than the material that I sent you yesterday. They are doubtless more accurate on the whole. I have gone into particular pains to make clear the tremendous economic importance to the Indians of observing the solstices. I suppose that this is really one of the main points that your student would want to make. I believe that I have answered all the ten questions of your circular, as far as the Nootka are concerned, except no. 6, concerning which I am unable to

give any information. In regard to no. 10, I may say that the ideas of year, season, and month are expressed by certain suffixes. The suffixes for year and season are distinct, but evidently related, that is, year being apparently a derivative of that for season. The suffix for month means simply "round object", i.e. moon. It is precisely the same element that is used for dollar in numeral forms. The independent word for month is the same as that for moon and sun. Distinct from the word for sun is that for daylight, which is also used to mean weather and day.

With best wishes,

Yours sincerely,

E. Sapir.

[221]

Affiliated Colleges

San Francisco, March 13, 1917

Dear Sapir:

Thank you very much for the calendar notes attached to your letter March 8th.

Sincerely yours,

A. L. Kroeber

[222]

Affiliated Colleges

San Francisco, March 15, 1917

Dear Sapir:

Thanks for your two last lots of calendrical information. If more data of this quality had been taken, Miss Cope's study would be more productive than it is now going to be.

Sincerely yours,

A. L. Kroeber

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Geological Survey

Ottawa, March 22nd, 1917

Dear Kroeber,-

Under registered cover I am sending you further calendric data gathered from other members of our Division. The material consists of:

1. "Temporal Divisions and Units of Measurement", from W. H. Mechling's MS. work on Malecite and Micmac ethnology. Kindly return this when you are through with it.
2. Two Ojibwa calendars from Western Ontario, obtained by F. W. Waugh. You may keep these.
3. Extract from D. Jenness' MS. monograph on Copper Eskimo. You may keep this.
4. Information of Wyandot seasons, recorded by C. M. Barbeau. You may keep this.
5. Comparative information on Iroquois calendars, prepared by C. M. Barbeau. Kindly return this when you are through with it.

You will find recently published Iroquois information on pages 32 to 36 of F. W. Waugh's "Foods and Food Preparation" (Geological Survey, Memoir 86, Anthropological Series no. 12). This includes in summarized form no. 5 of above list.

Yours very sincerely,

E. Sapir.

[224]

Geological Survey  
Ottawa, April 3rd, 1917

Dear Kroeber,-

Under registered cover I am sending you the revised galley together with original galley of my Yana paper.

With best wishes,

Yours sincerely,  
E. Sapir.

[225]

Affiliated Colleges  
San Francisco, April 10, 1917

Dear Sapir:

Miss Cope asks me to return the manuscript data on calendar by Mechling on the Malecite and by Barbeau on the Iroquois, which you forwarded to me on March 22nd, and they are enclosed herewith. Please accept the best thanks of Miss Cope and of this department of the University of California for this assistance so generously rendered by these men as well as yourself, and express our appreciation to them. Full credit will of course be given for any of the material which Miss Cope uses.

Sincerely,  
A. L. Kroeber

[226]

Geological Survey

Ottawa, April 12th, 1917

Dear Kroeber,-

Under registered cover I am sending you the article on Yana terms of relationship that I promised you some time ago.<sup>1</sup> I hope you will find that this paper fills the bill. I may say that I would much rather have it published in your series than in the American Anthropologist, though you may find it rather too slight a thing for the series. In that event, if received in time, it may possibly be included under a single cover with the linguistic paper which is now in proof.<sup>2</sup>

Yours sincerely,

E. Sapir.

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1 See Letter 187.

2 It was published separately (Sapir 1918a), but as part of the same volume (13) of the UCPAAE series. Much of this volume was taken up with papers on Ishi, the Yana generally, and related topics. See Letter 228.

[227]

Geological Survey

Ottawa, April 17th, 1917

Dear Kroeber,-

Thank you for the manuscript material that you have returned. I am glad to learn that it has been of some use to Miss Cope. In glancing through Radin's Ojibwa manuscript, I find that he refers to some Ojibwa calendric data that may be of interest to Miss Cope, though it is likely enough that she has come across this reference by this time. The reference is to Peter Jones' "History of the Ojibway Indians", the calendric data being on pages 136 and 137.

Yours sincerely,

E. Sapir.



P.S. If the book is inaccessible out there, I would be pleased to send you a transcript, should you care to have it.

[228]

Affiliated Colleges

San Francisco, May 21, 1917

D[ear] Sapir:

Your relationship paper has been filed with the University Press. Waterman having gone into the Army, there is not a single editorial correction. We are setting aside volume 13 of our series for papers on the Yana. Number 1 is your Hokaan essay. Number 2 will be an historical summary of the Yana and American relations by Waterman. Three is by Pope on Archery and 4 is your relationship terms. I am planning to write as soon as possible a little personal history of Ishi which would make number 5<sup>1</sup> and should be delighted if we could count within a reasonable period on having from you a big number 6 on texts or morphology to close the volume.

I recently found it necessary to go over literature on Oregon and Washington before writing a final chapter on culture areas for my California book. In this connection I went for the first time systematically through your Takelma ethnological notes<sup>2</sup> and am glad to tell you that I agree entirely with what you indicate and what A. B. Lewis had previously suggested,<sup>3</sup> namely that southwestern Oregon belongs with northwestern California. I see in them only a single sub-culture area with its formation on the lower Klamath and the Rogue and upper Umpqua drainage historically dependent on this center. Your little Takelma sketch, brief as it is, is worth more to prove anything with than all the rest of the literature put together. Not only do I put the Takelma and Shasta on the same basis of tributary relation to the Yurok, but they seem to me very similar.

Radin has enough in his new position at Mills College<sup>4</sup> to live on, but no margin. I was hoping to get him our summer school but the Dean is discouraged

at the prospective attendance and at present seems inclined to avail himself of Waterman's being away to drop the work altogether.

Sincerely yours,

A. L. Kroeber

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- 1 Not written. Instead, Dr. S. T. Pope contributed a paper on Ishi's medical history (Pope 1920).
  - 2 Sapir 1907b.
  - 3 In Tribes of the Columbia Valley and the Coast of Washington and Oregon (Lewis 1906), pp. 175-78.--Albert B. Lewis (1867-1940), a biologist who switched to anthropology, took his Ph.D. at Columbia in 1907. He was on the staff of the Field Museum and specialized in Melanesian ethnology.
  - 4 A women's college in Oakland, California.

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Geological Survey

Ottawa, May 21st, 1917

Dear Kroeber,-

In regard to Waterman's letter to you, which I am returning to you enclosed, I may say that there can be no talk of "a lot of geographical information from Ishi". I presume that Mr. D. B. Lyon, whom Waterman refers to, is the man that I spent something like an hour all told with together with Ishi. Most of that time was consumed in his trying to explain to Ishi certain incidents in his own experience in connection with Ishi's tribe. Incidentally I succeeded in noting down a few geographical terms. As I remember it, he was of service in explaining to Ishi what geographical features were referred to. I sent you these terms a long time ago; I believe they were on a blue slip of paper. At no time did I seriously take up the matter of ethno-geography with Ishi, as I understood that that had been fully attended to by Waterman and yourself. I would have been glad to go over what material there was with Ishi,

but as this was not suggested and there were other things to do, I did not take up this work at all.

With best wishes,

Yours sincerely,

E. Sapir.

P.S. Some time ago I sent you a paper on Yana relationship terms. Please let me know if you have received it.

[230]

Ottawa, Ont.

June 11, 1917

Dear Kroeber,

Just got your "California Kinship Systems"<sup>1</sup> and have very rapidly glanced at Yurok. Certainly smells Algonkin in structure. Are you thinking of following it up? Note, offhand,

Yurok tsits voc. "younger brother or sister"

Blackfoot stem -sis- dit. (both meanings)

You remember I showed that Algonkin s sometimes goes back to Wiyot-Yurok ts, tc. Some one should take up Yurok-Algonkin relationship terms thoroughly, both as regards linguistics and system.

Yours sincerely,

E. Sapir.

P.S. Note N. Paiute yahi "father-in-law, mother-in-law" : S. Yana yāhi "father-, mother-, son-, daughter-in-law". Borrowing, I suppose, of N. Paiute from Hoka.<sup>2</sup>

1 Kroeber 1917c

2 But see Letter 231, postscript.

[231]

Ottawa, Ont.

June 12, '17

Dear Kroeber,

I wish I had seen your Mohave kinship terms<sup>1</sup> before finishing off my Yana-Hokan paper. I should have been able to add 4 or 5 entries, 3 of them specifically Yuman-Yana, as far as data go. Of very special interest is:

Mohave a'ava-k "son's child"

: Yahi 'awāwi "father's father, man's son's child" (-wi as in 'amāwi and 'a'djuwi, and apparently corresponding to Moh. -u- of n-apau-k, n-akweu-k, n-amau-k; in fact, Yahi 'amāwi comes closer to Mohave n-amau-k than to any of its other recorded cognates)

Note Yahi 'a- : Moh. a'a- as in Yana 'au- "fire" : Moh. a'auva. --However, I suppose extra Yana-Hokan cognates will accumulate from time to time; I can publish them all together some time.

The use of "older" and "younger" in cousins as determined by ancestors, not themselves, is, of course, not peculiar to Mohave. I have recorded this peculiarity for Takelma and Nootka. (In Nootka, indeed, this fact is at the very basis of their whole system of rank and inheritance of privileges. Significantly enough, the Chinook Jargon term for "chief", tyee, is merely Nootka (tāyí') for "male's older brother, in widest sense".)

As for the Mohave generation-skipping habit ("great-grandfather" = "younger brother"), cf. Nootka "younger sister" for "man's great-granddaughter". This is doubtless a purely "psychological" problem.

Sincerely,

E. Sapir.

P.S. I was probably hasty about N. Paiute yahi. I did not at first notice cognates in Kawalisu and my own Ute. If there has been any borrowing, it is more likely to have been from Shoshonean.

P.P.S. I am much pleased with Luiseño kung "husband" (similarly in Tübatulabal, I believe). In my Uto-Aztekan paper I combined Cora kin (Tepecano kun I believe) and S. Paiute quma- by positing original \*kuna-,

which regularly has its ŋ become Cora n and which was apparently labially assimilated in Plateau Shoshonean (-uŋ- > -um-).

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1 In "California Kinship Systems" (Kroeber 1917c).

[232]

Affiliated Colleges

San Francisco, June 14, 1917

Dear Sapir:

I do not expect to do anything on the connection between Yurok and Algonkin.<sup>1</sup> Gifford is now in the field getting kinship terms along the coast section from San Francisco to the northern end of the state. He writes me that he has Wiyot already and that it is of a different type from anything he has so far found and on the whole similar to what I got among the Yurok. He will get the Yurok fresh.

With what is already in print Gifford will have kinship systems for every group of consequence in the state when he gets through with his present trip and as you know he gets them quite exhaustively. Among others he should have systems of from four to six Athabascan dialects.

I sent off my California book a couple of days ago<sup>2</sup> and it may interest you that I am going to take home to-day the notes for a beginning of a presentation of Penutian. Dixon and I had planned to present our Hokan as well as Penutian vocabularies, but since much of the material that was available to us on Hokan is already in print and your paper which is on the point of appearing here seems to me to clinch the question of relationship, I am planning to suggest to Dixon that we confine ourselves to Penutian. I have no very great stomach for this work but feel that we owe it to the world if not to ourselves to make good the claims that we have left unsupported so long.

I suppose you have heard that Lowie<sup>3</sup> is to be with us next year in place of Waterman who has gone in for being an officer, and that I shall be in Wissler's service for the first six months of 1918.<sup>4</sup>

Sincerely yours,

A. L. Kroeber

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- 1 I.e., resemblances in kinship terminology. See Letter 230.
  - 2 Handbook of the Indians of California (Kroeber 1925a).
  - 3 Robert H. Lowie (1883-1957).--Lowie had received his Ph.D. at Columbia in 1908, and since that time had held a curatorial position at the American Museum of Natural History. He was invited to Berkeley for the academic year 1917-18 as a visiting associate professor, filling the vacancy created by Waterman, who had resigned to join the Army. After this teaching stint Lowie returned to the American Museum, but he came back to Berkeley in 1921 for a permanent appointment. Lowie was a close personal friend of Radin's, and also on good terms with Sapir (see Sapir 1965).
  - 4 Kroeber took leave from California during the spring semester of 1917-18 to work at the American Museum of Natural History. He was President of the American Anthropological Association for 1918, and it is likely that he felt the need to be in closer touch with colleagues in the East.

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Ottawa, Ont.

June 14, '17

Dear Kroeber,

Don't mind my garrulousness. I like to let off steam sometimes. You may remember that long ago I ventured to suggest that Penutian included not only Takelma and Coos-Siuslaw (to which Frachtenberg adds now Yakonan and Kalapuya), but also Chinook and Tsimshian.<sup>1</sup> Frachtenberg has independently come to believe that Chinook belongs with Coos-Siuslaw-Yakonan-Kalapuya<sup>2</sup> and was much surprised when I told him I had already decided it was to be grouped with Takelma, Coos, and Lower Umpqua. It is Tsimshian I want to talk about now.

In your Yokuts kinship terms<sup>3</sup> you speak of prefixed n-, which seems to disappear in vocatives (Yauelmani n-opop "father", Yaudanchi voc. opo-yo --

-yo probably equivalent to -ya in voc. isha-ya "mothers). Now this is precisely true of Tsimshian terms of relationship, e.g. Nass R. nɪyɛ'e<sup>i</sup> "my grandfather" (-e<sup>i</sup> "my"), voc. yɛ'e<sup>e</sup>. There are quite a few Tsimshian n- terms of relationship. Possibly in both Yokuts and Tsimshian this n- goes back to an old, old first person singular possessive prefix which later lost its formal significance. We may have here the vestige of a special class of possessive affixes used only for terms of relationship (this is brilliantly confirmed by Tak. wi- "my" only for terms of relationship, all other possessive and verbal pronominal elements being suffixed; still more strikingly by Chinook, in which prefixed -k-, -tc- is used for "my" except in "father" and "mother", where -n-, as in verbs, is prefixed, e.g. wa-n-aq-c "the fem.-my-mother"). What it all boils down to, I think, is that possessives in these languages are objective in form except originally (only in small part now) in terms of relationship, where subjective forms were used (note that in Nass R. -e<sup>i</sup> "my; me" contrasts with n- "I"). This analysis implies, then, that such a form as Nass R. nɪyɛ'e<sup>i</sup> is really to be analyzed as "I-grandfather-my", "I-grandfather" (original form for "my grandfather") having long lost its pronominal significance and having become generalized as non-vocative "grandfather".

But there seem to be actual Yokuts-Tsimshian cognates for terms of relationship besides! Note:

1. Yokuts n-atet "father"  
Nass R. voc. häd-ɪ'ɪ<sup>i</sup> (-ɪ'ɪ<sup>i</sup> "my") (said only by woman)
2. Yokuts (Yauelmani) n-opop "father"  
Nass R. voc. pä'p' (said only by man)  
Note. I wonder if the Yokuts doublets n-opop and n-atet do not rest originally on a difference of sex in the speaker.
3. Yokuts (Yauelmani) no'om "mother"  
Nass R. nɔ'-'ɪ<sup>i</sup> "my mother", voc. nä'ä
4. Yokuts (Yauelmani) ts'utsa "woman's daughter's child", originally also "mother's mother"

Nass R. nts'e<sup>h</sup>ts'-i' "my mother's or father's mother", voc. tsi'ts (without n- and with unglottalized ts'; ts in voc. for ts' doubtless due to children's language--perhaps Yaudanchi t'uta for ts'utsa is of similar origin)

? 5. Yokuts na-hamish "father-in-law"

Nass R. lämc "father-in-law, mother-in-law"

? 6. Yokuts bap "father's mother"

Nass R. nəbēb-i' "my mother's brother", voc. bi'p'

I can hardly believe that all the Yokuts-Tsimshian evidence here given, in view of much other evidence for the Penutian affiliation of Tsimshian, is purely accidental. I expect to publish my Nass R. terms and may refer to the Yokuts resemblances.<sup>4</sup>

Yours sincerely,

E. Sapir.

1 Letter 175.

2 Frachtenberg published his paper on these relationships in 1918. Sapir is referring to correspondence with Frachtenberg.

3 In "California Kinship Systems" (Kroeber 1917c).

4 In this paper on Nass River kinship terms (1920b) Sapir did not cite these Yokuts forms. He did however suggest that the n- prefix was an old subject pronoun, and noted that he hoped "at some future time to adduce certain comparative linguistic evidence that serves materially to strengthen" this hypothesis.

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Hotel Rowardennan  
Ben Lomond, California  
7-24-17

Dear Sapir:

I think you're barking up the wrong tree. When you don't like a fellow's stuff you say he's only got form. How can you see less content or more form in either Shaw or Wilde? Only a different content.



You see that Radin has been reading me your letters. The above [criticism] is by his permission.

I am most interested in what you say about language soul or consciousness. Only I think you ought to put more of them before us. There are very few people who could have worked out the Uto-Aztekan mora.<sup>1</sup> Boas did, but it meant so little to him that he didn't publish it; there wasn't A.B.C. enough about it, I suppose. I sensed there was something but didn't have nimbleness and control enough to dig it out. But how many people are going to read your paper in the next thirty years? And of these how many will have the modicum of imagination to know it means anything more than a useful rule? I think it's up to you to write a paper that will drive the significance of mora and glottal stop and the traits of Uto-Aztekan soul home to a larger group--something à la Huxley--the real goods, but even the finest inessentials suppressed, so that fools included must take notice. At least, some of us would make enough noise to drive it into the fools that there was something doing. Now they shrug their shoulders and say "technical."

By Jove, old man, if I had your knowledge and power of assimilation and skill in handling the damn brute material, I'd have cut you out in reputation as a linguistician long ago. At least I'm not afraid to try. You had no business to leave the little incorporation thing lie undone until I jolted you into it.<sup>2</sup> Since then you've probably slept on half a dozen like it. If I had half your philological wits I'd have five times your place and influence in the philological world.

As to the Superorganic,<sup>3</sup> I've left absolutely everything to the individual that any one can claim who will admit the social at all. That was my point and the history of the paper: I tried to reclaim the social by granting the individual everything that could be asked for him and still emerging with a residuum.

As to metaphysics, which Radin and Elsie Parsons<sup>4</sup> and Haeberlin<sup>5</sup> and even Goldy charge me with, forget it. I'd be willing, but I couldn't if I tried. No more than composing a tune. Where you see philosophy there's only awkwardness of abstract expression. What misleads you is merely that you fall back on the social at such occasional times as you're through with the individual;

whereas I insist on an unqualified place, an actuality, for the social at all times. We all base on my point of view in our concrete work; but we're not accustomed to the formulation. Result, it strikes you as outré, and you say "metaphysics." I'll admit it's a jump; but it's a jump we've all made unconsciously in practice long ago. The only difference is that I have a passion for beating the angel to it on the rush and then making him tread where I tell him to.

And there's the inconsistency, but an inevitable one, in all my so-called methodological writings. They're not intended for anthropologists, who nearly all follow perfectly good method and are often keener and usually better informed than I. I'm trying to reach public opinion. I can't print what I can write in the Atlantic Monthly or the New Republic or a philosophical journal; or if I did, there wouldn't be the least effect. I've got to hit general sentiment, if I hit it at all, though our profession. I'm tired of anthropology being a charity orphan allowed to pick up a profusion of scraps until biologists or geographers or psychologists or Madison Grants<sup>6</sup> take a fancy to having them again. If there's nothing social, we've got no business and had best retire. If there is, we have a place, and it's up to us to maintain it. And without apology. And with all the respect extended our work that it merits. We don't get respect now: we get kindness and tolerance. And I'm fed up on it. That's all the "philosophy" that's in me.

If you don't agree with me, the greatest favor you can do me is to say so publicly.<sup>7</sup> I don't know whether I'm right or wrong (though I have some strong convictions on the subject), but I am sure that the only progress is by forcing issues to a head.

Kroeber

Address as usual.

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1 Sapir 1913a:385-6. See Letter 150.

2 Kroeber is referring to Sapir's paper on "The Problem of Noun Incorporation in American Languages" (1911d), which had been stimulated by an earlier paper of Kroeber's (1910b). See Letters 71-4.

3 Kroeber had recently published a paper in the American Anthropologist (1917d) claiming "superorganic" status for socio-cultural phenomena (i.e.,

they should not be reduced to psychological or organic processes). Sapir had evidently expressed to Radin his reservations about Kroeber's formulation, which seemed to deny the importance of individual behavior in social explanation.

- 4 Elsie Clews Parsons (1875-1941), a wealthy individualist who came to anthropology at the age of 40 after an earlier career in sociology. Her anthropological research focused on the Southwest, and she generously supported the fieldwork of students and colleagues. See Spier and Kroeber 1943.
- 5 Hermann K. Haeberlin (1891-1918), a German-American anthropologist who was closely identified with Boas at this period. He was interested in cultural psychology, and was involved in several field projects (most importantly on the Northwest Coast) at the time of his early death. See Boas 1919.
- 6 Madison Grant, author of The Passing of the Great Race (1916), a racist tract. For a discussion of the "scientific" reaction against cultural anthropology, see Stocking 1968:270-307.
- 7 Sapir's public reply to Kroeber, "Do We Need A 'Superorganic'?" (1917c), was published in a subsequent issue of the American Anthropologist.

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[San Francisco]

8-21-17

Dear Sapir:

What you say now isn't on the basis of content and form, and I'm in the fullest accord with it. Shaw may be shy, or one of the damned Puritan herd, or just an intellect without personality--it doesn't matter: he doesn't write any personality into his work. His characters are faultless, but nobody can give a damn for one of them because Shaw doesn't. Whether he's alive in fifty years or not I'm not much interested in, because that's a question of the value of a method I'm not practising. He's fun now at any rate.

I saw two of your verses in the expiring Mnaret.<sup>1</sup> I wish I could tell you I liked them. I find it hard to say anything about any free verse. The cadences are very easy to get--so easy in fact that I suspect their value as an instrument of consequence--but the sentences are too much for me, in their ultra-conversational involutions. I'm so busy trying to hold the thought that runs through the chopped twistings that I'm like the old lady that doesn't get

any sport out of a novel because of the labor of spelling words out of letters, and prefers passages out of the book of prayer which she knows by heart. Radin has brought a professional [illegible] in and yesterday he showed some of his stuff, which appeared to have ideas, but I had to lie down flat, I was so busy dodging in and out of the syncopated rhythms that I couldn't tell if there was a melody. Perhaps my fault, and I plead innocence to the particular style; but perhaps also the result of an ultra-mannered style, so far as I can tell.<sup>2</sup> I should say from your two examples that your interest is as yet wholly in the trick of composition. I suspect when the technique becomes a vehicle instead of the end, you won't be writing about moths and star harmonies. And please--leave out God. I know that's the way it's done, but it's not your way.

If I said "duty" I was wrong. I'll change it to "opportunity." You can make the whole damn bunch of philologists--and there are thousands of them and they hold an assured repute in the world's eyes--either stand on their heads or get up and toe the mark. If that isn't worth while, there's absolutely nothing in the whole linguistics game, and it's only irresolution that holds you from shucking it. I'm not arguing for the thing the way it's done but as it might be done. I know that if I had half your mobilization for the campaign I'd make a stab at it. If Meillet<sup>3</sup> were only a little younger and could have five years in Africa or America he might get away with it. There's nobody else in sight but you. Yet if you wait ten or fifteen years, there'll be some youngster jumping the claim you've let lie. Paiute morae are the way to do it--but you'll never get a hearing on them. The world wants a big canvas, a big subject, and a sweeping brush. You can handle it as finely--at least as precisely and deftly--as on Paiute, but the strokes must be a foot long. Isn't that a play worth while? Brush away a couple of dozen of the old parrotted "forms" like incorporation that you are weary of, put in their place as many subtleties of mora type--but on a world basis, not Uto-Aztekan or Indo-Germanic--and I don't care how dense the most philological dunce in Germany or America is, fifty years from now, he'll be working as you direct now. He'll parrot again of course, but he'll be spoiling Sapir's technique instead of Brugmann's. And it strikes me it's some sport making him do that.

Amen to what you voice on ethnology. The same applies there. I recommend linguistics only because it's your first love and therefore probably your most natural medium.

I offered to Lowie to review your Time Perspective<sup>4</sup> and then passed it on to Radin. I can't review. If you don't know it, read the only one I've done in 10 years--Teggart's Prolegomena.<sup>5</sup> It takes the judicial temperament--and I was built for a prosecuting attorney. I'd only quote from your paper or banalize it.

Final. I don't say you have no sense of duty. I've never thought about it. I certainly hope you haven't very much. I do doubt whether you have determination--in plain English, courage--enough to win the whole stakes that fortune has laid before your "individuality."

A. L. K.

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- 1 The Minaret, a literary journal, ceased publication after the issue (June, 1917) that contained Sapir's poems, "The Moth" and "The Music of the Spheres".
  - 2 It is unclear whether this refers to poetry or music; probably the latter.
  - 3 Antoine Meillet (1866-1936), French Indo-Europeanist and general linguist, known for his breadth of erudition and his concern with the sociology of language.
  - 4 Time Perspective in Aboriginal American Culture: A Study in Method (Sapir 1916b), an expanded version of the paper given at Berkeley in 1915 (see Letters 155-160). Lowie was at this time the Associate Editor (in charge of reviews) of the American Anthropologist. In the end, Lowie himself reviewed the work (Lowie 1919).
  - 5 Review of Frederick J. Teggart, Prolegomena to History (Kroeber 1917e).

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Geological Survey

Ottawa, September 21st, 1917

Mr dear Dr. Kroeber,

Some time ago I received a bill from the regents of the University of California for \$2.76 to pay postage on my separates of "The Position of Yana

in the Hohan Family". Just now I have received a further bill for \$1.35 to cover cost of corrections. I have paid the former and shall, of course, pay the latter also. I trust, however, you will pardon me if I point out to you that I consider bills of this kind as rather curious affairs. I do not think I have ever heard of any reputable organization, that does not pay for scientific contributions, demanding a refund on postage of separates. Furthermore, in view of the fact that all corrections made were not of a stylistic nature, but purely with a view to increasing the scientific accuracy and usefulness of the paper and therefore of adding to the reputability of the scientific series itself, it does seem somewhat steep that an author should be penalized for taking his work a bit seriously.

Please do not misunderstand me: I do not at all wish you to have any exceptions made in my favor, nor am I seeking to get out of this and future charges of a similar nature; I merely wish to point out that the policy of the Editorial Committee of your University strikes me as being on the canny side of liberality.

With best wishes,

Yours sincerely,

E. Sapir.

[The following handwritten note is attached.]

Dear Kroeber,

The postage bill was paid by cheque (\$2.76) on Aug. 9th; the bill for corrections also by cheque (\$1.37) on Sept. 14th. I did not think it would be good policy to protest the payment of particular bills but rather the policy followed by the University of charging for postage and corrections. It seemed illogical to me for a non-commercial institution to adopt commercial methods when it happened to be to their advantage.

Yours sincerely,

E. S.

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Affiliated Colleges

San Francisco, October 11, 1917

Dear Sapir:

I am very pleased you put your notice about Washo in the last Anthropologist.<sup>1</sup>

I recently finished and sent off to Dixon my part of our joint paper on relationship. I think I wrote you that I had left out Hokan as being covered by your recent paper in our series. At the last minute, however, it struck me as misleading to omit Washo when there was every prospect of its tying up to everyone's satisfaction in a short time. I therefore put together all the readily available evidence on Washo and Hokan and included it in the manuscript together with a reference about Harrington's announcement<sup>2</sup> and a mention of the fact that you had expressed yourself privately to me to the same effect more than a year before.<sup>3</sup>

After the manuscript was on its way to Dixon I recalled what I should have remembered before, namely: that Dixon once got some Washo and that the available data on the language were, therefore, undoubtedly larger than the little I had published. I asked Dixon to send me his material. It has just come and I think that the note book contains probably twice as much vocabulary as I secured and a number of sentences.

Now it seems to me very unwise for both us and you to prove the same point independently and about simultaneously, not to speak of Harrington perhaps making it a triangular affair. If you would rather be relieved of the job of proving Washo to be Hokan, Dixon and I will go ahead. Personally, however, I am confident that you will make a much better job of it and in addition I should be somewhat relieved to be excused from the task of going over Dixon's new material. Having finished the manuscript I would much rather let it stay finished or cut something out of it.

If you will handle Washo I shall accordingly be very glad to suggest to Dixon that we omit our Washo section and that he put his Washo note book at your disposal. All I want in our paper is to put the language where it

belongs. I think we could accomplish this by saying that you had started the idea, that we had looked into the matter, and compiled sufficient evidence to convince ourselves but that as you were going into the matter we had contributed our results to you.

Will you let me know your wishes in this matter?

I am wondering whether your sentence on Yuki means that you have something up your sleeve.<sup>4</sup> As a general probability I feel exactly as you do but admit to be completely baffled. If the connection is with anything in California it should undoubtedly be with Penutian on the basis of structure. But I recently went over the material again and obtained only negative results.

As to Lutuami<sup>5</sup> I have absolutely nothing. I have never gone into it and don't want to. It is a hundred to one shot that it links up with something. That is all I was trying to tell Gifford.

Sincerely yours,

A. L. Kroeber

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- 1 "The Status of Washo" (Sapir 1917b).
  - 2 An announcement (with no supporting evidence) of his discovery of a relationship between Chumash and Washo (Harrington 1917).
  - 3 See Letters 174 and 197.
  - 4 At the end of "The Status of Washo" Sapir had written: "The elimination of Washo leaves Yuki as the only Californian language that can be called 'isolated'. It is hardly to be expected that this privilege can be allowed Yuki indefinitely" (Sapir 1917b:450).
  - 5 I.e., Klamath-Modoc.

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Affiliated Colleges

San Francisco, October 16, 1917

Dear Sapir:

I had a talk with the new manager of our University press, who has a faculty of seeing the human side of business matters, and is in rather full



accord with your position and mine, in the matter of the charges, but has his hands pretty well tied by the rules of the editorial committee.

As to corrections, I think there is this to be said. Many of our authors have difficulty making up their minds and unless restrained by charges against this will run up enormous and quite unnecessary expenditures. The only way to hold them in check is not only to make a rule but to enforce it. For many years while I was on the editorial committee the rule was in effect but rarely applied and the improvement in the number of corrections was marked as soon as application became rigid. It undoubtedly happened, now and then, that a conscientious and careful author is caught as you were but the amount in such cases is usually nominal.

As regards separates, I am wholly of your opinion,--that an institution that does not pay should not charge. I have recently twice been charged expressage on separates and in each case have resented it as an imposition. Here again, however, it is a matter of a rule, though as this rule works easily on members of the University and bears almost wholly on outsiders who honor them with their contributions, I believe it to be an unjust and unfortunate rule.

The manager of the press wished me to explain to you that the reason you were sent a bill for postage is that parcel post costs less than expressage, and the idea was to give you the lowest, possible rate as long as you had to be charged.

All I have been able to secure is an understanding with the manager that in the case of future charges he will take the matter up with me before mechanically sending you a bill. Before a matter has got into the channel of official red tape I may be able to devise an adjustment. If by chance any bills should be sent you from the office hereafter, please do not pay them, but advise me.

Sincerely yours,  
A. K. Kroeber

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Geological Survey

Ottawa, October 17th, 1917

Dear Kroeber,-

By all means let us pool resources. I am very glad indeed to be relieved of the necessity of doing the Washo part of the work. With the utmost cheerfulness, therefore, I am sending you, under registered cover, my cards covering lexical resemblances both in radical elements and in prefixes and suffixes. The general morphological resemblances, which are perhaps even more important, I have never written down: they are mostly in my head or could easily be verified by a little looking up of source material. There are also a number of phonetic laws that I have worked out that are implicit in the material that I am sending you. I believe I wrote you in regard to some of these quite some time ago. What you might do is to let me have a look at galley of that part of your paper which deals with Washo, so that I might have a final opportunity to correct any possible misunderstandings on your part in the use of my material or to suggest further particulars that you might seem to have omitted. Perhaps you will not thank me for being so ready to shelve everything on you, but the truth is that I have no end of other work to do. Presumably Harrington, with his much fuller knowledge of Chumash and Yuman than I, and possibly you, possess, will be able later on to make the most satisfactory demonstration.

Frachtenberg has probably the last word on Lutuami. I understood from him, when he last wrote me about it, that there was no reasonable doubt that it linked up satisfactorily with Sahaptian and Molale. I am afraid I have nothing definite on Yuki. The sentence referring to Yuki in my recent note was merely a statement on general principles, and therefore methodologically unjustifiable!

From your silence as to my answer to your article on "The Superorganic",<sup>1</sup> I judge that you have some whopping rejoinder up your sleeve with which to

put the quietus on me in the next number of the Anthropologist. With best wishes,

Yours sincerely,

E. Sapir.

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- 1 Sapir's critical appraisal of Kroeber's paper, "Do We Need A 'Super-organic'?" (Sapir 1917c), appeared in the same issue of the American Anthropologist as his note on Washo. See Letters 242-6.

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Affiliated Colleges

San Francisco, October 22, 1917

Dear Sapir:

Yours of the 17th was received this morning.

"Vive sibling!" is right. Lowie surrendered without even a show a resistance. I have gone through my finished manuscript and changed "geschwister" to "sibling" and "siblings" everywhere.

I shall handle the matter of Cahuilla cross-cousins in the cautious manner which you suggest.<sup>1</sup>

I am working upon the Yuman kinship systems and am wondering if you would care to have a copy of the manuscript, since these are Hokan languages. It is very likely to be two years before the paper appears in print so if you desire these data for early comparison I shall be glad to have our stenographer type them in triplicate. The groups represented are Yuma, Kamia, Cocopa, southern Diegueño, and northern Diegueño.

We seem to have here in California a small area with matrilinear reckoning. Barrett records for certain of the Pomo that the chieftainship passes from a man to his sister's son. The kinship systems of the southern Pomo and Wappo which I gathered this summer give indication of matrilinear reckoning. The indication is in the cross-cousin terminology: these peoples counting the father's sister and all of her female descendants through females as "father's sister", as do the matrilinear Crow. The Wintun, Miwok, and Yokuts, on the

other hand, reckon the mother's brother's male descendants through males as "mother's brother", as do the Omaha, Oto, and Kansa. Lowie has correlated these differences in cross-cousin terminology with differences in descent among the Siouan tribes mentioned. Why should not the correlation hold in California?

Next week I visit the Wappo and southern Pomo again with the intention of obtaining information of a more general character, which I hope will definitely settle the question. It is rather interesting to find the matrilinear descent without clan organization.

Sincerely yours,

A. L. Kroeber

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1 This and the preceding paragraph contain references to a letter not preserved.

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Affiliated Colleges

San Francisco, October 23, 1917

Dear Sapir:

The thanks that go out to you from me for the bunch of Washo cards are, admittedly, half-hearted. I would much rather have turned over to you our considerably more slender material. And yet, I presume it is up to us to see this thing through, so I am taking it up with Dixon. I think I shall suggest to him that he let our resemblances stand and add yours as a separate list accredited to you. This will result in some duplication but I think will have the value of any double presentation independently arrived at. I should like, however, to reserve for ourselves the right of merging your list and our own in case ours proves too insignificant by the side of yours. I hope this arrangement will be satisfactory to you.

I disagree with your rejoinder,<sup>1</sup> but have nothing up my sleeve at present.

I sent an article to Small for the Sociological Review sometime ago, which when it comes out there, or elsewhere, may make my position clearer.<sup>2</sup>

I want to answer your criticisms and Goldenweiser's, as well as Haeberlin's earlier ones some time but would rather do so in connection with some new exposition.<sup>3</sup> Goldenweiser's criticisms seem to me to be very similar to yours. I am referring not so much to what he printed as to what he writes me. I feel tempted to begin to discuss with you now in this letter, but as I am hoping you will be at the Christmas meeting it will be more satisfactory to wait until then.

Sincerely yours,

A. L. Kroeber

- 
- 1 To his paper on "The Superorganic".
  - 2 "The Possibility of a Social Psychology" (Kroeber 1918a). The journal was the American Journal of Sociology, edited by Albion Small.
  - 3 Goldenweiser had also published a rejoinder to "The Superorganic" (Goldenweiser 1917), and Haeberlin (1915) had replied to an earlier paper of Kroeber's, "Eighteen Professions" (Kroeber 1915b).

[242]

Geological Survey

Ottawa, October 29th, 1917

Dear Kroeber,-

I knew that you would not be over-enthusiastic about the Washo material. What is to become of American linguistic work anyway? Is nobody genuinely interested in the stuff? Please use your own judgment in regard to the merging of your and my lists. I leave all matter of policy entirely to you. As I said in my last letter, though, I should like to see the galley, so as to correct possible misapprehensions, also to have the opportunity of pointing out resemblances of a morphological character that you may inadvertently have omitted.

Of course I did not expect to convince you with my reply to your article on the Superorganic. Nobody ever is convinced in these discussions. They are merely opportunities for different people to air themselves. Still they do

have the merit of sharpening one's own views. Differ with me as you may, it seems to me that, on the whole, we agree more closely than you do with either Haeberlin or Lowie. Perhaps this is an illusion on my part. But it does strike me that our common tendency is away from conceptual science and towards history. Both of us seem to want to keep psychology in its place as much as possible. I can not say, for instance, that I find that Lowie has succeeded in getting very clear ideas about the relationship between psychology and anthropology, if his first chapter in "Culture and Ethnology" is to be taken as a criterion.<sup>1</sup> This, undoubtedly the weakest part of the book, seems to indicate that he does not clearly recognize the difference between psychology as a science and what one might call the history of thought or of psychological attitudes. The latter is, of course, nothing but a particular aspect of culture-history. What the specific psychological attitude of an artist or of a tribe of Indians toward a given phenomenon is, is a purely historical fact, not a psychological one any more than a specific technical account of a building is mineralogy or chemistry. In both cases concepts are employed which are taken out of the quarry of conceptual science (material or psychic), but that is unavoidable. Every historical fact subsumes, in description, one or more scientific concept-groups, explicitly or implicitly. The real upshot of Lowie's trend, though he of course would not admit it himself, would be to reduce all culture-history to conceptual science. However, this letter is getting too long, and as I intend to take up matters of this kind in my Columbia University lecture in March, on the subject of "Is Anthropology a Science?", I had perhaps better say no more at present. I am afraid I shall not be able to be down at the Christmas meeting. Please do not hesitate to drop me opinions on culture-historical concepts from time to time, as they may help me to crystallize some of my own notions.

With best wishes,

Yours sincerely,

E. Sapir.

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<sup>1</sup> Lowie 1917.

[243]

[San Francisco]

11-4-17

Dear Sapir:

I'll see you get either ms. or galley of the Washo.

I ought to tell you that I long ago promised to Lowie to review your time perspective paper, then with his consent transferred it to Radin, but Paul is up to his old tricks. I am a poor reviewer--two to my credit in 13 years, and those badly done. I am reviewing Wissler's volume,<sup>1</sup> but that is mostly concrete, and so far as it has a problem, I in a sense started it. Your paper should be discussed, but I'm afraid I should only be able to say amen. However, it's my job if Radin falls down.<sup>2</sup>

I don't think your first criticism of the Superorganic<sup>3</sup> holds because it seems to me the contemporaneity of other phenomena of growth of culture also occurs. Modern inventions are merely easiest to use because they date. I hope some time to present the other sort--if there is no time establishable, I think there will be a sequence, or something similar that repeats. I think you will agree, and that something in my manner of statement repels you.

As to your second point, that the separation of psychic from cultural as all important is arbitrary, I admit subjective differences, but hold that these are characteristic of equally valid methods. The organic-psychic gap appeals to me as far less abysmal than the psychic-social. You may feel differently, and another would see chiefly the inorganic-organic split. I think we agree that to argue which deserves primacy would be philosophy; if mind exists at all the scope and methods of ethnology are definitely on the beyond side of it. I have a sharper formulation of this view in Albion Small's hands--it was written a year ago.

I find myself changing. My instinct is invariably toward the historical, and Lowie's insistence on the conceptual and causes and proofs jarred on me as late as my New York winter. Emotionally I despise proofs. But that is no reason why they should not be feasible and useful and interesting in the field of culture, provided we don't stretch them, and don't delude ourselves with

psychological surrogates. I think the "pattern" as distinct from the Tardian principle of "imitation" is an approach to a concept usable in cultural mechanics. I don't see that it gets us very far, but I no longer insist that because we have no such mechanics there can't be any. As usual, I went off half cock there,--though it seems to me saner to heighten doubt into such denial than to take illusions for reality, as has been the custom. I quarrel with Rivers now not for preaching determinism, but for claiming that he has achieved it. Since Lowie is here, I find that we are in closer agreement than I suspected in New York, where Goddard and Goldy used to bait him and egg us on. I think too his reconciliation of psychology and ethnology is a disappearing remnant; he admitted two years ago he held only part of what he had held.

However that may be, it's a safe bet my actual work will always be literature.

The decadence of linguistics is largely your own fault. You're an individualist and haven't built up a school. Do something general in character and you may get opposed. At least I promise you an opponent if you can make me disagree. I've urged your opportunity on you before. I don't think Paiute has to be done by any means. If you have anything broader, it's far more important. And I think you can have.

Kroeber

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- 1 The American Indian (Wissler 1917). Kroeber's review appeared in the American Anthropologist (Kroeber 1918b).
  - 2 Sapir's Time Perspective in Aboriginal American Culture (1916b) was finally reviewed by Lowie, after considerable delay (Lowie 1919).
  - 3 Kroeber is referring to Sapir's published reply to this paper (Sapir 1917c).



[244]

[no date--November, 1917?]

Dear Sapir:

I'm sorry about Frachtenberg if it's true.<sup>1</sup> Have you it authentically?

I've read Rickert--not the big heavy book, but the one of 150 pages.<sup>2</sup> I even had a rash moment of wanting Goddard to let me print translations of excerpts.<sup>3</sup> Lowie says all of Rickert is in Windelband.<sup>4</sup> That will probably annoy you. I don't yet get your point. I just don't see there is any less natural or more arbitrary basis for regarding cultural phenomena as such than for regarding psychic ones as such, or any kind. And real work is done only by keeping psychic emotions out of gravitation, affinities out of Chemistry, atoms and enzymes out of psychology. I don't give a red cent whether cultural phenomena have a reality of their own, as long as we treat them as if they had. You do, most of us do largely, but most of [us] hang back and fear to avow it and let geographers and biologists and Frazers<sup>5</sup> walk over us. If we're doing anything right, it deserves a place in the world. Let's take it, instead of being put in a corner. That's not metaphysics: it's blowing your horn and Goldy's and Haeberlin's and Wallis's, who think I'm trying to fasten something on you. My only moral ally is Goddard; first because he mistakenly sees a prospect of a dog fight, second because he abhors philosophy; and last because he has a keen professional sense. As to substance, he probably agrees less than any of you.

Old man, some people found schools by teaching, but most schools are set by example. Your Uhlenbeck reviews<sup>6</sup> are examples, but they're not going to conflagrate. They carry the stuff--all that's needed; but like Paiute, they're not going to get anybody excited. The same talent put on something ten times as startling would produce a hundred times the effect. I would like a "practical man", I know, but waste revolts me--even negative waste. I can see some opportunities I can't grasp. I've been trying for 15 years--nearly 20--to draw up that table of pronominal elements. If I had succeeded, I would

have invested it better than in a review. Properly dignified, it would have made a fine chapter of a great book.

Kroeber

- 1 Leo Frachtenberg's connection with the Bureau of American Ethnology was terminated on October 1, 1917. It was rumored (apparently with some basis in fact) that Frachtenberg was fired because he was a Jew of German (actually, Austrian) background, closely associated with Boas. A "scientific" reaction against "foreign" influences in American anthropology had been exacerbated by the nationalistic fervor of World War I, and it was to culminate in a public repudiation of Franz Boas at the Cambridge Meeting of the A.A.A. in December, 1919 (see Stocking 1968:270-307).
- 2 Sapir had referred to the work of the German social philosopher, Heinrich Rickert in a note at the end of his reply to Kroeber's "The Superorganic": "For a penetrating analysis of the fundamental distinction between historical and natural science I strongly urge all anthropologists, and social scientists generally, who are interested in method to refer to H. Rickert's difficult but masterly book on Die Grenzen der naturwissenschaftlichen Begriffsbildung; eine Einleitung in die historischen Wissenschaften [Rickert 1896-1902]. I have been greatly indebted to it." (Sapir 1917c: 447). Kroeber is apparently referring to the shorter Kulturwissenschaft und Naturwissenschaft (Rickert 1899).
- 3 I.e., in the American Anthropologist, of which Goddard was then Editor.
- 4 Wilhelm Windelband, another German social philosopher. Lowie apparently was referring to his Präludien (1915). For a lucid retrospective on the influence of Windelband, Rickert, and other neo-Kantians on Boasian anthropological theory see Lowie (1956:1006-8).
- 5 Sir James Frazer, author of The Golden Bough.
- 6 Reviews of two works on American Indian languages by the Dutch linguist C. C. Uhlenbeck, published by Sapir in the first issue of IJAL (Sapir 1917d-e). In these reviews Sapir proposed that the system of pronominal reference in some American Indian languages was best analysed in terms of a verbal typology foreshadowing the modern notion of "ergativity". Kroeber's assessment of the significance of these reviews was remarkably prescient.

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Affiliated Colleges

San Francisco, November 22, 1917

Dear Sapir:

Can you not throw together your Yana ethnological notes for our Yana volume<sup>1</sup> and get this material out of the way? I have just been revising the Yana chapter of my book<sup>2</sup> and am appalled at its slenderness.

I remember you put most of this material in shape for the Putnam volume six or seven years ago--I recall sending you photographs of all our specimens--and it would probably not take you more than a very few days to get the paper in printable shape. If your manuscript is in handwriting give it the necessary touches and send it on and we will do the typing.

I am not forgetting what I wrote you only a few days ago,<sup>3</sup> but am speaking from the conviction that this Yana ethnology represents a really very small job with an infinitely greater utility when completed.

Sincerely yours,

A. L. Kroeber

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1 Volume 13 of UCPAAE. See Letter 228.

2 Handbook of the Indians of California (Kroeber 1925a).

3 I.e., that Sapir should "write something general" (Letters 243-44).

[246]

[Ottawa]

November 28th, 1917

Dear Kroeber,

I have your letter of November 22nd, also your more personal letter of previous date. Let me answer your question in regard to Yana first. You are quite mistaken in saying that I could get my Yana ethnological material together in a very few days. This is quite out of the question. It would be a

matter of probably a few weeks' pretty solid work, at least for one that works as slowly as I do. I do not believe I have a quarter of it done yet, and it already takes up something like 50 pages of manuscript, about half of that being in type. I may be mistaken in my figures, but I do not believe that I am very far off. Of course the terms of relationship being off my hands now would reduce the size of the paper somewhat. What I am willing to do, however, is to finish the paper as soon as I get my Paiute grammar out of the way. This I expect to be by the first of the year.

All I know about Frachtenberg is what I have heard from Mechling. I have nothing new since I last wrote to you about the matter. It would really be too bad for them to lose him, as after all is said and done he is by far the best linguist that they have at the Bureau.

I don't know whether you got the real ideas in Rickert that I was referring to. The book that I mean is large and a rather ponderous one of seven or eight hundred pages.<sup>1</sup> It moves along very slowly and deliberately, but it is very closely reasoned throughout. Whether Windelband has all that Rickert has to say or not I do not know. His History of Philosophy is one of the books that I have been planning to read for many years, but I have never gotten around to it. The books that make up this class would constitute quite a library. I do not know which book of 150 pages you refer to. If it is Gegenstand der Erkenntniss, which I have also read, it is certainly not the thing that particularly bears on our subject. That book is devoted entirely to metaphysical problems and has a lot to say about phenomena of the absolute and that kind of business. Rickert's metaphysical standpoint is brought in more or less incidentally in the larger work that I refer to, but it is not really necessary to an understanding of his ideas in regard to science and history. What vastly amuses me about our argument is that I really agree with you more than you do with yourself, only that you quite fail to see it. I am not at all concerned with the idea of proving that cultural phenomena as such have a right to be studied as distinct from organic and psychological ones. I take this so much for granted that I would not even condescend to argue about it very much. The point that does interest me, however, is that the break, as you call it, between psychic and cultural phenomena is not of the

same order as that between organic and psychic. Perhaps the whole thing could be put into a nutshell, but only approximately, by saying that all cultural studies maintain themselves in direct adherence to specific time and place, whereas what I would call conceptual sciences, including psychology, are able to enunciate statements that are valid at unrelated places and times. The real point is that history evaluates experience as it is directly unfolded and thus is [in] a philosophical sense somewhat related to art; the sciences proper, if you can only get yourself to see it, do not really concern themselves with reality directly experienced. As to the necessity of our keeping ourselves methodologically free from psychological and biological entanglements, I more than agree with you.

You have been prodding me so long about linguistic matters that I suppose I shall have to get really mad and sit down and write a book or a series of papers eventually leading up to a book. Quite a number of years ago, as a matter of fact, I communicated with Gilbert Murray in regard to writing a more or less popular book on language for the Home University Library. I sent him a somewhat carefully worked out outline, with which he was very much pleased, but the publishers were apparently easing up on new volumes at that time and thought the outline somewhat technical. I still have a copy with me, and I am sending it for you to look over and criticize. If you can give me any suggestions, I should be grateful. Perhaps on the whole I should prefer to make it rather less popular than I should have at the time I speak of.<sup>2</sup>

With best wishes.

Yours very sincerely,

[E. Sapir]

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1 Rickert 1896-1902. See Letter 244.

2 Whether this outline for a popular book survives is not known. It is likely, however, that some of the ideas in it were reflected in Sapir's early general paper, "The History and Varieties of Human Speech" (1911c).

[247]

[Received Nov. 27, 1917]

Hey, hey: We thought you were a Canadian official and citizen. And you write from Ottawa about "this country" giving the Philippines away! My!<sup>1</sup>

Kroeber

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<sup>1</sup> A letter from Sapir, advocating the "internationalization" of the Philippine Islands, was published in The New Republic, November 3, 1917 (p. 23). Although he was not a political activist, he did not hesitate to voice his opinions, which were generally of a socialist and internationalist nature.

[248]

American Museum of Natural History<sup>1</sup>

New York City, 3-7-18

Dear Sapir:

Your visits to civilization having become almost as rare as mine, I hope you will give us a few days this time. On Monday the 25 the Ethnological Society meets.<sup>2</sup> I am counting on you for dinner before and a glass of beer afterwards, also the following day for lunch at the Endicott, Tuesday being the regular day for the Museum and Columbia to come together.<sup>3</sup> Rather, it was recently changed to Thursday, but we will change back that week. Also, if you're not already engaged, please consider yourself dated for lunch either Monday or Wednesday or both, if you are here.

Besides about 500 other things, and a lot of gossip, I want to talk about your magnum opus philologicum. I see something bigger than a semi-popular handbook. If you had sent me your book of verse, I would talk about that--critically, probably.

Yours,

Kroeber

- 
- 1 For Letters 248 through 256, dating from the first half of 1918, Kroeber was in New York.
  - 2 The annual meeting of the American Ethnological Society, in New York. This group was dominated by Boas and his students, and provided a congenial atmosphere for socializing.
  - 3 The anthropological staff of the American Museum regularly lunched with Boas and his colleagues at the Endicott Hotel on West 77th Street, opposite the Museum.

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American Anthropological Association  
[New York, N.Y.,] May 3, 1918

Dear Individualist

There won't be any aristocratic privilege enforced, though the gate is always open. Only Farabee joined you in voting no--I like the partnership!<sup>1</sup> But as the thing was left optional with Goddard, and he doesn't want to apply it, the rights of the minority will be more than respected. The main purpose was to rub the situation into people of your temper.

I have half a dozen design sets for you, but got ambitious and tackled a couple of artists, with the result that I now have neither results nor the original. I'll forward in time.<sup>2</sup>

Kroeber

- 
- 1 The reference is to a poll of the members of the Council of the A.A.A., conducted the previous month, regarding a proposal of Kroeber's. Rising costs had depleted the Association's treasury, and Kroeber wished to have the organization (through the American Anthropologist) solicit donations. William Farabee was a curator at the Peabody Museum, Harvard, and an academic conservative, hence the irony of his being the only 'nay' besides Sapir's.
  - 2 During their meeting in New York in March (see previous letter) Sapir had evidently discussed his plans for a study of the "form feeling" manifested by individuals in their elaboration of a simple graphic design. The "design sets" Kroeber mentions were the various elaborations of a standard design motif produced by a test subject. Kroeber had presumably volunteered

to solicit a number of such sets from intellectual and artistic acquaintances in New York and forward them to Sapir.

[250]

Ottawa, June 10th, 1918

Dear Kroeber,-

How about the author's copies of my recently published paper on "Yana Terms of Relationship"? So far I have not received anything from the University Press. Perhaps they have instituted a new policy?

Could you let me have the series of design developments that you spoke of some time ago? With best wishes,

Yours sincerely,

E. Sapir.

P.S. I was much interested in Frachtenberg's Kalapuya-Takelma cognates.<sup>1</sup> It looks very convincing to me. His Kalapuya-Chinook is of far less consequence, many of his only 19 cognates being patent borrowings. Thus, Chinook -cgan "cup" : Kal. u'skan is not valid, because Chinook o'-cgan is merely feminine of masc. i'-cgan "cedar"; similarly, Kal. u'lxayū "seal" is simply Chinook u'lxaiu borrowed bodily with its fem. prefix. The Kalapuya, not being a marine people, could hardly have a native word for "seal", anyway. All that does not matter, however. I have considerable independent testimony connecting Chinookan with Lower Umpqua, Coos, Takelma, and California Penutian. Now Kalapuya nicely fills out the chain of evidence.

1 In "Comparative Studies in Takelman, Kalapuyan and Chinookan Lexicography..." (Frachtenberg 1918). See Letter 233.



[251]

[New York, N.Y.]

6/27/18

Dear Sapir:

These were mostly done months ago.<sup>1</sup> I wanted to get you more, but Ichikawa<sup>2</sup> kept the element and delivered nothing. I think you should have some most interesting material when it is all together.

You have heard, I take it, that we gave Radin an instructorship for a year.<sup>3</sup> He already feels that I have betrayed him, but by next spring will hold me his worst enemy. The arrangement will solve neither our needs nor his permanently and I can only hope that it will give him some little harvest of income or rehabilitation.

A.L.K.

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- 1 This note apparently accompanied the design sets mentioned previously (Letter 249).
  - 2 A Japanese artist living in New York. Possibly Toraji Ishikawa.
  - 3 Paul Radin taught at Berkeley from 1918 to 1921.

[252]

Geological Survey

Ottawa, July 2nd, 1918

Dear Kroeber,

Thank you very much for the design developments. I appreciated these very much and the sets are probably the most interesting that I have obtained hitherto, particularly your own with appended comments. The impressionistic drawings of Kretz are, of course, rather aside from what I had in mind, but of intrinsic interest and therefore welcome. I found myself in remarkable agreement with your comments on your own designs, which impressed me as possessed of considerably greater originality and firmness of artistic conception

than most sets that have been submitted to me. As you are interested in heredity, I will point out to you that your designs and your sister's contain at least one feature which, as far as I can remember, is not duplicated in any of the other sets, or at any rate is not at all prominent in them. I noticed that in a few of her designs, as well as in one or two of yours, one of the three elements making up the given design was eliminated, only two of the strokes being utilized. In other words, both you and your sister used the interesting device of developing by simplification. I have not seriously begun to master the material, but feel strongly that some very interesting points will come out. I must get at the brass tacks of the thing very soon now. Even if nothing very satisfactory will result, I shall at least have learned better how to pose some future problems.

Just what am I to do with the "Program for Winter Meeting" which I have just received? Am I to write to you indicating which work or works I should like to discuss at the meeting.<sup>1</sup>

With best wishes and thanks to all those who have contributed to the development of the design.

Yours very sincerely,

[E. Sapir.]

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1 This refers to a short-lived innovation adopted during Kroeber's year as President of the American Anthropological Association. In addition to presenting new papers, open for discussion from the floor in the usual fashion, members were asked to inform the program committee of any recently published papers that they wished to have taken up for discussion. Four published papers were discussed in this manner at the 1918 meeting. See American Anthropologist 21:112 (1919).

[253]

[Lennox, Massachusetts]

7-6-18

Dear Sapir:

Goddard turned down Hagar's last article.<sup>1</sup> Hagar appealed to me. I referred him to the Editorial Committee. He is now addressing you as a member of this to reverse Goddard at the Christmas meeting. Goddard did say something about professionals--more than he meant I think--but I got the straight from him and told it to Hagar: viz, that he would normally print articles of doubtful value from people whose position gave them professional recognition and normally reject them from those who lacked such recognition. Even this is a better working principle than formal admission, but Hagar should not have reverted to the original unqualified statement. I think the matter will require a little tact. In the end the question is likely to be whether we prefer to lose Goddard as editor--he will I am confident quit if flatly overruled--or Hagar as a member. The latter always talks a lot about helping the Association but so far as I know has never paid more than his regular membership dues.

As to Radin, I believe your overestimate rests on a sympathy which you feel because he is perhaps nearer to yourself than anyone in the crowd in fineness and sensitiveness of aesthetic perception. What this sympathy causes you to overlook is a fundamental lack of character. He is kind and affectionate and in many ways lovable; but his fibre lacks virility. He will face nothing courageously--duress, unpleasantness, criticism. His instincts are clean and more than ordinarily unselfish, but he always takes the easiest way and therefore always yields. As soon as a situation becomes crucial I am sure he will break. That is why I have absolutely no faith in his success with us. As long as he is sheltered and steered and nursed he will do very well. As soon as responsibility hits him he will collapse. In this opinion men of European background like Boas, Lowie and Laufer agree with straight Americans such as Goddard and Wissler. Tozzer alone speaks well of him and

he has met only his mind--in conversation, where Radin shines--not the man in action.

I think it is very decent of you to governess Radin, as I shall have to do the ensuing year. But though shorter I shall have the more trying time, because you were 2,000 miles away and wanted only research from him, and if ever you can and will take him over again, I shall be really relieved. I wish my Quixotic proposal of a few years ago would have been adopted: that the half dozen people in charge of institutions agree to take turns tiding Radin over until his immaturity hardened; or failing in such agreement that we all drop him and so shunt him into something else.<sup>2</sup> I don't like this miserable starvation process we are treating him to.

As to heredity, I may have told Elsbeth<sup>3</sup> that development meant anything--reduction as well as expansion: I think your next test should accompany the element with a careful definition of the treatment wanted. Nelson,<sup>4</sup> for instance, got it into his head that he mustn't take liberties with the base. That is very illuminating psychology, but as regards timidity toward orders, not decorative impulse.

I've just had a week with Boas, who is in fine shape and spirits--insisted on walking 24 miles in a stretch and then stayed up till midnight over a detective story--. Am now with Mrs. Parsons, and have a series of analogous weeks ahead.<sup>5</sup> It's rather fun and should work up a heavy appetite for work by September. I'd come to Ottawa, but fares are prohibitive.

Kroeber

A.M.N.H. remains summer address.

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1 Goddard, as Editor of the American Anthropologist, had rejected a paper submitted by Stansbury Hagar, a wealthy amateur archaeologist interested in Mesoamerican astronomy and calendrics.

2 Letter 213.

3 Kroeber's sister. See Letter 252.

4 Nels C. Nelson, an archaeologist with the American Museum of Natural History, and formerly on Kroeber's staff in California.

5 At her summer house in the Berkshires.

[254]

[Ottawa]

July 11, 1918

Dear Kroeber,-

I have told Hagar that I am going to read over his papers and try to arrive at a definite standpoint by the time the Society meets next Christmas. I assure you I have no stomach for this work. I know beforehand, of course, that Hagar is a crank and that his theses probably do not bear a moment's examination. The real question involved is, of course, the general one of editorial policy. I can not tell you how much I regret the point that Goddard made, namely, "that he would normally print articles of doubtful value from people whose position gave them professional recognition, and normally reject them from those who lacked such recognition." This seems to me to be so vicious a principle that mere editorial arbitrariness would even seem better. I mean to say that it would be much better to tell Hagar bluntly that his stuff is N. G. and that it won't be printed, than to hedge about on technicalities of status. I am all the more nettled at this whole matter as it is not the first time that Goddard has, in my humble opinion, put his foot into it. That famous announcement in regard to articles using the term Hohan and Penutian was another such break.<sup>1</sup> It simply won't do to make up principles as to what Tom, Dick, and Harry are or are not to do or be. As it is, I am now between the devil and the deep blue sea. The probabilities are that what I will do is simply to read through enough of Hagar's stuff to get a genuine idea of its value; to be convinced if the stuff is convincing, as it probably is not, in which case I shall fail to be convinced; to then tell Hagar that it does not look very plausible to me but that as I am no astronomer and that as I am not well acquainted with Central American or South American archaeology I really have no right to judge; that furthermore I should be personally inclined to leave the matter of acceptance or rejection of MSS to the responsible editor; but that finally the principle of acceptance or rejection as stated by him seems to me to be an unfortunate one. Please tell me what you think of this, as I should hate to increase the muddle or alienate Goddard.

Needless to say this Hagar business is confidential between us, at least so far. There is of course no particular harm in losing Hagar as a member of the Association, but we certainly do not want to have it noised about that we are a close corporation willing to publish our own rubbish but extra critical when it comes to somebody else's.

I believe you are inclined to overestimate my overestimation of Radin's usefulness. I am quite well aware of the presence of such defects as you point out. The only point is that in view of his undoubted ability, knowledge, and unexampled enthusiasm for field work, it is decidedly worth while overlooking some of these defects. I mean to say that for the good of anthropology in general we should accustom ourselves to condoning little irregularities and overlooking petty annoyances in a way that we would perhaps not do for an inferior man. As it is, it seems rather a joke to me that when such distinctly inferior individuals as Waterman, Barrett, and Mason are getting quite comfortable berths, it should be so difficult for Radin to get permanently landed. Aside from yourself and, of late, Gifford, the University of California has not done ethnological research work of particular magnitude. Radin is decidedly the man that would fill the bill in this respect, and I know that he is more than eager to get into the field for the University. Why not frankly look upon him more as an invaluable field worker for the University publications than anything else, accepting his teaching usefulness incidentally for what it is worth. It is, of course, perfectly absurd for me to be suggesting things to you, as you are in charge of anthropology in the University of California and know best what is required and, presumably, what Radin can do to fill your wants; but really, I cannot for the life of me see that there are any insurmountable difficulties in the employment of Radin as permanent instructor. Whether I shall ever be able to add him to our staff is very doubtful to me. The prospects here are not a bit bright. I shall always try to help him along as much as I can, but that may have to be very little for some time to come.

I have not had a chance to look over Radin's linguistic material yet. He has sent me several addenda and it would seem that in this linguistic night which is spreading with such amazing rapidity, all cows are black. I am

prepared to find Radin as uncritical as he knows how, but will say nothing definite until I have really looked over the stuff.

Yours very sincerely,

[E. Sapir.]

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- 1 Goddard, in a note in the American Anthropologist in 1915, stated that, in his opinion, the data presented up to that time (mainly in Dixon and Kroeber 1913b) were "by no means sufficient proof of the alleged relationships" and that the "proposed family names" (i.e., Hokan and Penutian) were "not yet entitled to a place in anthropological literature". He proposed barring the terms from the journal until more evidence had accumulated.

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Stonover Farm

Lenox, Massachusetts

7/17/18

Dear Sapir:

Goddard's remark was unfortunate. I don't believe he put it as boldly as Hagar represents. At any rate he subsequently authorized me to qualify his stand. After all, the basic question is the worth of Hagar's stuff, not the propriety of any ill chosen remarks Goddard may have let drop when he was on the defensive, trying to turn Hagar down without drawing too much blood. It's not easy being editor when an author taunts you in person about his non-acceptance. I agree with you that the principle is best left alone, if it can be, but I feel we should not let Hagar get a publication for his piffle--if we think it piffle--because he is adroit enough to tie it up with an abstract issue which intrinsically doesn't change the value of his material. Goddard undeniably made a mistake. Hagar offered to pay the cost of his article, and Boas and I advised acceptance. But here Goddard's principles--ethical ones--interfered; I think he felt it to be a species of bribery. An outright upholding of Hagar would probably mean that we get Michelson as editor, or possibly Lowie. As compared with the latter, Goddard has the great advantage of prizing theoretical contributions only in the abstract, so that he plays no

favorite subjects. Lowie might get us into an orgy of kinship or convergence or questions of Germanic origin and coloring. Michelson is eager for the job, I expect, but like Goldenweiser would run the journal into the ground. Who else could take it? I don't suppose you or Swanton would accept, and you're not recommending Radin!<sup>1</sup>

As to him, my grouch is that the sort of teaching he can do I can handle without effort, and that the kind we have led the University to expect from us and therefore must continue to offer, is precisely what Radin cannot do, and it therefore falls back on me, to whom it is a distinct and wearing effort, at a time when I feel entitled to shove the last of it on other shoulders. I hold firmly to the value of what Waterman gave, but I'm neither showman, teacher, nor orator by nature, and after these many years have a certain sense of right to confine myself to scientific instruction and scientific work. This is what Radin cheats me of. Without putting him into an intolerable and disastrous position, I have no recourse left but to watch him eat the cream I had looked forward to. And to boot I'll get his contempt for preferring to do elementary or pseudo-science, because Radin is incapable of imagining any one doing anything but what he prefers.

His scientific capacity, I admit to be great, certainly in many ways superior to mine, but I fear that his lack of discipline will deprive us of any fruition from it. He'll start fresh instead of where we have left off, with the result that whether his work is intrinsically superior or inferior to ours he will equally stultify it by implication. If he could really transcend us, good, but I don't believe it, because his lack of control will negate his genuine gifts; and if he did, nobody would believe that he had. So we lose whether he wins or loses. His recent discovery is a fair sample.<sup>2</sup> If he's wrong, everyone will say Radin has made a fool of himself; if he's right, they'll think him wrong and a fool anyway. You admit he mixes in dross--that's all they are willing to see in Radin, because he hasn't the faculty himself of separating his dross from his metal.

Well, the only fair course is patience, and though I feel tired already, I'll have to exercise that.



As to comparisons with Waterman, Mason and Barrett, they are tame scientifically, but you forget that these men are backed by institutions that want certain institutional goods delivered which they delivered, and that my institution holds me responsible for similar deliveries. If you weren't delivering yours, your arguments would make me charge you with lacking institutional sense as wholly as Radin. Universities want classes, not science, and allot budget and salaries accordingly.

Elsie says your culture paper is with the Yale Review, but the editor doesn't answer.<sup>3</sup> Perhaps summer vacation (professors!). Also I would send you her regards if she did that sort of thing.

A. L. K.

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- 1 Goddard remained Editor through 1920. He was succeeded by John Swanton. In January 1924 Robert Lowie became Editor, and remained in charge of the journal until 1934.
  - 2 Radin had concluded, on the basis of superficial comparisons, that there were continent-wide genetic links among American Indian languages (Radin 1919a).
  - 3 Sapir had submitted "Culture, Genuine and Spurious" to the Yale Review, through Elsie Clews Parsons. It was apparently rejected. A portion of the essay was published in The Dial under the title "Civilization and Culture" (Sapir 1919a), but the full paper appeared (in the American Journal of Sociology) only after several years' delay (Sapir 1924b).

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[Ottawa]

July 19th, 1918

Dear Kroeber,-

I see that we agree absolutely on the Hagar business. I think that we should, if possible, have it clearly understood by all concerned that the publication committee expects to put no such principle in operation as Hagar referred to, but that every article is to be judged strictly on its own merits. I feel that it is also dangerous to make a distinction between those who pay for their articles and those who do not. This distinction I feel should not,

in the slightest way, interfere with acceptance or non-acceptance of the articles. After an article is duly accepted it is time to suggest to the contributor, if it be deemed advisable, that financial help would be acceptable. Hagar offering to pay for an article that has not yet been accepted should not be listened to. You see I believe that the less of these principles we have, the easier and juster will be our policy. To be frank, I rather agree with Goddard in regard to the ethical point. It is a species of bribery to accept a poor article, which would normally be rejected, merely because the contributor is willing to pay for its publication.

I should hate to see Goddard throw up the editorial job in a huff. Goldenweiser and Radin are quite out of the question for the job. For one thing, Radin has not yet learned to return a manuscript until two or three years after he has received it, if then. Goldenweiser is impossible in correspondence, and his business ability is, of course, zero. Michelson, I am afraid, would be too petty about it all. I think myself that Lowie would make an excellent editor, but it would be unfortunate to have him step in as a result of Goddard's angry retirement.

You may be right about Radin's teaching status this coming year. I can readily sympathize with you, you see. I think the only thing to do is just to make him do the more elementary teaching, exercising as much supervision as you think necessary but without holding him down too precisely to traditional methods. It is certainly in the highest degree absurd for you to have to put up with elementary teaching and similar drudgery merely in order to give Radin the chance to spread himself out. I should be inclined to doubt if he expects any such indulgence on your part.

I have been looking over Radin's tables very carefully.<sup>1</sup> As I expected, there is a fearful amount of evident rot mixed up higgledy-piggledy with some really good stuff. For comparisons of Yuki with Penutian, with Athabaskan, with Wiyot-Yurok, and with Algonkin are one and all weak and thoroughly unconvincing. I do think, however, that he has made a real hit with his Siouan comparisons, and I think also, as I have myself suspected more than once, that both Yuki and Siouan will turn out to be genetically related to Hoka. The fascinating possibility presents itself that Yuki is a major member of this

enlarged group; in other words, that it holds its own as against Hoka, both being eventually related to Siouan. Or, it may be that there is a more specific relationship between Yuki and Siouan. I am rather pessimistic about Radin's ability to work out definite phonetic relationships. He is singularly lacking in synthetic ability. To substitute Athabaskan for Na-dene, and then further to substitute Hupa for Athabaskan, failing to take account of very specific dialectic developments of forms that are in themselves secondary developments of other forms, is [Radin's] idea of historical linguistic method. Thus, I could see at a glance that some of his Hupa comparisons were utterly worthless. If I were you I would look more carefully at his Siouan material, give him what encouragement he really deserves and have him go ahead at that part of his problem thoroughly and systematically. Preliminary work on comparative Siouan phonology would be quite necessary. Please give my best regards to Mrs. Parsons. With best wishes,

Yours sincerely,

[E. Sapir.]

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1 In the MS of "The Genetic Relationship of the North American Indian Languages" (Radin 1919a).

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University of California  
Berkeley, November 15, 1918

Dear Sapir:

If I were of the disposition I would be jealous at not getting poetry when it comes to Radin so frequently. I would even jump on it if you so demanded.

Radin is making out rather well. I cannot say that he is more than a fairly satisfactory teacher. He gets on well with girls especially in smaller classes. I doubt whether he will ever do elementary work very successfully because he cannot bring himself to be interested in elementals. It is very

much like a Whistler teaching a novice perspective. Between the three or four others of us Radin is pretty well sheltered from business and institutional relations and this has probably contributed to make his course smooth. I find him very willing however to do anything that he is asked to, extremely patient under criticism and altogether cheerful and docile. He has abundance of time for scientific work. I am trying to hold him down as far as possible to one or two things at a time until they are wound up. If he were to stay with us a few years I am confident of giving him a string of publications that would be no better in quality than those which he has published heretofore but would be more bulky and concretely descriptive and therefore more impressive to most of his colleagues. I am beginning to think that all that Radin needs for his professional rehabilitation would be to actually deliver any kind of goods once or twice. His career has certainly been as sterile from one point of view as it has been fruitful in personal experience.

His wife seems to continue in very good health.

You will have gathered from the foregoing that we maintain an unimpaired budget for publications. With the war ending, the prospects for the next two years in this direction are bright. I wish I could say the same for our prospects for getting Yana ethnological notes or a Yana grammar in the same period.

And in a personal way what is new with you?

Sincerely yours,

A. L. Kroeber

[258]

Ottawa, Ont.

November 21, 1918

Dear Kroeber,

I was glad to get your letter of Nov. 15th. You are one of those so horrifyingly efficient people, disdaining, as a rule, to knock off time from their important researches, for mere correspondence' sake, hence your letter is

doubly welcome. So much so that I am going to take a holiday from Hagar's zodiacal circus (I've just begun to read the blooming stuff, so as to be in a position to give him at least some kind of an answer) and answer you instead.

I'm glad you are beginning to realize Radin is a member of the genus homo, not altogether an unclassified freak. Yes, he is quite willing to do what he's asked to do and quite willing to listen to criticism, provided always he's not handled in approved Prussian style (and Boas, say what you will, can be some Prussian!). As for "rehabilitation"--well, it seems to me he's been up against particularly bad luck. His Ojibwa ethnology, for instance, has been letter perfect for press, long ago, but we can't print it.<sup>1</sup> Not yet, anyway. Radin may have sinned in starting too many things and leaving them unfinished, but I have sinned so much more that I am inclined to be charitable. As to his teaching ability, your remarks agree with what I had expected. I would not be prepared to find him an extraordinarily successful teacher, particularly at elementary work. Good elementary teachers need to be immediately purposive in their outlook and reactions; roughly speaking, they must be unaesthetic, not particularly able to feel the significance of the one fleeting and irrevocable moment per se. On the other hand, I think that with experience Radin could learn to fill the pedagogical bill satisfactorily enough. After all, he's no fool. If you will only give yourself a chance, I think you will find that the wounds of parting from T.T.W.<sup>2</sup> will actually heal!

In his last letter, received after a long silence, Radin again says nothing about my poetry MS, in his hands for criticism. I presume he doesn't greatly care for the stuff and keeps postponing its consideration. As you, perhaps to your eventual regret, manifest some interest in this matter, will you kindly ask Radin to hand over the 100 odd poems he now has to you? I have given up hope of ever getting him to respond and send back the MS with comments, so would really appreciate it if you would take charge of it and give me the benefit of your criticisms. I have recently gathered a selection from the stuff into a tentative MS volume, "In Sun and Shadow", and am rather curious to see if your opinions would corroborate my choice. A good many of the poems Radin has I have discarded for good or practically so, a few others I have revised since they were sent to him. I am telling you this to indicate that

I am quite aware of the fact that much of it is poor work. Still, I feel that I have perceptibly improved since the "Dreams and Gibes" days.<sup>3</sup> Free verse is only an experiment with me now, not a dogma (though it never was really a dogma with me). Please criticize away all you want, only remember one or two things. First, I do not want general criticism of form, except where a given form seems intrinsically unsuitable to the subject. You must accept all forms as possible. That you personally do or do not like rhymed free verse or unrhymed metrical stanzaic verse or sonnets is of no interest to me as regards these poems. (In your opinions on form as such I am, of course, interested.) Secondly, I do not in the least care to be told that this idea is morbid or that one unpalatable. You've got to accept, in a general way, what I wish to express at the moment. Your only problem is to see whether I've put it across. Pardon these insulting remarks. Of course, they are quite needless in your case. To be frank, I really do crave genuine criticism; the more detailed, the better. I would have kept in touch with you on this matter if it were not for a fear I've developed of presuming on people's patience. When it comes to a show down, I find that very few are really willing to go to much trouble to help me winnow out the stuff. Later on, if you're good, I'll send you the MS of the children's volume. That, at least, will give you a few laughs, so let it serve as a candy bribe. The "Portraits" I am sending under another cover may interest you a little. Three or four of the persons I had in mind are known to you. You and Paul may get a bit of fun in solving the conundrums! When you're through with the MS volume, do return it to me, will you?

I have still some Paiute work to do for Gordon (ethnology, dictionary, music). Yana ethnology was begun and is fairly on its way. I'll return to it before too long and let you have it in a year or two! As for the language, it's a big job. You missed your chance when I was your "research assistant". I would have been only too glad to stay on at Berkeley instead of going to Phil. I was full of Yana then and profoundly interested and had made a mighty respectable bite into the collectanea stage. Now--! To tell you the unvarnished truth, I don't exactly see why a man with pressing economic problems and plenty of other obligations should be held down to delivery gratis of the fruits of a solid two years' job resulting from a \$600 per year incumbancy.

Maybe I'm crazy, but I don't see it. Life is sweet, science--is pretty damned dull sometimes. Somehow, I have a feeling that I'll never write the Yana grammar and dictionary in extenso unless I get a long furlough at the Univ. of Cal. That is not likely ever to be.--Say, would you care to publish a paper on Yurok and Wiyot kinship terms as related to Algonkin terms? I've got the cards ready, but haven't actually written the MS. There seem to be about 20 correspondences or so in actual terms. Say "yes", for I have other plans for the "Anthropologist" and linguistic journal.<sup>4</sup> Have you seen T.T.W.'s Yurok-Salish evidence? I wonder if he's right. You may remember I once supposed something of the kind for Tsimshian, but gave it up for Yurok-Algonkin.<sup>5</sup> That Wakashan-Salish group is a stubborn and specialized group, like Nadene, but it will, of course, link up somewhere. I find it difficult to think of Kwak.-Nootka as joining hands with Yurok and Wiyot--nearly as implausible to me as Athabaskan-Siouan, which I don't, so far, in the least believe in.<sup>6</sup> How is your Zuñi working out? I have a vague feeling, from incidental forms in your "Zuñi Kin and Clan", that it belongs to Penutian, but, of course, I have no right to speak.

Of personal news, only that Barrett and his wife stopped over to see us. They are on an Iroquois collecting trip. Pleasant chap, Barrett, and evidently efficient. Too bad he's so confoundedly colorless. His wife--not bad-looking, but no beauty (pace Radin). A typical product of our deplorable Middle States "culture", yet nicely kittenish withal.--Oh yes, Morice<sup>7</sup> was around some time before. I had quite a talk with him. I understand he's working hard at his Carrier grammar and dictionary. It will probably be the Athabaskan monograph, which Goddard's Hupa conspicuously fails to be.<sup>8</sup> Morice is naive, of course, and no good for comparative work in Dene. But he does evidently know his Carrier facts. He is probably thoroughly adequate in morphology (which is no joke, for Dene is probably the son-of-a-bitchiest language in America to actually know) and reasonably adequate in his phonetics. His phonetic descriptions will not be good and, on minor points, he probably assimilates unconsciously to French, but his grasp of the phonetic dynamics, the phonetic organic system as such, may be better than Goddard's.--Barbeau got a mountain

of French folk-lore. He talks placidly of thousands of song records. Intends to transcribe them all, too. Don't want his job!<sup>9</sup>

Under another cover I am sending you a duplicate set of development drawings (photographs, rather, of redrawn drawings) of my design ~~X~~, with carbon of subjects' comments. I haven't done the actual work yet. That too is waiting for the moment. I thought you'd be interested in the sets submitted. Make what comments you like and return. I think 2 or 3 of your own drawings are incomparably the best in the set as genuine developments. Haeberlin's set is good, too, I think. Coluzzi Kretz is interesting, but his drawings are rather beside the point. "Z99" is evidently a real artist. Clarence Day, Jr. also has genuine talent. But it is your series I am most pleased with, particularly j and k. These two designs do precisely the thing I was after. Without slavishly copying the original, or treating it as a mere geometric abstraction deprived of subtle particularities, they do in a mysterious way keep the spirit of the original and idealize that spirit. I have a notion that the solution of the problem, i.e. of "spirit" preservation, lies along kinaesthetic lines. Your j and k do the same talent thing, whatever it is, that the original design is, less gracefully, striving to do. Very few of the others do it. I hope to analyze out quite a number of points of interest from the various series of designs--psychological chiefly, though I think there will be some interesting hints for speculative ethnologists too (convergence, realistic tendencies, assimilation to cultural type).

What is this I hear about Mrs. Parsons' Southwestern Society? Mason has just dropped me a note about a Papago trip under its auspices.<sup>10</sup> (À propos of Mason, did I tell you that his Salinan grammar<sup>11</sup> makes the Hokan character of the Salinan language absolutely certain? In my opinion, at least. Frankly, though, I was rather disappointed by it as a piece of analysis. He is obviously wrong on some points; other interesting points slipped away from him.)<sup>12</sup>

Well, I've babbled enough.

Yours,

E.S.

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1 This MS was never published, and presumably remains in the Archives of the National Museum of Man, Ottawa.



- 2 T. T. Waterman.
- 3 Dreams and Gibes (Sapir 1917f) was an early (and the only published) collection of Sapir's poetry.
- 4 This paper was eventually published in the Journal de la Société des Américanistes de Paris
- 5 In fact, it was Waterman who had noted parallels between Yurok and Tsimshian. Sapir had considered a Yurok-Salish connection. See Letters 107-110.
- 6 A speculation of Paul Radin's, incorporated in his general paper on North American genetic relationships (Radin 1919a). Genetic connections among Athabaskan, Tlingit, Yuchi, and Siouan were much later (and independently) suggested by Haas (1964).
- 7 Father Adrien G. Morice, O.M.I. (1859-1938), a missionary priest in British Columbia who had written extensively on Northern Athabaskan ("Dene") languages.
- 8 Morice's The Carrier Language, an imposing work in two volumes, did not appear until 1932. It was dedicated to "that bright young philologist, Dr. Edward Sapir".--For Goddard's Hupa work see Goddard 1905b and 1911.
- 9 Sapir took some interest in this work, nevertheless. See, for example, his translation of some French Canadian folk-songs in Poetry (Sapir 1919a).
- 10 The "Southwestern Society of New York" was one of Elsie Clews Parsons' many devices for subsidizing anthropological work. Mason worked on Papago linguistics during the winter of 1919 with funding from this source.
- 11 Mason 1918.
- 12 See Sapir's published review (1920d), and further comments in Sapir 1921b.

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Ottawa, Ont.

Nov. 27, 1918

Dear Kroeber,

Here is my reply to Hagar. You may keep it, as I have another copy. You may think I have met Hagar too much and turned my back a little on Goddard. At any rate, I have been very frank about Hagar's case and careful to say

precisely what I think about it. Presumably Hagar will get sore and resign in a huff. Well, let him.<sup>1</sup>

Yours,

E. S.

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1 Stansbury Hagar remained a member of the A.A.A. until his death in the mid-1930's.

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University of California

Berkeley, December 6, 1918

Dear Sapir:

I think your letter to Hagar covers the situation admirably. I am returning the copy to you. I rather think the whole matter may go over a year. Hagar has just written me that he does not expect to attend the meeting and I have urged him not to have the case come up in his absence. It seems moreover that he was more or less counting on my support although I am sure I have not given him any grounds for such belief other than the desire to be courteous and fair. It seems he had also counted on my being present at Baltimore.

You will hear fully from me both as to the designs and verse quite shortly.

As to the Yana grammar I think you have got into an unnecessarily peevish mood. It does not seem to me in the least a question of what our outlay was ten years ago nor whether you were then adequately compensated. These considerations affect only the legal title to the material and of that there is not the least question, or desire on my part to raise it. If the language or material on it seem to you to be sufficiently interesting to constitute a number we shall be delighted to have you go ahead and to bring out your results. If you have other things in hand which promise more or would bring you income, it would be foolish for you to proceed with the Yana. In that case I would only ask you that when you are sure you have reached a decision you are

willing to abide by to tell me so in order that the matter may be wound up in the best possible shape. It would be almost criminal to tie your data up permanently. In that event I would ask you to prepare for us at least a list of the principal stems and suffixes as an aid to the next comer in using your texts, or if you do not wish to trouble even with this, to let me print a list of affixes of which you sent me a copy some years ago. However I do not anticipate anything of the sort. I realize keenly your reaction to the amount of time that good linguistic work takes but have a feeling that at bottom you will be more anxious than anyone else to do the grammar yourself.

I also do not in any way wish to prejudice a possibility of arranging for you to come out here for some summer. Nothing in fact would please me better but you should realize that I can scarcely see any near prospect. If we were starting fresh without any commitments to the past or obligations which established work always carries, we could easily with our present budget arrange such a thing. In fact we can provide for a number of other undertakings which I should regard as infinitely more valuable than some of the things we are doing. But human affairs do not go in this way. The reason we have such income as we possess is precisely because we have not devoted ourselves purely to the prosecution of the study of the Yana language and the personality of Edward Sapir and the like, but have given extensive lectures, opened a museum, and so on. Considering that it does these things that are most largely appreciated and that most of our money is given us for the purpose of maintaining them, our income is really distinctly inadequate and it is only now and then by some special combination or by giving a twist to some passing opportunity that we can make the sort of arrangement that you have in mind. I feel the sting in your remarks and admit its force but construe your attitude as a sort of railing against the fact that our institution is not ideally conceived or operated. Being yourself a successful head of an institution, I am convinced that at bottom you will agree with me that you have vented your spleen rather than brought any serious charges.

Ancient history is ancient, but you make one statement which may influence your future attitude and which I wish therefore to correct. You are very much mistaken if you thought that I did not wish to keep you when you were here.

I can say the same thing for Goddard. In fact he went so far as to propose a virtual splitting of the department with himself in undisturbed charge of one of the wings and you as his assistant. I know that he definitely made and urged this proposal. You were however with us during the last of the three years for which Mrs. Hearst had granted her support and the end of your year was precisely the period when the University took us over on its own shoulders and we were squeezed through the small end of the horn. Everything that could be thrown overboard was thrown overboard in the spring of 1908 and to argue from the mere event of the dispositions entertained is very misleading.<sup>1</sup>

Sincerely yours,

A. L. Kroeber

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<sup>1</sup> Kroeber describes Goddard's views in 1908 accurately enough, but he can be accused of overstating (or at least misremembering) the extent of his enthusiasm for Sapir's continuance at Berkeley. See Letters 31 and 42.

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[Berkeley]

12-7-18

Dear Sapir:

I'm tremendously interested in the designs,<sup>1</sup> and have gone out of my way to add a series of comments. I'm very keen on 27.

I tried them on a crowd the other night and was surprised how casually they took them. Perhaps because their interests were literary. I think our culture represses feeling for visual decorative form. Paul was interested, but chiefly in the revelations of personality contained. One spontaneous comment may amuse you: 1 j was labelled "insincere"

I think you ought to reveal no names, or else suppress or edit some of the comments. Mine were meant for you not for publication. Spier<sup>2</sup> may feel the same way.

By the way, I see no great family likeness, and what there are I lay to family habitus rather than heredity. I've seen a good deal of the Spiers in

the past year, and recognize in their drawings the familiar pall that stifles all their inborn qualities.

As to your verse, it isn't form or lack of form I quarrel with, but the want of passion. There is everything but intensity of feeling. I don't know whether you are conscious of this, or find some substitute for yourself; but it's the quality that sticks out to me--the more patently because feeling as content, mood, and formal mastery are sufficiently there. Your impressions are definite, the moods keen, the expression of them accurate, but all that doesn't make poetry. Take your Vexation in Dreams and Gibes: how can that sort of feeling be made into poetry? You've carried the emotional status across to the reader; but what of it? The feeling as such he is familiar with; the expression is more adequate than he normally could give; but what does he get that is new to him, or stirring, or relieving, or leading to anything different or anything pleasurable? I may see you making a good job of an evacuation and realize the comfort you get from it, or see your piles hurt and sympathetically feel something of your pain, but I don't recognize any way of gathering for myself any catharsis or faith or anything else that I want from being keenly aware of your physiological mood. Well, you do it psychically, but the principle holds. You get everything over except the thing that will make me not only directly interested but wanting more and a repetition; and that isn't in the nature of what you are doing. You have keenness of mind, both emotion and thought, and show it in every sentence; but you have gone on the assumption that you could do without intensity. Well, one can; but not in poetry. Review yourself and see if you haven't taken one quality for the other.

I like Dreams and Gibes better than the later things, on the whole; but the best bit, the one good bit, is the Buttercups Mary. You've got intensity there. Write some more poems to your wife and you may hold on to it and develop it. Also, it's straight, swinging metrical form and spontaneous rhyme; which makes me think that when you write with passion it won't be free verse. The Prayer for [illegible] is a gallant attempt, but only that; the feeling is genuine but dim, the intensity of expression artificial. DeBussy is promising, but only as a snatch of something. The Woman in the [illegible] just misses

being the real thing. You felt her eyes but they did not bore themselves into your aching heart. But Mary in the buttercups touched you more than you could put into the lines. There's the difference.

I'm sorry, old man, to be so rough. You asked for it. Don't burn anything up but put nine tenths of it away for a year, go over the remainder for the touches of quality of which I speak--if you admit any justice in what I say,--don't write again till you feel the drive of that, then don't miss the opportunity, and you'll do something that will please others as well as yourself.

Thanks for having let me see it.

Kroeber

I'm expressing everything to you in lieu of a frank--in one bundle.

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- 1 Sets of design developments from various subjects (see Letter 249), including friends and colleagues, which Sapir had apparently sent Kroeber for his reactions.
  - 2 Leslie Spier (1893-1961), at this time a graduate student at Columbia and Assistant at the American Museum of Natural History.--After receiving his doctorate in 1920, Spier (and his then wife, Erna Gunther) founded the anthropology department at the University of Washington. In the 1930's Spier was at Yale as Sapir's junior colleague, and in 1939 joined the faculty of the University of New Mexico, where he remained until his retirement. He was an exceptionally dedicated field ethnographer, with an unsurpassed breadth of acquaintance with North American Indian groups.

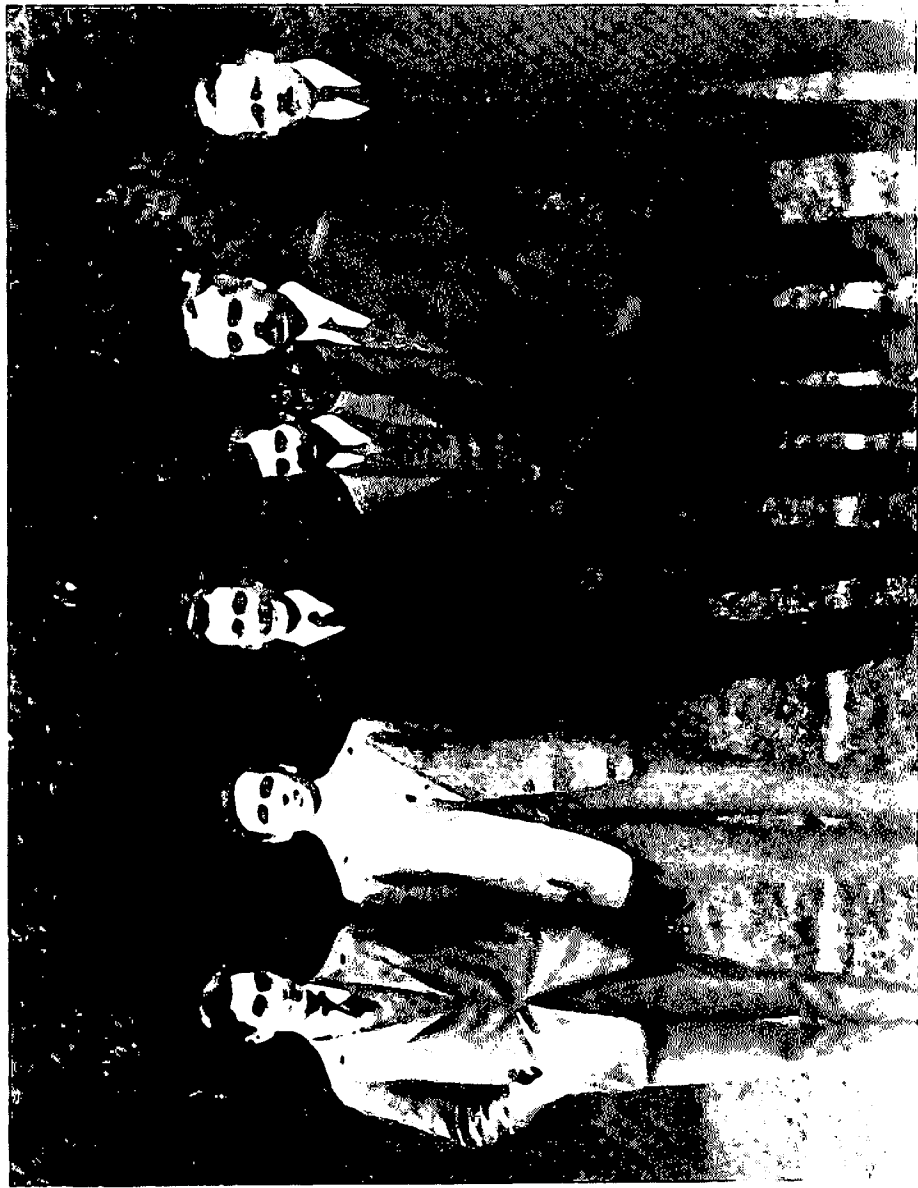
[262]

Ottawa, Ont.

Dec. 13, 1918

Dear Kroeber,

I am glad you liked my answer to Hagar. I was a little afraid he might tear out a passage or two from their context and read them at Baltimore, so I have sent Goddard a copy of the letter too. Perhaps I am unwise to show my hand to Goddard, but I should not like to have him get a perverted idea of my attitude.



Left to right: Leonard Outhwaite, Leo J. Frachtenberg, J. P. Harrington, A. L. Kroeber, T. T. Waterman, and J. Alden Mason. Taken in Berkeley or San Francisco, December, 1916, during a meeting of the Archaeological Institute of America. (Courtesy of the Lowie Museum of Anthropology.)

Your remarks on the Yana business<sup>1</sup> seemed quite just and entertaining. There was only one remark that jarred a little, i.e.: "The reason we have such income as we possess is precisely because we have not devoted ourselves purely to the prosecution of the study of the Yana language and the personality of Edward Sapir and the like..." No doubt you realize by this time that this statement was too gratuitous and silly for one of your subtlety of mind to make. Did you ever stop to consider that the safest and most conservative way to run an institution is to indulge occasionally in reckless generosity?

Now as to the brass tacks. I must, first of all, frankly confess that the absorbing interest I once felt in Americanistic work is somewhat in abeyance now. The result is that I am much less inclined to devote all my time and energies to it than, say, in 1908. My peevish attitude, as you choose to construe it, is in reality a sort of dissatisfaction with myself compounded of two factors: my inability to get up enough steam to catch up with my data (Yana grammar, by the way, is only one of many big jobs) + lack of physical energy. Only I myself know what this latter really means for my work. To this inner difficulty must be added a powerful external one. Money! One wife and three children, with frequent doctor's bills to pay, seem to mean, in my case, that I must look about me now and then for outside methods of supplementing my income. That is at least one reason why I did Paiute grammar for Boas and Paiute and Ute Texts for Gordon and why I am to do more Paiute work for Gordon. If you could arrange compensation for Yana, I should be much more inclined to resume the work. I would suggest:

1. Yana Grammar (N. and C. dialects), complete study (probably longer than Goddard's Hupa)	... \$750.00
2. Yana Dictionary	... 500.00
3. Ishi's material	... 250.00
	\$1500.00

To be completed within 5 years at most from beginning of actual work. The Yana ethnology would be thrown in without compensation. Remember, my dear boy, I am not talking scientific ideals just now, in which I believe, with moderation, but merely business. If you can do nothing financial for me, then all I can



say is that I am honestly planning to do the Yana work freely as well as if remunerated but that I cannot promise to have it done by any certain date, as it must take its turn after work which is remunerative. Should this displease you, you have, of course, the right to take over my Yana data and do as you see best about them. I would give you not only my MS texts with notes but also all collectanea already made. I should even be willing to read MS prepared on this material and correct all misunderstandings and supplement shortcomings, if that is within my power. I really do not see what more I can do.

I am glad, by the way, to realize that you did make a vigorous attempt to keep me at California. Not, perhaps, that it matters so very much, but it is nice, after all, to contemplate anything in the way of friendly relations.

I hope I have broken no bones. I am too fond of both your and my carcasses for that. Au relire.

Yours sincerely,

E. Sapir.

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1 In Letter 260.

[263]

University of California

Berkeley, December 17, 1918

Dear Sapir:

We want very much to use your History and Varieties of Human Speech from the Popular Science Monthly of 1911 as an assigned reading in our introductory course. The first consideration in such a case is always a sufficient number of copies to allow from one hundred to two hundred students to read the assigned selection almost simultaneously, that is within a week. Can you dig up ten or a dozen reprints and let us have them? I should very genuinely appreciate the favor. The copies would be thumbed too much for us to be able to return them to you but I have this suggestion to make: unless the reading proves too difficult for our students and I hardly think it will we will put it

on our permanent list and include it in a new and enlarged Selected Readings or source book which we are planning to compile during the spring.<sup>1</sup> In that event we can return to you an equal number of copies in the new form.

Will you let me know whether you can help us out in this matter? In the interim I am assuming that you can and listing your paper as a required reading in our syllabus.

Sincerely yours,

A. L. Kroeber

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1 Sapir's paper was reprinted in a preliminary volume of Selected Readings in Anthropology (1919), but was dropped from Kroeber and Waterman's subsequent Source Book in Anthropology (1920).

[264]

Geological Survey

Ottawa, December 26th, 1918

Dear Kroeber,-

I regret exceedingly that I am entirely out of copies of my paper on "The History and Varieties of Human Speech". This paper, as you may know, was reprinted in the Annual Report of the Smithsonian Institution for 1912. Under another cover I am sending you the only two copies I can scrape together, a separate returned to me long ago by Smith and the Smithsonian Report including the paper. You may keep both of these copies as long as you like. I am also writing a number of people that I had sent separates of this paper to, with a request that, if they have no particular use for it, they send it to you. I am venturing to promise them a copy of the source book that you mention as being planned to include a reprint of my article.

With best wishes for the New Year.

Yours sincerely,

E. Sapir.

I find that you, Paul, and Gifford were sent separates.

[265]

[Berkeley]

1-11-19

To hell with critical faculty. I wish to God I could trade some of it for something else.

Surely I've read the persons into their designs: one couldn't do otherwise. I was almost wishing you had first [drawn?] the comments and then got a second set of reactions from the name. Or you might try still another game on some one: give him the names, at least those he knows, and let him guess who did each card.

If you have ideas as to my reactions on your wife's handwriting, why don't you have her write me a letter?

As to verse, I'm feeling too friendly to rub it in any more. I'll get the outstanding mss. from Paul and wait for you to send me a few new mss., then go at you again. Anyhow, you got even suggesting I was trying to hang sentimental conventionality onto your "Parting", which I don't remember by its title, and still less amending it. That I incline to sentimentality I've been told so often that it must be true, but you're first in the conventionality dig. But please don't even hint that I might like Chopin's Nocturnes. I don't. They positively set my teeth on edge. Also Traumerer and Ave Maria and the Rosary.

As to being atypical, it means, if true, that you must write and compose for your own fun only instead of that of others. But I doubt it. I see no evidence of anything abnormal or tortured or warped in your work. It rather comes out clean and neat. All I don't see is the drive behind that makes the product compact and hard and arresting. That's what I mean by passion. You evidently have great sensitiveness toward images. They run away with you over two and three pages. But the intensity of emotion that cuts them out and burns them in isn't there, or hasn't shown itself. Perhaps you have some of it and don't let it out. Thirty years of environment and habit can stifle a lot.

As to Yana, you may be entitled to all you indicate--I won't go into the question, because it's plain that without a large endowment falling on me from

heaven, we'll never have the money. But let me outline a possibility. When next I finance myself away from here for six months, can you get your people to pay your salary and get mine in addition here? That might be something feasible, and the little teaching you would have to do might be fun as a novelty after these many years. If I can get a temporary job elsewhere, my salary will be available: if it's a half or whole sabbatical at least part of it would be loose. Anything doing?

Kroeber

[266]

Ottawa, Ont.

Jan. 18, 1919

Dear Kroeber,

Your dark hints as to a term of teaching in Cal. are interesting. It is impossible at present for me to say whether I should again be able to get extended leave of absence at salary or even half salary. We might possibly work the game in due time (at least I think Barkis would be willin'). Meanwhile, tell me a bit more definitely, if possible, as to when, how, and what (I mean what and how much teaching). Please don't overlook the incidental fact that what little anthropology I ever knew has long been forgotten! I should certainly welcome an opportunity to clean up the Yana account as far as possible. It could hardly be done in a 6 months' term with teaching on the side, but a very respectable bite could be made and I should feel obligated, under such circumstances, to finish up the Yana grammar even if that required work beyond my leave of absence term.

As soon as I finish the Yurok-Wiyot-Algonkin kinship paper, just begun, I shall submit it to you, unless you warn me in time that it could not be considered for publication by your Univ. When I get that off my hands, I am planning to finish the Yana ethnological paper begun so long ago. I am rather ashamed so meager a paper should drag out such an absurdly long time.

Thanks for further remarks on poetry. As I said before, some day I shall learn that I haven't anything worth while expressing. As yet that realization hasn't come. So I go on turning out half-baked stuff that sometimes pleases me and, apparently, few others.--Do take all the outstanding poems away from Paul. I don't want him to handle any further MS of mine (at least non-scientific MS); his promises are not worth the air they're written on. And, if you don't mind, return stuff as I send it to you rather soon. Spontaneous notes dashed off on individual poems would interest me more than general remarks on a mass of work.

Best wishes for the New Year!

Yours sincerely,

E. Sapir.

[267]

University of California

Berkeley, February 11, 1919

Dear Sapir:

I may ask for a half year's absence from January to July, 1920. I have half a sabbatical year occurring then. I have not yet made up my mind. The University may decide to engage a substitute for this period or to worry along without. The last time they engaged Wallis. They might well not wish to do so for six months. You will realize that the entire situation is tentative. At the same time it may soon come to a head since the matter affects our budget for the fiscal year 1919-1920 and budget recommendations have just been called for. Will you advise me whether you would consider coming here for these six months if we have the opening and if so on what salary terms. Teaching would be light. The greatest amount of work is in connection with the large Introductory course. This has become a sort of organized machine into which a new man fits for his particular part quite easily and without carrying any responsibility. The work there will hardly come to more than one or two semipopular lectures a week. Outside of that I think one course for upper division

undergraduates and one graduate course would fill the bill. Of late years practically all of our graduate courses have been given by the staff jointly. This system has certain distinct advantages and would still farther reduce the exaction of time. You will understand of course that I am not making you a proposition but asking for your attitude in case certain contingencies eventuate.

Sincerely yours,  
A. L. Kroeber

[268]

University of California  
Berkeley, 2-25-19

Dear Sapir:

I'm pleased you're so attracted, and sorry the prospects aren't better. My salary is--2750! What it will be next year, there's no telling. During a sabbatical I get 2/3. They might give 1/3 or 2/3 or 3/3 of any regular pay to a substitute. There's no rule: the individual case determines. Finally, I've so far struck a snag: they profess to doubt whether I can be spared. I'll find out as soon as I can what it really means.

Yours  
Kroeber

[269]

University of California  
Berkeley, 3-5-19

Dear Sapir:

I was told yesterday that I should have to choose between a possible promotion and a leave next year. I took the former. I am sorry that this ends

our plan for 1920. Perhaps 1921 will bring better. I am very sore at the indignity of the alternative.

I think Radin's chances of staying on are about 3 out of 5. It is an interwoven situation, with the ends not wholly in my control. I very much hope that if we fail you can take him in again. A few more years of decent job and he will be professionally vindicated and rehabilitated. His inner worth can't be impaired, but a stable career will bring more out of him.

Yours,

Kroeber

[270]

Geological Survey

Ottawa, March 15th, 1919

My dear Kroeber,-

Thanks very much for the copy of the map of North American tribes recently gotten out.<sup>1</sup> This seems to be a great improvement on anything of the kind that has preceded. I should be much obliged to you for half a dozen extra copies, if you can spare them, as there are several in my department who would like to have copies.

With best wishes,

Yours sincerely,

E. Sapir.

---

1 The first tribal (as opposed to linguistic) map of North America appeared in Wissler (1917), but it showed no boundaries. The University of California issued a small map in 1919, based on Wissler's, with tribal locations shown more accurately (by numbers), but still without boundaries drawn. A truly comprehensive tribal map of North American was not prepared until the late 1930's (Kroeber 1939:8-12).

[271]

Ottawa, Ont.

Apr. 3, 1919

Dear Kroeber,

In looking over your Arapaho material<sup>1</sup> (in connection with my relationship paper, which will astonish you when you see it)<sup>2</sup> I am led to wonder if you did not perhaps overlook a distinction between animate and inanimate plurals after all (see p. 117). You speak of "-a<sup>n</sup>, -ha<sup>n</sup>" (probably really -a<sup>n</sup>', -ha<sup>n</sup>'). This suggests that you heard sometimes -a, sometimes -a'. May it not be that -a is inanimate plural, -a' animate plural? -a < Algonkin \*-an, \*-al; -a' < \*-aki (k regularly either disappears or becomes ' in Arapaho; never s, which goes back to sk, ckw). Have you evidence pointing that way? It's rather a fundamental point.

It is amazing how readily Ar. yields when one has worked out the phonology. Ar. is not a big divergent at bottom; it is strangely like Cree (as against, e.g., Ojibwa).

Yours,

E. S.

---

1 Kroeber 1916a.

2 "The Algonkin Affinity of Yurok and Wiyot Kinship Terms" (Sapir 1923b).

[272]

[Berkeley]

4-7-19

Dear Sapir:

Don't be impatient. A poem comes with a lot of business mail while I am at my desk in office hours. I couldn't give it my mind then if I tried. I



shove it in a pigeon hole with the others. Some day I'll go through the lot and shoot them back to you--perhaps with more comments than you'll like.

Yours,

Kroeber

[273]

[Berkeley]

5/26-19

Dear E. S.:

I return all your poems but one, I think. The comments may be of no use to you, but you asked for them and I did what I could. At least they're not evasive nor theoretical.

I'll try Paul once more, but can't really compel him, any more than I could collect money he might owe you. It's hard enough to get department work out of him once it's a day in arrears. He has equal manias for seizing and not disgorging anything on paper--letters, books, ms., or notes. The vehemence and tenacity of these instincts in him are really very interesting when they don't happen to annoy.

He and I get on very well so far. His job is permanent and his title assistant professor. He gets less pay than he should, but it's more than he's ever had, probably, and he seems content. I believe I have got him committed to activity in publication. But he is fickle as hell, and his fidelity to anything is ultimate rather than continuous. He admits he is built for light touches--intermittent, inconsecutive, and vague, that means; but his contacts are a sort of art.

Kroeber

[274]

University of California  
Berkeley, 6/4/19

Dear Sapir:

Here's your chance to get even for all my brutalities. I'll be glad to get any reaction from temperamental background to pure style. When through, please send it to my sister, Mrs. H. O. Rosenthal, Gaylordsville, Conn.

Sincerely,  
Kroeber

[Note by Sapir: à propos: "Obiad and Urdu"]

[275]

[Berkeley]  
6/17/19

Dear Sapir:

My God, I hope you will not send such a notice to the Anthropologist, or at least not without letting me see it.<sup>1</sup> It would only fan the fires of Goddard's sadistic joy. He encourages his best friends to wrangle undignifiedly in print and in this matter would have double satisfaction over a dissension in our camp. You will even have difficulty retracting any manuscript: he sends such bits to the printer at once to nail them fast.

I talked repeatedly with Paul and he knows I believe he is brewing hot water for himself. He wanted to go to bat, however, so what could I do? This is not an institutional matter in which I could give orders, but free science. I told him he was likely to be mainly or partly right--he is finely organized and his intuitions are good even if not always balanced--but that this way of doing it would never go down. We argued the point only last week before some friends who were interested in the psychology involved. He stood pat: and I think we must put up with him. The course to follow is one between getting

ourselves identified with his activity and repudiating him publically. The latter would be construed as dictated by fear, and would be so.

Why don't you write a general review of the situation--my impending Penutian, Paul's abomination, Swanton's Atakapan,<sup>2</sup> rap Harrington for not producing data, etc. That would give you a chance to wipe Paul's tar off yourself without tying him hand and foot and dumping him overboard. If you attack him he will certainly come back in his childish temper and we should have a squabble. There is need of a dispassionate examination of the several related efforts; and a calm criticism of Paul's in with the others would weigh much more heavily than a pettish one directed wholly at him. You are irritated now; and that would only help him, or at any rate divert progress.

Do let me see whatever you do before you print it.

Kroeber

- 
- 1 Sapir had apparently sent Kroeber the draft of a scathing review of Radin's monograph on North American genetic relationships (Radin 1919a).
  - 2 Swanton (1919) had recently proposed grouping Tunica, Chitimacha, and Atakapa together as a genetic unit. Swanton called this extended group "Tunican", however, not "Atakapan", at least in his final published report (1919:56).

[276]

Dear Kroeber,

Can you send some more copies of my "Yana Texts"? I am all out of them. Say about a dozen or two.

Yours,

E. S.

Ottawa, June 24, '19.

[277]

Berkeley, California

July 11, 1919

Dear Sapir:

Belabor me or not: it's not my business to play censor to authors.<sup>1</sup> I see to it that their papers are worth printing, or that they present a prima facie case in favor of printing, and that they are reasonably organized and written; and then I wash my hands. I pointed out to Paul both before and after he wrote that what he planned to do would never be taken as sufficient evidence; and his consistent stand was that it was sufficient for himself and he didn't care about anyone else. You know his reactions when he is left free. Well, he was entitled to an innings, so I let him go to bat. After all even this governess business has its limits when one's charge is a man of 35.

So I regret nothing. It would rather nice if Paul were once to get professional commendation, but if he won't earn it he'll not get it. In the last analysis he is like Goldie and Wallis: he prefers the vague corona of anthropology to its heart.

You are quite correct as to my type of memory. I have to painfully collocate material under my eye. If I were differently endowed I should probably have gone as far as you in these matters by now. I play conservative because I feel my weakness. Paul's memory is somewhat better than mine, but as in all matters he likes soft contacts, therefore the outlines are hazy, and in spite of great speed and keenness of apperceptions he is never thoroughly analytic. He shrinks from the knife. This is the basis of practically every one of your counts: most of which I should accept even without your substantiation. As Boas once put it in his peculiar way, Paul "lacks the icy enthusiasm" which is necessary for science. He certainly does. These neurotic tendencies of personality are unalterable, and of course come out in a thousand ways.

I particularly like your point that he dissolves all existing syntheses into a protoplasm from which he reconstructs subjective organisms of jelly-like and fluctuating organization. He is only happy when he can do this. He hates the hard and fast, the definite, the accomplished. It would hurt him to

achieve anything. He loves to dissolve, as you say, and then lets his mood play idly. He is an absolute introvert. That is why he is at his best in a drifting conversation and at his worst with a job on hand.

Well, I'm glad you feel less harshly. The matter will find its own level and way out, and a row would have been a pity.

I was not hurt about the story. Rather you let me off easy. And your criticism is remarkably penetrating. So far you are the only person who has seen that there is no symbolism, that I unconsciously replace narrative by a series of images, and that the endeavor is to form a new style. The Indian influence is there, but only furtive. There is just as much Old Testament, and lack of both an old (and only half-articulate) but strong personal impulse toward what you so aptly call starkness. The excess of "ands" is only a natural excrement of this. Everybody hollers at the offensive mannerism. But try to write a page of literary prose which is free from the accepted sentence structure of English without connectives and see where you get. I agree that English prose is set. Perhaps the only recourse is to use verse if a purely literary or poetical effect is wanted (novels of course are non-literary in style as well [as] organization.) But your suggestion of a rich rapid blank verse, sound though it is, made me laugh as if you had advised writing in the exact style of Keats. So aus dem Aermel geschuetzelt.<sup>2</sup>

I plead guilty to every awkwardness and inconsistency. I feel a good many you do not specify. It takes considerable gift to evolve a new technique, and I expect I have no business to try. The inadequacy of the result often appalls me. But I can't help making the effort that way, if I'm to do anything.

What you say about colorlessness of milieu I also accept. My next best critic charged that some perversity had led me to devitalize everything. No doubt. But what can a man do who with every striving for objectivity and concreteness abhors what we are wont to call realism and feels it as a blight, in the face of which only the greatest talent manages to succeed. The whole trick of realism is the anti-aesthetic one of putting the milieu over at all costs. When this is dazzlingly done, as by Kipling or Chekoff, it blinds one to the essential banality of the story. We can do doubt intensify every

emotion, aesthetic or otherwise, by fixing it in time and space, handling it with names and generally making it familiar ("Casey Jones", for all its vulgarity, is superb in the skill with which it does this). But modern art and literature both are so taken up with this trick that they subordinate everything else to it. Let the emotions be trite or cheap, as long as the milieu is living? Well, that turns a means into a prime end, and I won't play that game. No doubt I overdo my reaction, and am straightened and hampered enormously by my aversion--or obsession--against bringing in the least actuality that is not indispensable. I lean backwards and yet I have the satisfaction that however little I get away with the attempt, it is free from meretriciousness.

There are people so unimaginative that they want the looks of every character described. Let those who wish humor them.

This is of course not a defense of what I've done, but a profession of endeavor.

A subjective treatment is another way out; but this I seem incapable of, as you have probably gathered from certain lacks in my remarks on your verse.

Well, I am extremely appreciative of your analysis. I only wish I didn't have to admit it's ninety-nine percent correct. The only people who have professed to like the story have let it go at that: in proportion as they became specific they have slammed it.

I'll send you one more after I get it copied.

Yours

Kroeber

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1 Sapir seems to have taken Kroeber to task (in a letter not preserved) for not editing out the more outrageous portions of Radin's paper on genetic relationship.

2 I.e., so "off the cuff" or "spur of the moment".

[278]

[Berkeley]

August 3, 1919

Dear Sapir:

I have got Paul to manifest shame and regret at not sending back your verse. I rather expect though that by the time he returns to Berkeley tonight these feelings will have (future perfect as English tense!) evaporated too much to drive him to action on a point on which he has a clear psychasthenic resistance. There is only one way you are likely to get the mss., and I shall adopt that. You will have to wait a month however as I am likely to be away a fortnight and pretty lazy the first days after my return.

Yours

Kroeber

[279]

[Berkeley]

August 17, 1919

Dear Sapir:

I don't know about being a mother to Paul, but I said when I took him on that therewith I became governess. And he isn't so much a spoiled child as a constitutional neurotic. And you know as well as anyone that a neurosis is only repressed and accentuated, not cured, by bullying. He will of course never overcome his very far, but he can be fitted usefully into society and be made rather happy by gentleness and tactful steering. I think the net effect of his paper<sup>1</sup> will be all to the good. It is easy enough for you to wash your hands of its extravagances, but I didn't see what you would gain--nor the subject--by a savage attack. I should like nothing better than a really dispassionate examination, and so would Paul. He is really extraordinarily dispassionate in all intellectual matters, even where he is involved--far more than I, for instance; but his masochistically cringing

sensitiveness completely unnerves him as soon as the criticism takes on any personal emotion. And that you were injecting in your first reaction. Paul's psychasthenias are so astounding as to be remarkably interesting: the return of your verses is a case in point. The mere fact that a slightly unpleasant or troublesome thing has to be done swells it into a mountain of horridness: and he unconsciously fights the person whom he holds responsible for this looming nightmare with every kind of evasion. He has boasted to me of the successfully long delay to which he subjected me in parallel cases--work, at that, which was part of his contract and for which he was taking pay. It is his emotional compensation for the indignities to which his weakness subjects him: a victory in the world of unreality in which he lives. I have only just got through a case: for two months he kept funds advanced for fieldwork which he did not do. I know positively that he had the whole of the momey all the time; but writing and handing Gifford or me a check for the amount swelled with each successive delay into a more insuperable difficulty, just as I think at bottom he knows exactly where your poetry is lying and if he really wanted to could lay his hand on it in three minutes. In my case he admitted both that he was inevitably putting himself in the position of an embezzler in the eyes of Gifford and whoever else learned of the situation, and that he was doing me a gratuitous injustice by putting me tacitly in the position of winking at his action. He was really concerned about the latter, but this did not prevent him from coasting a month longer, and if Gifford had not finally got him physically into a corner, he would still be owing the money to the University under circumstances which, though thoroughly innocent, would never bear official examination.

What I should like to know is whether these traits are old or have materially developed in recent years. The unsolved neurosis may simply be accentuating with age; or the sort of duress in which he lives toward Rose<sup>2</sup> may gradually have aggravated his habits; or he may always have been the same. I have only known Paul about two years, and would give a lot to talk him over with someone who was both intelligent and sympathetic about him and knew him well ten years ago: you or Goldie, for instance, though I doubt whether Goldie retains enough sympathy. Lowie knows him well enough to predict rather



successfully how he will behave in a given set of circumstances, but is too essentially lacking in psychological-mindedness to be of much help for analysis; while the rest, like Boas, Laufer, Wissler, and Goddard are too definitely antagonized by having had the irritation of dealings with him, or by the mere furtiveness of his manner. Tozzer,<sup>3</sup> by the way, thinks very highly of him, but then their contacts have been purely conversational. Do you realize of all the men in anthropology in America you are I remain the only ones who do not either dispose of Paul as a "queer fish" or rate him as a sneak? That's his doing, of course; but I think makes it behove us to be as tolerant as possible. I have affection for him, with all his trying me, and as for his intellect, my admiration still grows. Even his paper appeals to me more as being an emotional weakness than an intellectual lapse. If it's worth while, I'll justify that position against your recent analytic criticism of it. And if you can contribute anything on the relation of his past and present, I'd appreciate it.

He was operated on for appendicitis a week ago, by the way.

I haven't much news that will interest you. I worked more or less on my Yurok myths during the summer: the points I am trying to bring out as to individuality, treatment, and style would interest you; but the work not being based to any considerable extent on texts or knowledge of the language, I expect you will shrug your shoulders when it is done.<sup>4</sup> For variety and as a mental discipline I have just put in several days on Freud's Totem and Taboo (the old Imago essays in English,) the result of which is going to the Anthropologist.<sup>5</sup> Paul wholly dissipated his summer but Gifford continues to increase his woodpile. It proves how much that is useful can be done without imagination. I begin to think that the world overrates the faculty, stirring and pleasing as it is to encounter.

Kroeber

---

1 Once again, the reference is to Radin's paper on genetic relationships in North America (Radin 1919a).

2 Radin's first wife.

3 Alfred M. Tozzer (1877-1954), of Harvard, a specialist in Middle American ethnology and archaeology.

- 
- 4 Kroeber put this work aside, returning to it only in the 1950's. His full study, Yurok Myths, was published posthumously (Kroeber 1976).
  - 5 "Totem and Taboo: An Ethnologic Psychoanalysis" (Kroeber 1920a).

[280]

Berkeley, California

August 31, 1919

Dear Sapir:

Will you try your criticism on this one?<sup>1</sup> The style is probably but little toned down, but I think the treatment is more rapid. I should particularly appreciate anything on the underlying psychology--not of the story but of the author.

The writing was done before I had your comments on the first one.

An enrolment of over 400 in the introductory course has kept us jumping for a fortnight to establish organization. I see quieter times ahead now.

Yours

Kroeber

---

1 Kroeber apparently enclosed the manuscript of another short piece of fiction with this note.

[281]

University of California  
Berkeley, September 11, 1919

Dear Sapir:

Under separate cover I am sending you forty reprints of your History and Varieties of Human Speech.<sup>1</sup> They are a bit rough but I hope they will answer.

Sincerely yours,  
A. L. Kroeber

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<sup>1</sup> See Letter 263.

[282]

Geological Survey  
Ottawa, September 17th, 1919

Dear Kroeber,

Thank you for the forty reprints of my paper on "The History and Varieties of Human Speech" which I have just received. Sometime ago I wrote to several individuals who I know had copies of this paper to forward them to you. Those who reported to me were Waugh, Wintemberg, Michelson, Teit and Barrett. It may be that several others sent you their copies without letting me know. If so, I should be much obliged if you could let me have their names, so that I may send them reprints to replace their original copies. Those that I have mentioned have been supplied with new copies.

I am eager to see the complete volume of which the reprint is a part.<sup>1</sup>

Yours very sincerely,  
E. Sapir.

---

<sup>1</sup> Selected Readings in Anthropology (University of California 1919).

[283]

[Berkeley]

September 21, 1919

Dear Sapir:

I take it you want the embroidery without the tray.<sup>1</sup> Those made for the purpose run about \$2 to \$4. I cannot recommend them highly. Most are coarse showy gold thread. Others are blue and green, pleasing in tone, purely decorative, but tamely conservative. There are a few pieces in a new style of crude figures in many garish colors, something like Mother Goose lithographs à la Chinese--out of the ordinary, but attractive only in their semi-grotesque qualities.

There is another style of embroidery in strips, usually but not always in pairs, each about 3 X 12 or 4 X 18 inches. These can be worked into cuffs, smock trimmings, or put, singly or double, into a [illegible] tray. Unframed, they seem to make a better gift on account of their plasticity of use. They run about \$2.50 to \$5 the pair. I saw many fair ones but nothing startling except one or two old ones, on whitish ground, somewhat faded or stained. They had evidently been worn in China. If your friend is the sort of woman whose preference for "antiques" outweighs her objection to "second hand" goods, I think I could pick you up a striking piece for \$3 or \$4.

There's nothing much around \$6 to \$10. Large embroideries are skirts or piano covers and if they have any quality run considerably higher.

The knick-knack I feel shakier about. It would probably be a Cloisonne vase, and I have little feeling for them, except the texture of the sheen and that can run up into money, whereas the inferior ones leave me cold.

There are ready made trays at about \$6, \$7.50, \$9, \$12.50, etc. but they hold the conventional tray embroideries, and there would be nothing distinctive about these.

Why not cut your \$10 in half and let me get you a good faded strip embroidery?

Kroeber

- 1 Sapir seems to have asked Kroeber to purchase some oriental art objects for him in San Francisco's Chinatown.

[284]

University of California  
Berkeley, October 7, 1919

Dear Sapir:

Replying to your letter of September 17, let me say that so far as our records or the copies of the pamphlets show, the people who furnished them are: Lowie, Goddard, Gifford, Waugh, Wintemberg, Michelson, Teit, and Barrett.

Sincerely yours,  
A. L. Kroeber

[285]

University of California  
Berkeley, October 7, 1919

Dear Sapir:

I have got from Paul manuscript of your Paiute Grammar<sup>1</sup> and a volume of my Arapaho papers. I am sending you these collect inasmuch as we cannot ship under a frank. I have not yet got your verses from Paul because he has them at home and I have not had occasion to break into his house and rifle his desk although I shall do so if it becomes necessary.

Sincerely yours,  
A. L. Kroeber

---

1 Southern Paiute, A Shoshonean Language (Sapir 1930-31).

[286]

[Berkeley]

October 20, 1919

Dear Sapir:

I have the stuff and it comes to a couple of dollars less than you allowed.

Tray embroidery	3.--
Tray embroidery	3.--
2 doilies	1.--
10 yds curtaining	1.80
2 prints	1.50
	<hr/>
	10.30

I'm hoping Mrs. Sapir is satisfied. I rather liked the buying. As to responsibility, I figured if I chose badly, she would have more to worry over than I. Most of the stuff isn't as good as I should have liked; but to get 25% more quality would have meant 100% more cost to you, and articles of somewhat different type. Prints in particular have gone up. The stock is smaller than it used to be, and the prices triple: \$2.50 up for decent ones.

I'll ship in two packages by express, filling out the blanks here if I can do so without excessive trouble, otherwise asking you to have this detail attended to.

Yours

Kroeber

[287]

Ottawa, Ont.

Nov. 4, 1919.

Dear Kroeber,

The appearance of your and Dixon's linguistic paper<sup>1</sup> reminds me of 3 or 4 papers submitted to the "International Journal of American Linguistics" but not yet published. I am sending them to you enclosed.<sup>2</sup> Possibly you are no longer interested. The Chimariko point I make I consider a real "find". My view of Mason is really further discussion of Hokan. I cannot understand why neither you nor Frachtenberg have ever discussed the Penutian type of stem with repeated vowel, which I take up in the longer paper. Could you use "A Characteristic Penutian Form of Stem" in your series?--Please return at convenience.

Sincerely,

E. Sapir.

P.S. Have only begun to read your paper. Hope to find much of value for my own "extended-Penutian" study. Am looking over Chinook some nowadays. Most interesting specialized development on Penutian base.

---

1 "Linguistic Families of California" (Dixon and Kroeber 1919).

2 These must have been: "The Hokan and Coahuiltecan Languages" (1920a), "A Note on the First Person Plural in Chimariko" (1920c), "Review of J. Alden Mason, The Language of the Salinan Indians" (1920d), and "A Characteristic Penutian Form of Stem" (1921a). These had been written much earlier--all but the review of Mason in 1915--but Sapir had not been able to find a suitable journal for them until IJAL came into existence (the first issue appeared in 1917).

[288]

Ottawa, Ont.

Nov. 8, 1919

Dear Kroeber,

Just got embroideries and curtaining. Prints not arrived as yet. I think your choice was very good and we are both very well pleased. Thanks!

Am going through your lists.<sup>1</sup> It is tempting to reconstruct hypothetical prototypes. I feel the typical Penutian stem is long, often 3 or 4 syllables in original form. E.g., no. 118 ("fish") seems to me to reconstruct pretty easily to \*ilapita. You will find this grouping of vowels and consonants explains every form you give, with easily accepted phonological processes, except pu, which I don't think belongs with the rest<sup>2</sup> (I think you might have weeded out some to advantage). Or take no. 151 ("one"). \*yikati seems to cover all forms; e.g. W. \*yikáti > \*ikáti > \*kétí- > kéte-; Md \*yíkatí dissimilated to \*wíkatí > \*wíkti (wite = \*witte < \*wíkte ?); Y \*yíkatí > \*yékaçi > \*yekç > yetç; Mw \*yikáti > \*ikáni > \*kéne. In Y and Mw a is unlauted to e by following i. So far, so good. But now comes the exciting part. "One" (personal) in Chinook is -ixat, "one" (non-personal) is -ixt. But there is very good evidence to show that postvocalic -x- in Chinook is frequently spirantized from -k-, hence -ixat easily reconstructs to \*-ikat, only a step from our hypothetical \*yikati. And Coos has yixé<sup>i</sup>, which is pretty close!

I'm glad to see your paper out. It is a notable and valuable advance in comparative linguistic work in America.

Sincerely,

E. Sapir.

1 The 171 sets of resemblant lexical forms in the five California Penutian languages in Dixon and Kroeber's "Linguistic Families of California" (1919:56-61).

2 A Plains Miwok form.



[289]

Ottawa, Ont.

Nov. 10, 1919

Dear Kroeber,

I have just finished your linguistic paper and have enjoyed it very much. Just a few notes.

I don't see why you express yourself so cautiously about Washo.<sup>1</sup> I think that between us we have accumulated enough evidence to prove the Hokan character of Washo beyond reasonable doubt. In structure it is more typically Hokan than Yana or perhaps even Pomo. Many of your new cognates are very convincing, too.

Clearly Maidu has been much modified by Hokan influence, particularly morphologically. I suspect Shoshonean influences, e.g. in certain instrumental prefixes, have been pretty strongly at work too. Conversely, I imagine Pomo to have been somewhat modified by contact with Penutian. The key language for Hokan is probably Chimariko, possibly Shastan eventually; for Penutian, Yokuts (I don't see why locative -u is from -tu; you have u-locatives in Coos and Siuslaw, survivals in Takelma).<sup>2</sup>

I don't feel satisfied about Yuki. Your Hokan parallels seem more convincing to me than your Penutian ones. I suspect it is a very divergent Hokan language that has a[t] one time developed in isolation from other Hokan languages, being, like Pomo at a later date, somewhat modified by Penutian influence. Pomo is in many aspects so close to Shastan, Chimariko, and Yana, that I believe it must at one time have been geographically contiguous with them, leaving its present territory occupied by Yuki and Penutian (possibly Wintun). Before Pomo pushed W., in other words, Yuki was an isolated splinter of Hokan. Possibly Yuki needs to be compared rather with Hokan-Coahuiltecan than with Hokan in its narrower sense.

Progress in this tangled field will require clear working out of phonetic laws and clearer feeling for stratification. Secondary developments and

borrowings must not be confounded with primary features. That's where Paul so often goes astray.<sup>3</sup>

Sincerely,

E. Sapir.

- 
- 1 "... sixty parallels of greater or less validity... is not a wholly convincing showing. But the general plan of Washo structure is so similar to that of Hokan that material resemblances weigh more heavily" (Dixon and Kroeber 1919:105). A few pages later Washo is noted as a "probable addition" to Hokan (1919:112).
  - 2 "A general locative is marked by t followed by a vowel: Yokuts -u evidently represents former -tu" (Dixon and Kroeber 1919:95).
  - 3 In Radin 1919a.

[290]

Berkeley, California

November 13, 1919

Dear Sapir:

The Chinese things were shipped on October 22 after some delay in getting the requisite papers made out. I am now in receipt of a notice from the frontier station of the Express Company that the goods cannot be put through without papers. Inquiry at the local office reveals that they have probably been mislaid and that I must file a new set. I am doing this to-day. I advise you because you are no doubt wondering and perhaps chafing at my apparent delay.

Sincerely yours,

A. L. Kroeber

[291]

[Postcard]

Ottawa, Ont.

Nov. 13, '19.

Dear Kroeber,

Your 2 Japanese prints have not yet arrived. Did you send them? If so, by mail? Please let me know at your early convenience.

Sincerely,

E. Sapir.

[292]

Ottawa, Ont.

Nov. 18, 1919.

Dear Kroeber,

Have read Miss Cope's paper<sup>1</sup> with interest, though I do not know that much of consequence ensues from it all. I should have liked the month names correlated more extensively. There are quite a few Nootka mistakes. Enclosed may be of use to you or Miss Cope.

I noticed several examples of carelessness and gather she is unfamiliar with tribal nomenclature. In map 3 she gives "Tlingit" and "Chilkat". Of course the former includes the latter. In the text (p. 148) she states that rutting names are particularly characteristic of the Osage, but map 3 does not include the Osage in the "rutting area"!—On p. 144 she refers to the Chehalis and quotes Hill-Tout, but Hill-Tout never worked on the Chehalis, a Washington tribe. She seems to have confused Chehalis with his StsEélis. On map 3 she locates Chehalis correctly, but it is not, so far as I know, what her authority means.—On p. 149 she lists Tusayan as a tribe, but she has already listed Hopi.—On p. 164 she gives "Montagnais" of Petitot after "Montagnais" of McKenzie. Of course the former is really Chipewyan (both Petitot and Legoff

use "Montagnais" for this tribe), while the latter refers to the Algonkian Montagnais of the lower St. Laurence region.

She has not studied her material very closely. Thus, in speaking of Numeral Systems, it would have been worth while to refer to the Nootka names for 2 months, ("older sibling" and "younger brother"), also to 3 Kwakiutl names ("elder brother", "under elder br.", and "next one under"), as these show seriation implicit in the terminology. These two tribes would have given more body to her Numeral Type distribution in map 3.--I don't care much for her classification, as the "astronomical" type (which boils down to observation of solstices) may at the same time be either "numeral" or "descriptive". Still, the whole thing is a good start, if only to show how defective our information is.

Sincerely,

E. Sapir.

---

1 "Calendars of the Indians North of Mexico" (Cope 1919).

[293]

[Berkeley]

November 22, 1919

Dear Sapir:

Will you let me have your xxx ratings<sup>1</sup> on as many as possible of the following: Spinden, Mead, Earl Morris, Hough, Fewkes, J. P. Harrington, J. N. B. Hewitt, Frachtenberg, Dorsey, Hooton, Kidder, Jenks, Barrett, Aitken, Hawkes, Wallis, Alice Fletcher, Edgar Hewett, Morley, Farabee. I would rather answer your several points after I have your returns.

Are you going to the Cambridge meeting?<sup>2</sup> I very much wish you could. I leave for the east on Dec. 6. From Dec. 13 on address me at the American Museum.

Yours

Kroeber

- 1 Kroeber seems to be soliciting Sapir's opinions on the qualifications of the named anthropologists for appointment to the Council of the American Anthropological Association. Kroeber was the Past President, and Sapir a member of the Executive Committee of the Association. Sapir's "ratings" (which originally accompanied Letter 295) have not been preserved, but on Kroeber's original letter Sapir has placed an "X" above the names of Earl Morris, J. N. B. Hewitt, Hooton, Kidder, Jenks, Aitken, Alice Fletcher, and Morley.
- 2 The Annual Meeting of the A.A.A., in Cambridge, Massachusetts, Dec. 29-30, 1919.

[294]

University of California

Berkeley, November 25, 1919

Dear Sapir:

I very much wish you could arrange to come up to the meeting at Boston.<sup>1</sup> I think we shall have a representative attendance. It is one of the few places at which meetings always develop definite spirit. I should be very pleased if you could make your trip a little longer and manage to include a few days in New York before or after Boston.

I am planning to present a paper on behalf of Paul discussing his method and point of view in regard to relationship, and also want to add some remarks of my own.<sup>2</sup> We have got so far in this work that it seems desirable that we sit down to take stock and find out what we are in substantial agreement about. There will always be and should be differences, but we have surely got to certain results in which all of us can concur. It seems to me that we have overlooked this common element and are accentuating minor differences so much that our private as well as our public discussions are over-disputatious.

It will not be long before we have some new linguistic material available for you. McKern<sup>3</sup> has made very definite progress with southern Wintun. He is very slow but thorough. Miss Dangberg,<sup>4</sup> who holds a University fellowship, had begun on Washo on her own initiative. She blundered along rather ramblingly but we gave her a chance to sit in on some Wintun and Maidu and she

picked up the technique very quickly. She will be back in Nevada over Christmas, and with the foundation that has been laid I expect she will return here with a really good body of Washo material. I think she controls the phonetics with complete reliability. We have just finished having a Northern Wintun and a Southern Maidu here for the benefit of a class in field methods.<sup>5</sup> Half a dozen of them joined in and Paul gave them active assistance. He is throwing together the joint results on Wintun and Miss Dangberg and another girl, Miss Freeland,<sup>6</sup> are going to work up the Southern Maidu.<sup>7</sup> I think they have fully as much material as Dixon published on Northwestern. Miss Freeland has great native aptitude for the work and if she will stay with it will do something distinctly worth while.

I feel rather relieved at this turn of events. The relief is a bit mixed with regret at seeing myself passing out of the game, but it is better so.

From a number of errors which I caught I suspected that there were others in the Cope paper.<sup>8</sup> I did not see, however, how I could go back to the original sources and work them all over. The root of the trouble is that our students have no basis of real scholarship or training. Miss Cope is usually painstaking. In some ways she is over-conscientious. She simply has never had the chance to soak in the attitude that certain little things count for a lot and that certain problems are only met in a certain way. A year or two at Harvard or Columbia would have helped her a lot. Instead she is now teaching high school in Placerville. You will have to take her as she is--the good mixed with the bad, in the same spirit of tolerance with which you take Barbeau and Waugh.

Sincerely yours,

A. L. Kroeber

- 
- 1 Actually Cambridge (the 1919 annual meeting of the American Anthropological Association).
  - 2 "The Problem of Genetic Relationship of Languages" (Radin 1919c). This paper was never published.
  - 3 W. C. McKern (1892- ). McKern published two short papers on Patwin ethnology, but nothing on the language. His career focused on archaeology, and from 1925 to 1958 he was on the staff of the Milwaukee Public Museum.

- 4 Grace M. Dangberg (1896- ). Her only published work on Washo linguistics was a volume of texts (1927).
- 5 The Southern Maidu (Nisenan) informant was William Joseph.
- 6 L. S. (Nancy) Freeland, later the wife of Jaime de Angulo. For a biography see Leeds-Hurwitz (1982).
- 7 None of the Wintu or Nisenan linguistic material collected at Berkeley in 1919 was published. A short paper by Faye on Nisenan ethnology (1923) is apparently the only published record of this work. Radin's Wintu notes are now in the Library of the American Philosophical Society (MSS 3904-3905, and 4905).
- 8 On calendars (Cope 1919). See Letter 292.

[295]

Ottawa, Ont.

Nov. 29, '19.

Dear Kroeber,

Ratings enclosed.<sup>1</sup> Have several ideas about the subject, but more anon.-- Still don't know whether I'm going to Cambridge, but hope to. We must try to get together this winter. Au revoir.--How about those Japanese prints? They did not show up.

Sincerely,

E. Sapir.

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1 See Letter 293.

[296]

[On board the] Overland Limited

12/8/19

Dear Sapir:

I saved up your five months' accumulation of verse for this occasion and have gone through the whole of it in one undisturbed sitting. I said last time

that your stuff lacked passion. I will now add that it lacks restraint. You take an idea or feeling which, compacted into eight lines, might make a firm intense poem and spread it over a page and a quarter. The greater the number of images that the central thought can throw off, the more richly poetical you seem to think the whole is; whereas it only weakens, and no amount or quality of diction can save it. At least you'd have to equal Keats to make your method go. A poem must be charged; surcharged, if you will. Cut yours to the bone, and your stock of emotion will gain in potency. You may not think so; but your relatively commonplace passages are by far the best. This is not philology, in which you are a past master who can play with technique and refinements, but an art in which you are a novice. Draw a short straight line, hew to it, and I am sure you will meet more encouragement. You have a gift of expression. Don't waste it.

I'm too far away from poetry to give illuminating criticisms on separate poems, so have confined myself to brief reactions which are meant to be taken in the sum. The one thing I have done that may have some value, I think, is checking the lines to which I react as poetical. (The unmarked ones, except for sporadic instances, I believe would impress most people as not being "real" poetry.) Look them over and you may get a sense of that manner in your work which impresses me as worth while. If you doubt the typicalness of my reaction, ask someone else to do the same thing on half a dozen pieces: not a poet, who is likely to be intolerant, or a free versist who is prejudiced on theories; nor a dead dub but an educated layman who occasionally reads poetry for pleasure.

All my marks and comments being in soft pencil, you can readily erase them. I had to work without table on the train.

Thanks for your experiments with "Re" and your full criticism. Other people have liked or not liked the two stories; you have been the only one willing to go into them. Surely, the style is the one thing that counts; but this in turn is subtly and wholly dependent on the underlying substance. I may have a few more for you before long.

Do try and make Cambridge and N. Y.

Kroeber



[P.S. at head of letter]

I have not had a chance to mail this until now. Hope to see you in Cambridge.  
ALK 12/19/19.

[297]

[New York]

December 27, 1919

Dear Sapir:

I had already diminished your number in the last column by one. I think I had you at 2 before. However, be thankful if it's so. These god damn long-functioning individuals like myself pay a bitter price for it.

Listen. I am inclined toward the classic, at least the formal. That means I have little capacity for appreciating disintegrated or unintegrated form. But I'm not wholly devoid. There's a good deal of free verse I've read with pleasure. It simply happens that I feel more value by formal standards in your formal verse than I see by standards of unintegration in your unintegrated verse. Deduct as much as you like for subjective limitations; something of a definite personal reaction remains; and what else is aesthetic criticism? The little review in the Dial said the same thing.<sup>1</sup> I shouldn't ever have made this comment if you didn't so often seem to throw away very reasonably satisfying attainment by a sort of perversity. If a poem of 12 lines has eight formally good ones, the sense of cheating which the other four give is not lessened by your declaration that their anarchy is intentional. I don't get the effect you intend; I only hear their grating; and if I didn't tell you so there'd be no use saying anything. I think your suggestion wise: write in two manners; one will please Scott and the other me; but keep them apart.

The more fundamental thing I should again criticise is the over emphasis you put on subjective moods as poetic material. A lyric must reflect a definite mood; it must therefore be subjective. But that doesn't mean you can make a lyric out of a fit of irritability, as you've done or any other

transient evanescence of emotion. That's what I meant last spring when I spoke of the comparative lack of passion. Read the Stanzas Written in Dejection, subtract the mood, and when you bare the kernel that remains you'll see what I mean--what they call the "permanent" or "universal" or "fundamental" elements of poetic thought. I suspect you don't really give much attention to this side, and that the bringing in of God, death, etc., is largely an easy substitute for attending to it. When you don't strain after it, it sometimes comes quite spontaneously, as in the returned soldier talking with the four aunts.

I know I'm a rough critic, but if men can't talk to each other straight from the shoulder on what concerns them most, we might as well quit altogether. There are few people I feel kindlier to than you. I'm awfully sorry you can't make the meeting.

ALK

- 
- 1 A brief anonymous review of Sapir's published collection of verse appeared in The Dial (67:174, August 23, 1919):

Dreams and Gibes, by Edward Sapir..., is seldom of the magic of poetry, but often of such harsh, warped magic as a poet these days can at best hope to wring from life. These lines reveal a free, idealistic spirit whom the world finds more than usual difficulty in hog-tying and hamstringing. Many of Mr. Sapir's shorter pieces show a valuably suggestive turn of thought--not quite poetically "turned." Excrescences, too, are apt to be tacked on these tiny things, at their endings. Of the longer poems, strangely enough, two in more conventional forms, The Woman on the Bridge and The Water Nymph, come perhaps as close as any to beauty as well as depth.

[298]

[Berkeley]

January 23, 1920

Dear Sapir:

I send you three more stories. I am much less satisfied with them than with the last. One is as well done as I can do, but insubstantial. Another amounts to more potentially but is very inadequately realized. The third seems to resist working out both in matter and form.

In addition to literary criticism, you may be in position and feel free to render some opinions as to my psyche. I am rather surprised that I have never had a consequential reaction along this line from any of the several people who have seen or heard any of my stories. Some of them were in a position to be frank with me. I don't know whether they were reticent or uninterested in my psychology. Go as far as you are inclined.

All three of these stories were put into final shape only recently, but their first conception and attempted working out are of rather widely separated date[s]. I wonder whether you have any inkling of their temporal order.

I think it's more than mere self-centering that leads me to suggest the injection of my personality in your criticism. Obviously I could better deal with the defects in my style and subjects if I saw more clearly what parts of me they were associated with. In spite of every mannerism of which I am aware, there is a great deal of unconscious in these scribblings; some of them is almost compulsorily automatic: once started, I have no freedom but must do it that way.

Kroeber

[299]

University of California  
Berkeley, January 28, 1920

Dear Sapir:

I recently got out of my desk three Yana lists which you furnished me in 1911 or 1912.<sup>1</sup> The first is of derivative suffixes, the second of formal suffixes, the third of verb stems. I should like very much to put these together, have you write an introductory paragraph or two and issue the whole as one of our publications. The material would be of use in comparisons, in the analysis of forms in other Hokan languages, and above all would unlock your Yana texts. The latter are now practically useless to any worker who is not ready to put some weeks or days of analysis upon them. If the completion of your Yana Grammar were within sight I should not now make this proposal. But twelve years have elapsed since you got the data on this language and we might as well be frank and admit to one another that there is a very fair prospect of another twelve passing before your complete grammar gets done. Publication of these lists would at least give the scientific world something to go on during this long interval.

I expect your first reaction will be negative. I believe you may look upon the publication of these lists as in some way prejudicial to the value or effect of your grammar. If you do have this impression I think a little consideration will dispell it. In fact I am sure that your grammar will be more keenly looked for and more appreciated when it does appear if you provide this preliminary offering.

In any event while I am waiting to hear from you I shall go ahead and have the lists typed so that if you agree they may be promptly submitted to you for orthographic revision and the writing of the introduction.

I am not surprised that the agitation which Boas has caused leaves you a little puzzled.<sup>2</sup> You are probably too thorough an individualist to feel very keenly an offense against either scientific or patriotic solidarity. Ultimately the trouble can be resolved into a clash between these two ideals. Boas has scientific highmindedness but is weak in his nationalistic feelings,

in fact in the present instance has a definite anti-nationalistic bias. The people who are most active against him have an average man's patriotic sentiments more or less in the form that was cultivated in the war. With scarcely an exception they also have not mentality enough to be genuinely interested in science. Consequently they see scientific principle only as a form of treason as soon as it clashes with their patriotism. They very correctly sensed the animus in Boas's letter, and met it with equal animus. To be sure they reinforced this with a lot of accumulated personal vindictiveness and jealousy. But on the other hand it is impossible to wholly acquit Boas of animus towards the men he refers to. It is a sorry mess. Apart from what he believed right and wrong the old man should have had sufficient instinct to realize that he could not write his letter without stirring up almost unlimited trouble, and that the only thing he would accomplish was trouble for himself and his friends and possible triumph for those he disapproved of.

The events in substance were these: the positively patriotic anthropologists and Boas's enemies in the profession who cannot always be differentiated, promptly joined hands in New York, Washington, and Cambridge, caucused, made their plans, brought them into the meeting, and made a vote. They prevented Boas's reelection to the Research Council, practically forced his resignation from the unexpired remainder of his term, expunged his letter of resignation and explanation from the minutes of the Anthropological Association, put into the minutes the Washington society letter of condemnation, and passed in the Anthropological Association a vote of censure on him. All this went through on a vote of 2 to 1. It is their victory and if nothing further happens there will be no great damage. The old man has promised Tozzer<sup>3</sup> and me that he will not reopen the subject for at least a year. My only fear is that his enemies will realize his stubborn aggressiveness as a fighter and may try to continue the war until they have broken him. They may not be in a position to do much but I suspect that they have already tried to bring such pressure as they command upon Butler<sup>4</sup> to dismiss him. His best chance is in the whole thing being dropped and forgotten as quickly as possible.

The brains of the movement within the Anthropological Association was Spinden,<sup>5</sup> who acquitted himself not only intelligently but with frankness.



Franz Boas

(Courtesy of the Lowie Museum of Anthropology.)

The instrument used by the Washington people was Neil Judd.<sup>6</sup> I do not know whether he primarily represented Hrdlička and Holmes<sup>7</sup> or Walcott,<sup>8</sup> but was effective because he is by nature a sadistic and vindictive person. Walcott, by the way, has fired Boas as honorary philologist of the Bureau, or rather abolished the position. Fewkes<sup>9</sup> was inclined to protect Boas but more inclined to protect himself when the attack became strong. In the final vote he, Dixon, MacCurdy, and Gordon lined up against Boas. The attitude of the Anthropology-Psychology division of the Research Council was very different from that of the anthropologists. They regretted Boas's letter but were concerned only in preventing [damage] and finding the best way out for an eminent man who had made a faux pas. There will be nothing done against Boas in the Research Council unless it is by the people at the top of the administration, notably Walcott or his friends. I doubt whether Walcott, left to himself, would follow it up any farther.

Sincerely yours,

A. L. Kroeber

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- 1 Originally intended to aid in communicating with Ishi. See Letters 67-69.
  - 2 In an open letter published in The Nation late in 1919 (Boas 1919b), Boas had severely criticized the conduct of four (unnamed) American anthropologists who had carried out espionage activities during their war-time work in Mexico. In the highly charged chauvinistic atmosphere of the immediate post-war period, Boas's criticism of these men was perceived by many of their colleagues as near-traitorous. At the Cambridge meeting of the A.A.A., Dec. 29-30, Boas was publicly rebuked by a majority of the Council. Stocking has described this occasion--and the wider institutional rift it was the surface expression of--in a well-known essay, "The Scientific Reaction Against Cultural Anthropology, 1917-1920" (Stocking 1968:270-307).
  - 3 A. M. Tozzer was serving as Secretary of the A.A.A.
  - 4 Nicholas Murray Butler (1862-1947), President of Columbia University from 1901 to 1945.
  - 5 Herbert J. Spinden (1879-1967), Harvard-educated Mesoamericanist, and presumably one of the four "scientist-spies" whom Boas had attacked in his open letter. At this period Spinden held a curatorship at the American Museum of Natural History. Later he was at the Peabody Museum of Harvard, and at the Brooklyn Museum.
  - 6 Neil M. Judd (1887-1976), anthropologist on the staff of the U.S. National Museum at the Smithsonian, where he was Curator of American Archaeology from 1919 to 1949.

- 7 Aleš Hrdlička (1869-1943), the leading physical anthropologist in the country. Both he and W. H. Holmes (see Letter 188) were powerful figures in the U.S. National Museum at the Smithsonian.
- 8 Charles D. Walcott (1850-1927), Secretary of the Smithsonian Institution.
- 9 J. Walter Fewkes (1850-1930), Chief of the Bureau of American Ethnology from 1918 to 1928. Fewkes had carried out extensive ethnographic and archaeological work in the Southwest.

[300]

University of California  
Berkeley, February 3, 1920

Dear Sapir:

What is happening to your paper on Development of the Design which you passed around a couple of years ago? If you have any copies to spare when it comes out I think that E. G. Stricklen, Assistant Professor of Music at this University, would be interested. He is an unusually level headed chap and has branched out into design, mathematics, and music.<sup>1</sup>

Sincerely yours,  
Kroeber

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1 Stricklen contributed an analysis of eight Papago songs (Stricklen 1923) to the Hearst Memorial Volume (see Letter 308 below).

[301]

[Ottawa]  
February 4, 1920

Dear Kroeber:

I do not in the least object to your publishing the Yana lists that you have on hand. I would only request that you submit them to me before you send them to the printer, so that I may correct possible errors or misunderstandings. I should like also to take the opportunity when they come of adding liberally



to these lists, as I think I can do without too much trouble. Would you like me to append to the lists a thoroughly analyzed text from the published volume of myths? I could do this quite easily, and it would probably serve to give a clearer idea of the nature of the language, than would a mere set of detached lists. I have long felt that something should be done to give people an idea of what Yana is like, and I therefore welcome your suggestion heartily.

With best wishes,

Yours very sincerely,

[E. Sapir.]

[302]

[Berkeley]

February 8, 1920

Dear Sapir:

The stories haven't yet come back; I received your letter.<sup>1</sup> I wish I could make you feel how genuinely I appreciate your cool, painstaking analysis. Don't worry about hurting my feelings; if anything you still shade severity into pleasantness. I've had a number of appraisals; but what is a judgment worth as an influence on future activity, if its basis isn't convincingly set forth? That's what you have done; and as penetratingly critically on the side of personality as of style. You surely possess an unusual combination of acumen with objectivity.

You got the order right, but I've evidently fused the materials of my stimuli so thoroughly that these are unrecoverable. Rain is a poor imitation of the Hawaiian Laieikawai—vide Kalakana and J.A.F.L.<sup>2</sup> Twelve Brothers is also old, but the source is wholly my own so far as I remember; its influence is definitely Plains for the beginning, N. Calif. + Yurok for the end. I don't even remember that Curtin has anything that bears. Fun is an old episodic concept in a larger scheme that never would synthesize. I broke it off and wrote it last spring. If there is anything [of] Lowie in it, that is wholly indirect and unconscious.

It's curious how unaware we are. If anyone else had said these stories contained symbolism, I'd have denied. They do of course: and either too much or too little. I just never felt it as being such.

As to style, I admit all you say; also as regards treatment. All that I can answer is that I haven't wanted to be mannered. That's a result of awkwardness and a curious kind of conscience (which I see exemplifications of in a hundred other matters), jointly affected by something that is sufficiently unconscious to make me feel it as a compulsion. I don't claim that I have to write these things, only, if I do, it's had to be this way. Perhaps on the basis of your analysis I maybe able to do differently. I'll be interested to try. If I can't, quitting altogether will be indicated.

I don't know whether I'm neurotic. Two psychiatrically-minded doctors who've had me under treatment and know me pretty intimately, say I'm not. But I have always normally been in some internal conflict; the free periods are usually short. I have sublimated away something, but not much: far less than has been repressed. I often wonder people don't see more of the repressions. It's only lately I've begun to realize how successfully I disguise myself. I'm very rarely conscious of having *savoir faire*. In other words I feel the mask as mask, and usually sense it as wrinkled and obvious. I have to smile at anyone taking me to be really cool and impersonal, because I have to be constantly trying to be that at all.

Your analysis of the sexual element is as sound as the rest. That you slip up in the "time perspective" is inevitable because you don't know the facts. Most of the things you guess as having been, still are, unfortunately, to a greater or less degree. I suppose after all one alters little except in youth. I was always predominantly introverted. Of late years I have definitely extraverted, but I expect the change is infinitely less than I feel it to be; and however much I succeed, I shall probably always have to struggle with the introverting impulses.

You're right about dress, especially as regards the fashion paper.<sup>3</sup> I knew it there. Also it helped give the article an appeal that it wouldn't have had if it had dealt with furniture or coins. I rather feel the fetishism is sufficiently restrained in the stories to glimmer through more than mar,

which is fairly close to sublimation. The offal and slime I can't do much with in explanation. I do get definitely pleasurable tactile emotion from the ideas, probably as infantile remnant. Perhaps my anosmia<sup>4</sup> contributes an absence of a normal inhibition.

I'm not so sadistic as you imagine. Most people feel me as such. I always am more conscious of the masochistic streak. I do resent this, and therefore cover it up, and of late years have knowingly fought against it. That may illuminate some other aspects to you. It may even be at the basis of my remark about Aztec sacrifice, which sounds natural though I don't recall it.

That I ever said few women equalled their husbands seems utterly meaningless to me. Perhaps it was a rhetorical preparation for the compliment that yours probably did. I just don't understand--as if you quoted me as liking the odor of violets.

I can't comment on your dictum that I'm not truly at home with sexuality or women. There are limits to what one can tell about himself when not under pressure.

The begrudgingness is wholly true, but is compensation or compensated for; in other words, I'm in conflict of ambivalent balance instead of simpler and integrated. Do you know that most people that understand the term consider me well integrated, though somewhat baffling? The lack of ethical motivation in writing is a similar compensation for overconscientiousness in ordering life, I fancy. I'm relieved when I can escape beyond morality. I don't deny morality; I don't seriously fight it even; but I carry it as a fetter.

If I admire recklessness but haven't the courage for it--isn't that masochism?

Well, you see you've probed deep, and I haven't squirmed unduly. Pursue when you have a mind, and I'll try to furnish more data. I'm keen on trying a new story on the basis of your findings; but I feel small hope of accomplishing much that's different. I'm forty three, and therefore know even more intensely than you what that means in the way of difficulty of cutting out new channels.

I'm stumped at analyzing you. I see many points in your work, both scientific and verse, that might mean much; but the fundamentals of your personality

elude me. If they didn't, my criticisms of your poetry would presumably be more constructive.

Kroeber

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- 1 Not preserved. See Letter 298.
  - 2 A reference to Beckwith's work on Hawaiian oral tradition. See Kroeber 1921b.
  - 3 "On the Principle of Order in Civilization as Exemplified by Changes of Fashion" (Kroeber 1919).
  - 4 Loss of the sense of smell.

[303]

Geological Survey

Ottawa, February 10, 1920

Dear Kroeber,

I have not been able to get at the actual writing of my paper on design development, but I hope to do so one of these days. I have had lantern slides prepared of all the developments, and have lectured on the subject two or three times, but the actual close study is still to be made. Enclosed are a couple of series that have been added since those you last saw. You might give me your opinions as in the former cases. Please return me photograph and notes with your comments.

Yours sincerely,

E. Sapir

P.S. Have noted Stricklen's name for an eventual copy. By the way, you never gave me the copy of your anthropological source-book you promised me. I have only my reprints. Could you get me a copy? I am curious to see what you fellows have slapped together.

[304]

University of California  
Berkeley, February 14, 1920

Dear Sapir:

I am delighted at your willingness to proceed with the list of Yana elements.<sup>1</sup> I was about to send it to you for the revision which you wish to make but I am restrained by this consideration: the list as it stands would fill about 14 or 15 printed pages. Allowing for a paragraph or two of introduction and a half page or so taken up by the title, it would therefore just squeeze within a signature. One of the rules of our Press is that any paper not exceeding 16 printed pages is put out on the short calendar. That means, goes to the composer immediately. Even although "immediately" is a bit theoretical, we usually get proof within a month or two and the paper is then promptly pushed through. On the other hand papers that go on the ordinary or long calendar are now anywhere from 6 to 12 months in reaching the linotype, and conditions in this respect are growing worse. A member of the editorial committee told me only this week that the shop was running farther behind than ever in its history.

The dilemma is this: if you make additions which cause the paper to run only to 17 pages, a year or so may elapse without any progress after filing with the University Press. On the other hand if you have anything worth while to add and can do so without undue inconvenience it seems a pity deliberately to exclude this material. It would make no difference whether the paper was 17 or 34 pages in length, its status would be the same. My own preference is to have something available in print just as quickly as possible and to sacrifice fullness. On the other hand first consideration goes to you as author and I do not therefore like to press my preferences upon you. I do feel that once you accept the delay and begin to enlarge the paper you will be under an impulse to put in more and more until you are likely to be embarked on a fairly ambitious monograph which would take time to prepare as well as to print and we should again stand where in substance we stand now. But please consider that I want the final determining factor to be your wishes.

I might suggest the following as a partial compromise. Part of your list as it stands could perhaps be printed in 2 columns to the page. This applies particularly to the derivative suffixes and the verb stems. Columning would not cut the space in half because on account of the shortness of the lines a good many would run over. But it would enable us to pick up perhaps 25 or 33% of space and squeeze these two parts of your paper, which I have estimated at 10 pages, into 7. This would allow you about 3 pages for new material. If you think you can content yourself with this and will forego the analyzed text, you may perhaps be able to include most of what you want and at the same time have your paper out before summer.

The formal suffixes, especially the pronominal elements, will not go into half page columns, in fact in part will have to be printed in tabular form as you have them.

I am enclosing the complete list. The first two portions are in your handwriting; of the verb stems I can find only the typewritten copy which we seem to have made. In making corrections or additions may I ask you to use a fine pointed pen? Your handwriting is small and unfamiliar to our stenographer and Yana is of course totally foreign to her. I think she will have no difficulty with your handwriting if the strokes are fine and narrow.

Paul tells me that you are about ready to go to British Columbia for the spring and summer. Possibly he is somewhat exaggerating the earliness of your departure. However that may be I naturally hope that you may get this little job cleaned up before you leave for the field. It would be a cause for congratulation to everyone in the least interested in American languages if something from you on Yana could be in print within a few months.

Sincerely yours,

Kroeber

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1 See Letters 299 and 301. The paper that resulted was "The Fundamental Elements of Northern Yana" (Sapir 1922b).

[305]

University of California  
Berkeley, March 9, 1920

Dear Sapir:

I return herewith your design series 26 and 27. The former seems to me to show both feeling and originality. His oval curve in particular is pleasing. 27 appears inferior. These figures impress me as bizarre rather than original. Considering that the draftsman plans to work professionally their range is tremendously limited. His fertility is all along one line. Some of the designs are interesting but they produce no genuine esthetic satisfaction.

Sincerely yours,  
Kroeber

[306]

Geological Survey  
Ottawa, March 18, 1920

Dear Kroeber,

I have written Paul a number of times about a copy of Boas' "Bella Coola Mythology" and a Corona typewriter, both of which he borrowed a long time ago, and has not yet returned. I shall be needing these for my coming field trip in British Columbia, and should therefore be gratefully obliged to you if you could persuade him to return them at an early opportunity. If he refuses to take notice again, I shall probably have to ask the Directing Geologist to communicate with the University of California. I would suggest that both the book and the typewriter be returned to Ottawa within a month from date.

Yours sincerely,  
E. Sapir.

P.S. I have prepared a revised list of Yana verb stems and shall get the rest of the Yana stuff ready before long. Am not planning to add much to what I gave you before. You'll get the whole MS in finished shape for press,

typewritten.--A Haida Indian,<sup>1</sup> as well educated as a college professor, dropped in here with Teit<sup>2</sup> yesterday. I am planning to teach him to write Haida for later text purposes, and to look into his lingo a bit myself. I had him talk Haida a little. It sounded very strange and rather pleasant, extremely musical, almost like Chinese, a trifle Athabaskanish. I wouldn't be surprised if pitch turned out to be of importance, but I mustn't assume it too soon.<sup>3</sup> I'm also itching to hear Chilcotin at Bella Coola this summer. I'll be disappointed if I don't demonstrate pitch throughout Nadene!<sup>4</sup>

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- 1 Peter R. Kelley. A Skidegate Haida, he was a member of an Indian deputation to the Canadian government. Sapir later noted that he was "engaged in missionary work among the Indians at Nanaimo, Vancouver Island" (1923c:143).
  - 2 James A. Teit (1864-1922), a Scotsman who settled in the interior of British Columbia near a village of the Thompson Indians and subsequently became an authority on the Interior Salish and neighboring groups. He often worked with Boas. In his later years Teit acted as Secretary of the Confederation of British Columbia Indian Tribes, and was in Ottawa in this capacity.
  - 3 Sapir published his observations on Haida phonology, based on this one brief interview with Kelley, as "The Phonetics of Haida" (1923c).
  - 4 Sapir did not go to the field in 1920, remaining instead in Ottawa to finish the manuscript of his book, Language. Although he never was able to work on Chilcotin, he did push forward with Athabaskan work and spent the summer of 1922 with the Sarcee, where he discovered "a well-developed system of pitch accent" (Sapir 1922c).

[307]

University of California  
Berkeley, March 24, 1920

Dear Sapir:

I don't especially mind being dragged as a third party into a personal affair between you and Paul as in the matter of the poems. I am however reluctant to become involved in official questions as to Government property. I overcame my scruples to speak to Paul about the typewriter and he declares vehemently that it was returned years ago, in fact before he went to live at



Santa Fe. He admits having had the Bella Coola mythology and promises to send it before you leave for the field.

Sincerely yours,  
Kroeber

[308]

University of California  
Berkeley, March 24, 1920

The President of the University and the Editorial Committee have approved a plan to issue during 1921 in the series of American Archaeology and Ethnology of the University publications, a volume dedicated to the memory of Mrs. Hearst, and commemorating the twentieth anniversary of the founding of the department of Anthropology in 1901. The contributors would be those who at one time or another have been associated with the anthropological work of the University of California. The plan is to give the volume coherence by limiting it to papers dealing with the anthropology of California and the immediately adjacent regions. Owing to the need of keeping the volume within compass as well as the increased feasibility of printing shorter papers on time, it is suggested that authors limit themselves to contributions not exceeding one printed signature in length, say six or seven thousand words maximum. Maps and plates would be additional, the sixteen-page limitation referring to text and text figures. Contributions should be in the hands of the undersigned by September, 1920. The usual number of author's separates will be provided.

I very much hope that you will find it possible to be one of the participants in the undertaking which it is hoped will be of scientific service as a summary or cross-section of the anthropological knowledge of California to date.

A. L. Kroeber  
Editor

[handwritten postscript]

Dear Sapir:

Could your partly written notes on the ethnology of the Yana be worked in for this or will they not bear boiling down? Perhaps you would prefer to give us one of the texts you got from Ishi.

Sincerely yours,

K.

[309]

University of California

Berkeley, April 8, 1920

Dear Sapir:

The list of Yana stems and affixes will answer admirably.<sup>1</sup> I hesitate for only one reason. If you submit the manuscript now it will undoubtedly be issued by summer or autumn. If you put it into the volume it will not be available until the fall of 1921. If you are willing to have it so, we will let it go at that. If you see your way clear to doing the list immediately and giving us a text later for the anniversary volume, so much the better.

Sincerely yours,

Kroeber

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1 Sapir's reply to Letter 308 has not been preserved. He apparently suggested that "The Fundamental Elements of Northern Yana" (see Letter 304) be his contribution to the Hearst Memorial Volume.

[310]

Geological Survey

Ottawa, April 14, 1920

Dear Kroeber,

Now that my Nootka sketch for Mrs. Parsons is off my hands<sup>1</sup> I shall be able to let you have the Yana paper<sup>2</sup> before very long. I shall be glad to prepare a carefully annotated Yana text for the anniversary volume, but would not like to say definitely just yet whether it would be one of Ishi's texts or not. I presume that a northern or central Yana text would be better than nothing.

Yours sincerely,

[Edward Sapir]

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- 1 "Sayach'apis, A Nootka Trader" (Sapir 1922d), Sapir's contribution to Elsie Clews Parsons' collection of vignettes of traditional American Indian life (1922).
  - 2 The revised MS of "The Fundamental Elements of Northern Yana" (1922b).

[311]

Geological Survey

Ottawa, May 15, 1920

Dear Kroeber,

With this I am at last sending you the Yana manuscript.<sup>1</sup> I hope that you will not find it too long for your purpose. You may be able to cut it down a little, but I think you will find that it is all meat and bone, very little fat.

As to the paper for the Hearst Memorial volume, would it not be a good idea to give annotated texts for all three dialects, that is, not complete myths for each dialect, but extracts that are long enough to give some idea of

the peculiarities of the three dialects? I think this plan would be both easier and more instructive than the complete annotation of a full Yana text.<sup>2</sup>

Yours very sincerely,

[Edward Sapir]

- 
- 1 "The Fundamental Elements of Northern Yana" (1922b).
  - 2 The paper was written as Sapir proposed, with a Northern Yana text from Betty Brown, a Central Yana text from Sam Batwi, and a Yahi text from Ishi (Sapir 1923a).

[312]

University of California

Berkeley, July 13, 1920

Dear Sapir:

Annotated pieces of text for all three Yana dialects would be exactly my idea for an appropriate contribution from you for our memorial volume.

Sincerely yours,

Kroeber

[313]

[Printed Announcement]

A. L. Kroeber Ph.D. has opened an office for the practice of psychoanalysis in the Physician's Building 516 Sutter Street San Francisco.<sup>1</sup>

Telephone Garfield 1352

Hours by appointment

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- 1 Kroeber maintained this office for two years, until some time in 1922. According to Theodora Kroeber:

He was in [this] office several days a week, or parts of days... He managed to continue his direction of the museum, to keep in touch with the department in Berkeley, and to write. It was not only a crowded life, it was a divided one. He was holding in

delicate temporary balance two careers, two ways of life, the while he considered giving up his academic career and going full-time into psychoanalytic practice. (1970:105-7)

[314]

[Berkeley]

July 14, 1920

Dear Sapir

I shall get at your poems shortly. I cannot criticise them as you write them; I must steep myself in a series of them before my reactions begin to flow; and this steeping requires a certain psychic occasion, a margin of activity, like the actual writing of a projected paper.

Why are you silent on the notice of my new undertaking?<sup>1</sup> You are the one man in the profession that I counted on not to take the event either as a slap or as a morsel of gossip.

I am wondering whether you have met Ernest Jones, or if he left Toronto before your interest in the subject grew.<sup>2</sup>

I suppose you have heard that Paul is Director of the Pacific Coast Zionist Bureau at a good salary and declares that he is through with this institution but not with anthropology. He is tapering off with a year's leave of absence, for which he asked. We are to have Waterman back--regretfully, in the face of your disapprobation. It was a hard choice for me to make--he or Lowie. For the first time in twenty years it wasn't a question of whether the candidate would do, but of selecting between two men, and both friends, both of whom I wanted.<sup>3</sup>

Paul praised Lowie's book<sup>4</sup> but was antithetically stimulated by it. Wallis thinks it poor. I feel rather like Paul. If that is all we are going to get out of excellently done ethnology, what is the use?<sup>5</sup>

I'm awfully glad to hear from Spier<sup>6</sup> that you're at work on your book for Harcourt.<sup>7</sup> If it isn't up to your best, I'll roast.

I may add that I've been analyzed and have had some clinical experience, but that I believe it is open to doubt whether this community is ripe for psychoanalysis.

Yours

Kroeber

- 
- 1 The opening of his psychoanalytic practice.
  - 2 Ernest Jones (1879-1958), distinguished British psychoanalyst and biographer of Freud. From 1908 to 1912 he was director of a psychiatric clinic in Toronto.
  - 3 T. T. Waterman, who had resigned his Associate Professorship at Berkeley in 1917 to join the army, had resumed his academic career after the war at the University of Washington. Waterman's return to Berkeley lasted only for the academic year 1920-21, and the job went to Lowie in the end after all. (See Letter 325.)
  - 4 Primitive Society (1920), intended as an "up-to-date synthesis" of social organizational theory, and based on a course Lowie had given at Berkeley in 1917-18.
  - 5 Kroeber reviewed Lowie's book for the American Anthropologist (Kroeber 1920b), calling it "a clear and fair representative of what modern ethnology has to offer", but cautioning that "as long as we... consistently show a negativistic attitude toward broader conclusions, the world will find very little profit in ethnology".
  - 6 Leslie Spier (see Letter 261). Spier, who had just received his Ph.D. from Columbia, was about to take Waterman's place in Seattle.
  - 7 Language (Sapir 1921c).

[315]

S. S. Maui, At Sea

July 29, 1920

Dear Sapir

I'm off to attend the Honolulu conference,<sup>1</sup> with some misgivings and regrets at the three weeks, but also anticipation of experience. We're only twenty four hours out and the sea already looks, and the air is, subtropical.

Yes, it is an amateurish, rough and ready continent, but so are others: witness Paderewski and Masaryk, to go no higher. We merely leave every door open, Europe only those that lead to the chambers of most responsibility. And how about medicine? If ever you have touched it behind the scenes you know how utterly rule of thumb it operates, beyond the reach of X-rays and a little bacteriology. No, not rule of thumb, but trial and error. Does plugging two years over anatomy give a medical student any more insight into conditions of mind than I have into anatomy? And at that I don't begin without a certification of lack of anatomical complications, whereas he blithely handles mental symptoms and mental causations. I don't know much, but I know something of how to learn; and mean to learn, which is just about where I stood when I broke into anthropology. And I honestly think I know as much of the probable mechanisms of an ordinary neurosis as does the average physician, which is damned little for both of us.

I don't plead any preponderating objective scientific motive, of course. I'm not so rotten an analyst as that. But I do want a larger "pediment," a leverage to prevent me rutting ever deeper into my little job and, if I can pick it up, any legitimately earned money. Is it any different, except for the last factor perhaps, from your writing poetry? It's a healthy instinct that leads me to try to break the bars that tend to close in on us with the momentum of years; healthy, if the effort is not spasmodically hysterical.

I think you put too much trust in Paul's statements about his conditions of mind. In April he was happy and no more obsessed with the idea of escaping to Europe--as an actuality--than I am letting the notion of \$25,000 a year or Presidency of the National Academy worry me. We'd both like these respective things, but when Paul says that he has wanted, that is, has made the least serious effort, to establish himself in Europe, a year or even three months ago, he is fooling himself. His desire to leave dates from the day on which he found himself under fire: in short, when there was disagreeableness ahead.

As to Waterman, you made a shrewd guess, only it's inside out. If there is one man in anthropology who makes me feel that he invades my personality, against whom I have to summon my reserves, it's Waterman. When he left us, my regret was mingled with a sense of relief at cessation of the strain of

maintaining myself. Well, I wasn't ready to avow to myself that I was keeping Waterman away because I dreaded having him around. That's why he'll be back. It's strange how heavily you weight purely intellectual factors against personal ones. I could have Lowie outshine me all over Berkeley without being much exercised.

I wasn't hypocriting but teasing when I expressed regret at your disapprobation. It's a childish habit I have retained.

Don't ask me to review your book. I did my share in shoving the publisher after you. And, when you see what I'm saying about Lowie,<sup>2</sup> you'll be wise and not ask me to do the same to you. It's very different reviewing monographs: one sets only an everyday standard and most of them measure up quite creditably.

How do you like the last part of Paul's Mexican paper?<sup>3</sup> I think it's mighty good, but I expect our crowd won't see the quality and the historians as usual ignore the history in it.

Kroeber

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- 1 The Pan-Pacific Scientific Congress, held in Honolulu under the sponsorship of the National Research Council, August 2-20, 1920. The aim of the Congress was to draw up plans for coordinated scientific research in the Pacific. A sectional committee on Anthropology formulated a plan for research in Polynesia and other Pacific islands. Eleven anthropologists attended, six from the United States (including Wissler and Tozzer, in addition to Kroeber), three from Hawaii, and one each from Australia and New Zealand. Kroeber delivered a paper on "Peoples of the Philippines". See "Anthropology in the Pan-Pacific Scientific Congress", AA 22:392-3, 1920.
  - 2 His review of Lowie's Primitive Society (Kroeber 1920b). See Letter 314.
  - 3 "The Sources and Authenticity of the History of the Ancient Mexicans" (Radin 1920). Section IV (pp. 132-150) is an outline of Toltec and Aztec history, insofar as this can be reconstructed from surviving documents.



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Ottawa, Ont.

Oct. 4, 1920.

Dear Kroeber,

I hope you enjoyed your trip to Honolulu. I learn from Goddard, whom I saw recently in New York, that you have written out some impressions for the *Anthropologist*.<sup>1</sup> That ought to make good reading. How was the conference? Wissler sent me the preliminary report or recommendations and I have written him at some length regarding the linguistic part of it. I presume you wrote that. It looked very good to me, but I think the larger Malayo-Polynesian or even Schmidt's Austronesian problem should have been stressed a bit more. Polynesian itself is fairly small potatoes, I imagine. Recently, in connection with my book (which, of course, you are not to review! how did you foresee it was to fall short of Lowie's recent very successful effort?) I have looked carefully through Maspéro's *Khmer*<sup>2</sup> and dipped into Seidenadel's *Bontoc Igorot*.<sup>3</sup> Upon my word, there are some remarkable analogies. What do you think of two such features as this?: 1. Infix -m- for agentive nouns is found in both; in general infixation is remarkably well developed in Mon-Khmer, Indonesian, and *Muṇḍā* (where reciprocal -pa- is infixed, just as is reciprocal -r- in Khmer). 2. Causative prefix pa-, paŋ- in both. My feeling is decidedly that Schmidt is right.<sup>4</sup> From recent correspondence with Boas, I gather that he is still very ponderously concerned with mutual and extensive borrowing of morphological features as a very possible substitute for genetic explanations. I believe he still doubts (or pretends to doubt) the validity of the Na-dene construction. And all the time his own *Tlingit* book<sup>5</sup> is the finest corroboration of my article one could have wished. In effect he out-Na-dene's me in this book to a frazzle, but he won't see he's done it! Now, you psychoanalyst, where's the complex?

I have had a considerable recrudescence of interest in linguistics recently, but feel I want a really big problem on my hands. As soon as my little book is finished, I am planning to make a really exhaustive questionnaire on morphological and phonetic features for languages in Mexico and N. America and

to fill in for each group. I want to see what are the distributions of such features as use of syntactic cases, classification of verbs into active and static, use of diminutive -tsi or -si, and so on through the whole gamut; then correlate as far as possible, and see what happens. Finally, apply lexical tests to resulting groups. I feel certain we can get somewhere if we seriously tackle the problem. I think I am particularly qualified to do this because I have a better feeling for perspective, a clearer intimation of survival of old vs. development of secondary features than most others. Paul, for instance, cannot do this work really convincingly because he does not know how to evaluate; everything is fish for his net. My present feeling about N. American languages is something like this. We have 6 great groups, which I should like to hold distinct for the present, though even now I recognize certain promising "proto-American" features (such as negative \*ka, \*ku; diminutive \*-tsi; 1st per. sing. n-; plural and frequentative -l). They are:

- A. Eskimo-Aleut (includes also Chukchi-Koryak ?)
- B. Na-dene (Athabaskan, Haida, Tlingit--Haida stands off most)
- C. Algonkin-Wakashan:
  - I. Algonkin; Wiyot-Yurok
  - II. Kootenay
  - III. Wakashan; Salish; Chemakum
- D. Penutian:
  - I. California Penutian
  - II. Takelma; Coos-Siuslaw-Alsea; Kalapuya
  - III. Chinook
  - IV. Tsimshian
- E. Uto-Aztekan; Tewa-Kiowa (?)
- F. Hokan-Siuoan:
  - I. Hokan proper (Shasta down to Seri and Chontal)
  - II. Yuki
  - III. Coahuiltecan group (Coahuilteco, Tonkawa, Karankawa)
  - IV. Keres (decidedly, judging from what Boas writes me)

## V. Siouan-Yuchi group:

1. Siouan
2. Muskogi-Natchez
3. Yuchi
4. Tunica-Chitimacha-Attakapa

## VI. Iroquois; Caddoan

I know nothing of Zuñi. Modoc-Molale-Sahaptin is possibly transitional between D and E or is an outlier of E. Of Mexico I know very little, but imagine Maya may belong to F. Radin has shown that Huave belongs to Zoque-Mixe,<sup>6</sup> while his MS material proves absolutely that Mixtec-Zapotec goes with Otomi; Tarascan possibly another outlier of Uto-Aztekan. Of course all this is exceedingly tentative, but I feel much of it will stand. If I were to commit myself still further, I would suggest that C is a highly specialized polysynthetic offshoot of D; and that E is possibly a Mischsprache formed of D and F. B stands most aloof of all (aside, possibly, from Eskimo, though I feel Eskimo closer to Algonkin-Wakashan than Na-dene to any other group, despite Paul to the contrary). The test languages are, say, Eskimo, Tlingit, Wintun, Chimariko (or Iroquois). Master these 4 (or their equivalents) and you have an American linguistic vade mecum. Here's an interesting point: the polysynthetic tendency manifests itself at various points quite regardless of genetic relationship. I mean it is not a highly valuable criterion genetically, but expresses rather a certain extreme tendency to synthetic expression, however we explain it. Its interest is psychological rather than historical. To me it is worth less than such an obscure feature as prevalence of stems with initial vowel (à la Chimariko-Salinan-Keres-Pomo-Shasta- Iroquois-Muskogi) or classification of pronouns into transitive and intransitive vs. active and static (D is characterized by the former, F by the latter). Least polysynthetic is D, which is nearest of all to our inflective type, but even here a relatively late polysynthesis (or perhaps better incorporative development) grew up in Chinook, which seems to me to have taken a new synthetic spurt from a very analytic form that had broken down from a Takelma-Coos type. Further, polysynthesis in A and C grew up around an old inflective core; polysynthesis in B developed from a remarkably analytic non-inflecting (one might almost say isolating)

base--yes indeed! Déné complexities notwithstanding (nothing seems more certain to me; a Tlingit, Haida, or Déné synthetic form absolutely fails to monosyllabic fragments of considerable individual phonetic and functional independence once you begin to analyze, and yet Na-dene polysynthesis has in some ways won through to an inflective system of its own--most fascinating of all languages ever invented! no wonder Goddard can't budge); and polysynthesis in F is clearly of agglutinative type. The essentially agglutinative feel of Hokan is once more borne in on me as I work on Yana analysis for your Hearst volume<sup>7</sup>--as different psychically from Takelma as sun from moon. Wouldn't this threefold development of polysynthesis make a neat bit of linguistic theory?

But I am just now interested in another big linguistic possibility. I tremble to speak of it, though I've carried the germinal idea with me for years. I do not feel that Na-dene belongs to the other American languages. I feel it as a great intrusive band that has perhaps ruptured an old Eskimo-Wakashan-Algonkin continuity. And I decidedly feel the old quasi-isolating base. Then there is tone, which feels old (high and low)--I am all but certain that Athabaskan and Haida are like Tlingit as to tone. In short, do not think me an ass if I am seriously entertaining the notion of an old Indo-Chinese offshoot into N.W. America.<sup>8</sup> I am planning to work in Indo-Chinese to some extent. I have already carefully gone over two Tibetan grammars (Jäschke and Foucaux)<sup>9</sup> and find in Tibetan pretty much the kind of base from which a generalized Na-dene could have developed, also some very tempting material points of resemblance, e.g. Tibetan postpositive ma "in" and du "to, at", both of which, precisely as in Athabaskan and Tlingit, are used also to subordinate verbs; in both Tlingit and Tibetan the tr. verb as such is clearly passive; causative or tr. verbs have s- prefixed in Tibetan, si- and si- in Tlingit, ḥ- in Ath.; Tibetan verb ablaut is staggeringly like Déné-Tlingit (e.g. present byed "make", pret. byas, fut. bya, imperative byos); and so on. Am I dreaming? At least I know that Déné's a long shot nearer to Tibetan than to Siouan. Such things as instrumental prefixes, that Paul made so much of,<sup>10</sup> don't get us anywhere, as Na-dene prefixes of this type are simply late compositions and don't even agree among themselves (practically any noun might

have become an instrumental prefix; as far as that is concerned, Chinese may say things like fire kill "to kill with fire", I believe). I'm rather puzzled about it all. I shall have to work out the whole Indo-Chinese data to get my bearings.

I wish you every possible good luck in your psychoanalysis. I met Ogburn<sup>11</sup> and Corey in N.Y. They're full of it. I'm now reading Bjerre's horribly translated book.<sup>12</sup> Can you recommend something good in that line? I am sincerely interested and want to hear of your progress in your new field. I have no doubt you will make a brilliant success of it, though still a little gaping at your courage. I may as well confess that I have some notion of a certain limitation in your temperament that may impede you in your practice, but I am just as likely as not to be all wrong.--Can you not send me the poems? It all seems a bit selfish of you after the immediate and full attention I gave your stories, does it not? Damn all you like, but do something. Or return them anyway.--I was sorry to learn Paul has again miscarried. Unfortunately I can't get him field work here, tho I tried as well as I could. He is now threatening to write poetry for a living! Come, death, sweet death!

Sincerely,

E. Sapir.

P.S. What's the trouble with Goddard, anyway? He looked rather dumpy and sentimental-Jesuitical to me. I'm afraid he didn't like to have me break in on those lovely Wailaki place names.<sup>13</sup> He tried for a while to communicate his emotions about them, but as I have a stack of Nootka placenames of my own, I failed to enthuse as I should have.

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- 1 "Observations on the Anthropology of Hawaii" (Kroeber 1921a).
  - 2 Georges Maspéro, Grammaire de la langue Khmère (Cambodgien) (1915).
  - 3 Carl W. Seideinadel, The First Grammar of the Language Spoken by the Bontoc Igorot... (1909).
  - 4 Father Wilhelm Schmidt (1868-1954), founder of the Anthropos-Institut and the journal Anthropos, had proposed an "Austriac" linguistic stock, reaching from Australia and the Pacific across Asia to Europe. A link between Mon-Khmer and Malayo-Polynesian was an important part of his argument. (Schmidt 1906).
  - 5 Boas 1917.

- 6 Radin 1919b.
- 7 "Text Analyses of Three Yana Dialects" (Sapir 1923a). See Letters 311-312.
- 8 For an extended exposition of this possibility, see Letter 332.
- 9 Heinrich A. Jäschke, Tibetan Grammar (2nd edition 1883, or perhaps the earlier edition); Philippe Edouard Foucaux, Grammaire de la langue tibétaine (1858).
- 10 In Radin 1919a.
- 11 William F. Ogburn (1886-1959), at this time Professor of Sociology at Columbia. In 1927 Ogburn moved to the University of Chicago, where he and Sapir were colleagues for a brief time in the Department of Sociology and Anthropology.
- 12 Poul Bjerre, The History and Practice of Psychoanalysis (1916), translated from Swedish.
- 13 Goddard's contribution to the Hearst Memorial Volume (see Letter 308) was an ethnogeographic study of the Wailaki, a California Athabaskan group (Goddard 1923). He apparently discussed this paper with Sapir during the latter's visit to New York in September, 1920.

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Geological Survey

Ottawa, October 15, 1920

Dear Kroeber,

Could you have Mr. D. Jenness,<sup>1</sup> who has just been made permanent Associate Ethnologist of the Geological Survey of Canada, put on your anthropological mailing list? He is an A number one anthropologist and should be given all the literature that we can spare. Any back numbers that you can send him would be greatly appreciated.

Yours sincerely,

E. Sapir

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1 Diamond Jenness (1886-1969).--Trained at Oxford, Jenness came to Canada in 1913 to join Beuchat in the Canadian Arctic Expedition, 1913-1916. With this appointment as Associate Ethnologist, he became Sapir's deputy, and he succeeded Sapir in 1925 as Chief Ethnologist.

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University of California  
Berkeley, November 3, 1920

Dear Sapir:

Wissler has sent me a copy of your comments on the linguistic section of the Research Council recommendations for scientific research in Polynesia. I drafted this section and if I am not mistaken it went in at least substantially as I wrote it. I am sending you herewith a copy of what I am now writing Wissler on the subject.

What I specially had in view was the sort of thing which the Bishop Museum expeditions were likely to do that should be guarded against. If Churchill<sup>1</sup> had remained in good health instead of dying, it is likely that Gregory<sup>2</sup> would have put him in charge of the whole works. While I was in Honolulu he was planning to have Stokes and Aitken<sup>3</sup> "do" the Austral languages alongside of culture and physical anthropology.

In view of these matters properly belonging to the Anthropology-Psychology division of the Research Council, will you consider them confidential?

Sincerely yours,  
Kroeber

November 3, 1920

Dr. Clark Wissler,  
National Research Council,  
1701 Massachusetts Avenue,  
Washington, D. C.

Dear Wissler:

Sapir is quite right in his comment on the recommendations on linguistic research in Polynesia. I was writing with actual conditions at the Bishop Museum in view--trying to guard against some novice or enthusiast coming in and trying to do over the valuable work which men like Tregear<sup>4</sup> and many others have done for a century past. A broad-minded philologist would not want more field work than to get the living "feel" of the languages which Sapir insists on. This, once he were on the ground, there would be ample opportunity to secure.

I also agree thoroughly with Sapir's second point that a restriction to the Polynesian languages as such would be unfortunate. The real problem is the broad one as he outlines it. Here again the calibre of the man who is entrusted with the work must be decisive. To limit a man like Sapir rigidly to Polynesian would be criminal. On the other hand to turn a smaller gauge man loose on the whole of Oceania and Asia would probably lead to his never accomplishing anything orderly, and if he did attempt a synthesis it would be unsound.

If you wish this part of our report to be as general as possible in its bearings rather than to have immediate reference to the concrete work under way or likely to be undertaken in the near future through agencies already operating, I think you would do well to revise this section of the report in line with the spirit of the foregoing paragraphs.

Sincerely yours,

(Signed) A. L. Kroeber

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- 1 William Churchill (1859-1920), U.S. Foreign Service officer and student of Polynesian linguistics and ethnology.
  - 2 H. E. Gregory, Director of the Bernice P. Bishop Museum, Honolulu.
  - 3 J. F. G. Stokes and R. T. Aitkens.
  - 4 Edward Tregear (1846-1931), known for his pioneering work on the Maori and on Polynesian comparative linguistics.

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Ottawa, Ont.

Nov. 30, 1920.

Dear Kroeber,

Thanks for the return of the 6 remaining poems and for your comments. I have been giving verse a rest for some time now. This should do it no harm. It will either die a natural death or take on a little quality in its silent vacation.

Enclosed is a statement I am going to present at the Section H meeting in Chicago.<sup>1</sup> (I prefer to go there because I want to consult with Laufer on Indo-Chinese. Am now in the thick of Indo-Chinese philological treatises. Fascinating field.) Do drop me criticisms or suggestions, so that I may profit



by them for the Xmas meeting. Of course the scheme is extremely temporary, serving chiefly to give outward shape to my present feeling in the matter. It is sure to be greatly modified in details, probably also on some fundamentals. Have you any hint to offer on Zúñi? If not, give me a doz. or so very brief points on Zúñi morphology, so I may be able to offer a suggestion as to where it belongs.

I wrote Goddard recently I was working on Nadene again and felt I had to get a first-hand inkling of Athabaskan in the field, that I would be likely to run in on Sarcee. Tried to make it clear my objective was Na-dene (and beyond!), only incidentally Athabaskan (as necessary stepping-stone). He did not answer as graciously as I thought he might--said if I did what he had been planning to do 20 yrs. (comparative study of Athabaskan), he would consider I had treated him unfairly, though he would not tell me so (isn't this noble?). He might have added that he would die unhappy, with Hupa graven on his heart. Of course I wrote him then a most conciliatory letter--a long one--, in which I tried to make him see that if I suspected, for instance, there was tone in Athabaskan analogous to Tlingit and my objective was Na-dene, of course I simply had to find out and follow my problems where they led me. Made it clear I was not after text material as such, offered to turn over all my Athabaskan comparative data (etymological dictionary rapidly growing) to him for his use or our collaboration or anything he chose, and even offered to let him see any Athabaskan MS I might ever prepare before publishing it--in short, played completely into his hands for the sake of peace and good will. Result--no answer. Why I should feel particularly generous to Goddard I don't know. I owe him nothing but nasty cold water he has stolen from Boas' reservoir. Now tell me. You know his psychology better than I do. Is it best to ignore him politely or to actively try to communicate with him? I am willing to make some sacrifices but not to consider I have no moral right to do Athabaskan. The real fact is, Kroeber, Goddard's work is not good--decidedly not. It is not in the same class as Petitot,<sup>2</sup> Legoff,<sup>3</sup> or Franciscan Fathers.<sup>4</sup> Least of all in phonology. He is not really competent to write a thoroughly satisfactory

comparative study of Athabaskan. Perhaps he resents my fereting out dark closets! Anyhow, give me your think. I am prepared to be reasonable.

Sincerely,

E. Sapir.

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- 1 The 73rd annual meeting of the American Association for the Advancement of Science, Chicago, December 27, 1920 - January 1, 1921. Section H (Anthropology) of the A.A.A.S. had been organized the previous year and it convened for the first time at this meeting.--For the statement (i.e., abstract) and other materials relating to Sapir's paper, "A Bird's-eye View of American Languages North of Mexico", see Appendix II. See also Letter 324.
  - 2 Father Émile Petitot (1838-1917), a pioneer missionary priest in the Northwest Territories (1862-1882). His major work, Dictionnaire de la langue Dènè-Dindjié (1876) is a synoptic study of Chipewyan ('Montagnais'), Hare ('Peau-de-lièvre'), and Kutchin ('Loucheux').
  - 3 Father Laurent Legoff, French-Canadian missionary priest who worked on Chipewyan ('Montagnais') (Legoff 1889).
  - 4 The Franciscan missionaries at St. Michael's, Arizona, especially Father Berard Haile. Their Ethnologic Dictionary of the Navaho Language (1910) had been reviewed by Goddard (1910).

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Geological Survey

Ottawa, December 22, 1920

Dear Kroeber,

Under registered cover I am sending you the text analyses of the three Yana dialects.<sup>1</sup> I have made somewhat more of a paper out of it than I originally intended. I imagine that you will have reason to be satisfied with the Ishi part. It is really in places something of a tour de force. An isolated N. Yana form would sometimes help me out wonderfully.

Yours sincerely,

E. Sapir.

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1 Sapir 1923a.

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University of California  
Berkeley, December 27, 1920

Dear Sapir:

It has been lack of time and not of interest that has kept me so long from answering your letters about American languages. To begin with I want to say that so far as I can judge with my limited equipment you seem to me to be wholly on the right track, and that I am confident that at least the vast majority of your findings are true. When it comes to languages like Iroquoian and Caddoan I am hopelessly beyond my depth. Yet the feeble impressions I have are all in the direction of lining these languages up with the Hokan-Siouan group. I have the same feeling in regard to Maya, in fact have thought that were I to take two or three days off for the purpose I could probably get enough evidence to convince at least myself. If I had anything of your facility of absorption I should long ago have been your competitor in this field, making up for your finer discrimination by greater rashness, or at least willingness to be content with slenderer evidence.

Paul's method of leveling everything is essentially repugnant to me.<sup>1</sup> I rather favored his paper for the effect it would have in indicating the limits toward which we were tending, and emphasizing the fact that the problem is deepening. The real interest in the matter however obviously lies not in the fact whether we have one or six or fifty families in America, but how these languages grew out of their origins and what the causes of the changes have been. These real problems Paul, it seems to me, has wished to discard. The way in which he took the first language he happened to strike here, the particularly resistive Yuki, and promptly linked it with two such antithetical types as Hokan and Penutian, and then went on to tie up Shoshonean with Siouan, grates on me. Granted that there was a single proto-American language, this has unquestionably evolved in branches of quite different direction. But to blot out the history of these branches on account of an interest in the trunk, is as fanatic as Boas's and Goddard's refusal to have anything to do with the trunk even if it exists.<sup>2</sup> I feel more and more that Paul's actuating motive

in this case was less an intrinsic interest in the subject matter than a desire to revolt. He was fighting the established system of classification less because it was antiquated than because it was established. It was part of an anthropological world which had been cruel to him, and he was sideswiping in revenge.

I am glad you have retained Eskimo and Na-Dene as distinct. I should have had less faith in your findings had you united them with anything American. As to the Asiatic affiliations of Na-Dene, I am too ignorant to have an opinion, although I do feel strongly the ultimately monosyllabic and essentially isolating character of Athabaskan. The very fact that the polysyllabic words are really crystallizations suggests a more isolating original quality than for instance the fluid compounds of Hokan.

Algonkin also impresses me as very distinctive. Somehow the resemblances to Eskimo which you and Paul have mentioned before appeal to me as essentially superficial. Yet this may be mere habit conservatism.

The most radically new group is Hokan-Siouan. It absorbs the largest number of old stocks and seems to have been the group which the older students were unconsciously thinking of when they attempted formulations for "the American languages" in general.<sup>3</sup>

I share your feelings about Uto-Aztekan not possessing the same degree of distinctiveness as most of the others. I cannot quite feel that it is a blend, and therefore expect to see it link up with one of the others.

As to the unplaced languages, I have no hesitation in saying that Zúñi goes into Hokan-Siouan. My first intuition in 1915 was that it was Siouan, but there was little evidence to be got in comparing it with any specific Siouan language, and I was not ready to undertake a systematic survey, so I held my peace. I will try to send you soon a list of elements and some notes. You will be able to do more with them than I, and I rather expect you will put it into the same group.

Beothuk material is of course miserable, but I got the impression in glancing over it years ago that it was unquestionably Algonkin. I imagine these people were an early branch of Algonkins who got isolated on the island, and by the time the later wave joined them there had diverged considerably.

As to the three Oregon languages, I hope you can place the Sahaptin as it leaves a good sized hole in the map. As to Klamath I should not be surprised at Penutian affiliation. I suppose your hesitation is due to the same cause as mine: the bulky chaos which underlies the superficial order of Gatschet's presentation. It is not without significance that the fullest presentation which we have of an American language is also the least usable.<sup>4</sup>

Your ideas on polysynthesis as of secondary or mainly psychological interest appeals to me very strongly. There is not a point which you mention in this connection that I should dissent from. I think you should put these ideas on record in print without waiting until you are able to substantiate them in detail, which will be many years. The majority of us won't know what you are talking about, and Boas will think you have turned prophet instead of philologist, but a pronouncement would at least show which way we were tending and emphasize the nature of the problems that lie ahead. Boas's purely analytic and descriptive work has of course been badly needed, and is still; but it is also time that we proved we were not evading the historical problems. I do not believe we need fear any excessive swing of the pendulum toward speculation. Boas's influence in the direction of a soundly critical attitude will remain with us permanently.

I also hope you will really put through a distributional survey of structural features. I am sure this will prove a meeting ground for us all. Some years ago Boas urged me to undertake such a review. For a brief time I even revived a plan I had formed fifteen years ago. However I shall never more than dabble in linguistics and you are clearly the indicated man for the job. I am very glad you have found it for yourself. Boas's point was that we should probably find many structural features which had been transmitted like culture elements between distinct stocks. I am inclined to think he is probably over-sanguine on this point. But were the result to prove what he or what you and I expect, it would be all to the good.

As to Goddard, I would suggest leaving him alone and going ahead. He is in a difficult position. He definitely appreciates your work on Athabascan, as well as on other groups, but cannot do it himself, and at the same time cannot formally abandon his claim which is the only one he ever staked. He is over

fifty, and so far as I can see he is not essentially nearer to a really comparative Athabaskan volume than he was twenty years ago. If I were you I should go my own way, merely treating him with as much consideration as possible, and I think you will leave no serious scars. Goddard has an unusual facility for seeing and accepting the inevitable. His resentments and fights serve temporary gratifications. When he realizes that no unkindness or slight is intended he loses his animus. Making proposals to him now, or offering concessions which really involve renunciations by him, will only stir him up.

Sincerely yours,

Kroeber

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- 1 In Radin 1919a.
  - 2 From the first, Boas had privately expressed his dissatisfaction with the sweeping genetic syntheses being proposed by Dixon and Kroeber, Sapir, and others. He finally made his views public in a paper on "The Classification of American Languages", which appeared in the Oct.-Dec., 1920, number of the American Anthropologist. It was Boas's opinion that "the whole theory of an 'Ursprache' for every group of modern languages must be held in abeyance until we can prove that these languages... have not originated, to a large extent, by the processes of acculturation" (1920:374-5). He felt, for instance, that the similarities among Athabaskan, Tlingit, and Haida could be better explained by the "spread of morphological traits from one language to another" (373) than by Sapir's "Na-Dene". Goddard expressed similar views, most notably in another attack on Sapir's Na-Dene hypothesis (Goddard 1920). See also Letter 332 (Postscript).
  - 3 As in, for example, Brinton's proposal (1894a,b) that "polysynthesis" is a fundamental, identifying trait of all American Indian languages.
  - 4 See Letter 98.

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University of California  
Berkeley, January 7, 1921

Dear Sapir:

I have your manuscript of the three Yana texts with analyses. I am very much pleased that you have been able to put this paper through. Together with

the list of elements and the original volume of texts it will put the knowledge of Yana on a substantial status beyond the reach of accidents.

Our budget difficulties have finally hit our publications. There had been talk of this so long that I had come to believe that the lightning would never strike. It seems that for the next six months no work is likely to be done at the Press. I have every expectation that when the new budget becomes effective July 1, printing will be resumed with increased vigor. I think what I shall do will be to file your present manuscript now so as to give it priority when things move again, and then when our memorial volume is actually taken up next summer, decide whether it will be better to switch this paper or the Elements of Northern Yana from the general series into the Memorial volume. I suppose you will have no serious preference. If the memorial volume comes out full I should prefer to put the Elements into it. If it is slim your present longer manuscript will help. Some months ago it looked as if the volume would turn out rather meagre. Of late contributions have been coming in until now I feel more sanguine about the enterprise having been worthwhile.

Sincerely yours,

Kroeber

[323]

[Berkeley]

January 17, 1921

Dear Sapir

There is no doubt you should leave Ottawa.<sup>1</sup> The means of getting away is likely to be less certain. There aren't many Universities with a professorship of linguistics. I doubt whether we shall ever have an opening for you. Personally I doubt whether Waterman will leave us, at least permanently. If he does, we shall need a man to do straight anthropology. We are barely giving enough upper division and graduate work now. I have been hoping that the pressure of students would soon force the University to add a third man. But this semester shows a decrease, when January has usually exceeded August

in enrolment as three is to two. During the congestion of the past twelve-month we have evidently absorbed every student in the place who was at all ready to consider taking the subject, and now we are travelling on hangovers and new entrants. So there is no expansion in sight. If Waterman did go and you took his place the change would be self-defeating. One must paint with a broad brush to 700 ignorances, and you work with etcher's tools. You could learn to swab: but it would come hard and take a lot out of you. It is easier for Lowie and me; and twenty years have toughened my hide and slowly taught me some compensations.

Wheeler has just begun to teach linguistics: "Human Speech."<sup>2</sup> It will no doubt keep it elementary, and has drawn well. He may tire of it in a few years and leave some consciousness of a gap.

You are wholly right about getting your work out of the anthropological classification. Most of us don't dream of professing anything but abhorrence for languages, and the bulk of the minority are satisfied to claw together raw material. As for a theoretical interest, I may be overlooking someone, but when you have named Sapir, Boas, Radin, Kroeber, and Harrington, I think you are through. Harrington is too wholly under the sway of an obsession ever to do more than collect; Paul we've discussed to sufficiency; and my hand is too clumsy and perception too slow for me ever to convert interest into the sort of production that counts. So the sooner you shake off the anthropology stamp, the better.

At the same time, who is there in this country among the philologists that has enough breadth to make a move in your behalf? They'll admit your training with them qualifies you, they'll approve of what you do; but will they care about it? Philologists that I have known are a damned inert lot--excepting the old boy at Johns Hopkins whose name slips me.<sup>3</sup> I think your outlet is in books--books on language. You're doing a little popular one. Follow it with a heavy one on principles, illustrating equally with I.E. and Asiatic-American material. That will both humor the orthodox and impress them. With a certain reputation under your belt, the job will make itself: some institution will be alert enough to see you as a find.



In short, work your connections, but don't count on them. Bank on what you can carve out for yourself. You can do it. That you haven't, proves only that you haven't wanted to badly enough to fix every effort on the aim.

I haven't much to tell about psychoanalysis. Private practice remains almost nominal. My friends of course, keep away. The medical men with whom I have come in touch give me more recognition than I expected but of course don't send patients. The educated public is still much more suspicious of the subject than in the East, and doesn't know of me to any extent. And the uneducated want healing, spiritualism, direct stimulation by insistence, suggestions how to be successful. It will take time--possibly more time than I have. This has always struck me as significant: my first and to date best patient was a New Yorker.

The clinical work is fascinating, in spite of its difficulty, and I get as much of it as I can handle. There isn't much to be hoped for in the way of cures, as you might imagine, though I have one definite improvement of a long-standing hysteria and am hopeful of a couple of other cases. I get variety of experience: epilepsies and hypo-psychoses in with neuroses. They all illuminate mechanism and technique.

My impression is growing that I began too conservatively and cautiously. Freud's theorizing tricks irritate me as intensely as ever, but he certainly is an astounding observer. He has a preternatural eye for significant minutiae. On any concrete point I believe he is almost infallible. Brill, by the way, has a little of that; in spite of his fast orthodoxy.<sup>4</sup> Ernest Jones is the best writer in English. Freud's greatest book I hold to be his little Three Contributions to Sexual Theory;<sup>5</sup> though his late General Introduction<sup>6</sup> is much profounder than it seems. I rather liked Bjerre.<sup>7</sup> He keeps his head better than most of them.

On the psychological side as distinct from the medical, I think symbolism interests me most. It really seems to be more nearly "fixed" than any critical mind would at first credit. I am getting some nice cases from patients whom I carefully keep ignorant, so as to preclude the possibility of suggestion. One can't of course disguise to them the presence of the sexual factor and be sincere in treatment, but I try to pile up long series of dreams before I give any

interpretation. It's fascinating to watch the symbolism grow bolder and clearer each week.

The phylogenetic [illegible] however, I mean to have a crack at before long.

We need to know much more about day dreams; but they are very hard to get.

I expect to be here all summer. You'll get something on Zuñi. I'm slow but I deliver.

Kroeber

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- 1 This personal letter is evidently a reply to a letter from Sapir (not preserved) in which he expressed to Kroeber his frustration with the intellectual isolation of Ottawa and inquired about prospects at Berkeley.
  - 2 Benjamin Ide Wheeler retired from the presidency of the University of California in July, 1919, and returned to teaching. Before coming to California in 1899, Wheeler had been Professor of Comparative Philology at Cornell.
  - 3 Probably Hermann Collitz (1855-1935) is meant.
  - 4 A. A. Brill, Freud's chief disciple in America.
  - 5 Freud 1920a.
  - 6 A General Introduction to Psychoanalysis (Freud 1920b).
  - 7 See Letter 316.

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University of California  
Berkeley, January 20, 1921

Dear Sapir:

I know how you feel about exposed flanks.<sup>1</sup> Nevertheless if there is any danger you have already incurred it making your stand public at Chicago. I consider the wisest course therefore to be to put yourself on record and save yourself from misrepresentation. There may not be any great amount of talk about your Chicago presentation but there is no telling how far it may travel and it is almost certain to be distorted. You can count on the people who are

interested in the subject picking up some sort of gossip about it. You will be better protected if their knowledge of your attitude is authoritative.

I do not see how publication would lay you open to attack. Since you present no evidence you cannot be charged with misusing it or employing it hastily. Issue may be taken with your making the basis of relationship morphological rather than lexical. But people who take this stand would shrug their shoulders twice as high if you supported your attitude with a book of a thousand pages.

To make a confession, I gasped a bit when I realized that you had really presented this outline at Chicago. I was astounded not because your stand seems to me essentially an exposed one but because I have always been strongly impressed by your cautiousness. I conclude that you must have a lot of ammunition salted away or at least that you possess complete assurance that you can produce it in unlimited quantities when the call comes.

I return your outline. I hope that when you publish that you will include the map on however reduced a scale.<sup>2</sup>

Sincerely yours,

Kroeber

- 
- 1 Kroeber, here again, is replying to a letter from Sapir that has not been preserved. Sapir had apparently sent Kroeber a copy of the notes that he had used in delivering his paper on North American linguistic classification at the A.A.A.S. meeting in Chicago (see Appendix II), asking for Kroeber's advice on whether or not he should publish the paper. Sapir seems to have feared further attacks on his classificatory work from Boas and Goddard (cf. Boas 1920, Goddard 1920). In a letter to Robert Lowie written at about this time, Sapir expressed a disinclination to "run foul of" Boas and engage in "a family quarrel" (Sapir to Lowie, Feb. 15, 1921. See Sapir 1965:44-46).
  - 2 Sapir took Kroeber's advice to the extent of publishing a synopsis of his classification in Science later in the year (Sapir 1921d), but this one-page note included little of the typological characterization of the six "superstocks" that, to judge from the outline, had made up the bulk of the original paper. The full paper, somewhat revised, and with the addition of a section on Mexican and Central American languages, was published in 1928 as an article in the 14th edition of the Encyclopaedia Britannica (Sapir 1928b). The map to which Kroeber refers was a hand-drawn map of the distribution of the superstocks in North America; Sapir had used a lantern-slide of this map to illustrate his talk, and probably sent the original to Kroeber along with the outline.

[325]

[Berkeley]

April 2, 1921

Dear Sapir

Heye was here recently, the deal was completed, and Waterman is going with him.<sup>1</sup> We are going to have Lowie.<sup>2</sup> He is the peg that precisely fits our hole. He wants to plug in and there'll be no whittling needed.

I did not pass you over lightly and what I have to report may interest you. To begin with, you would not want the job as it is. You work with etcher's tools, and the wide-sweep frescos we do would irritate you. Every time you took the three-inch brush in hand you would resent it, and each time our mob remained indifferent to one of your subtle nuances, it would disgust you. Nor are we big enough to have a place for you as a cameo carver. There are 600 high school students to be fed every semester and 100 so-called upper-division ones. Whatever of this work the other man doesn't shoulder, falls on me.

I did suggest to the President and Dean that if they saw a chair of pure linguistics or comparative philology ahead, this vacancy in anthropology might be used as a bridge. But they pointed out the normal difficulty of the University committing itself to an undertaking ahead, and the impossibility at this moment of tangled and uncertain finances. I brought the matter up again before the special faculty committee to pass on Lowie: as a theoretical alternative and a practical possibility as soon as the University was ready to consider a straight-out chair of Linguistics. I think the seed was sown, and I will follow it up as I find chance with the more intelligent of the language people. I believe that you may consider that a certain amount of ground has been broken.

At the same time an outside man of standing, especially a non-anthropologist, could probably do more for your chances here, in the long run, than I. Do you know anyone who will make propoganda for you?

You should realize, also, that pleasant as it is to live here, you would probably be discontent at having no student following of any quality. There

just isn't any clientele of people of highly refined and purely intellectual interests. We turn out some good chemists, etc., to be sure, but that is because their laboratory problems have a certain kinship with industrial problems. As to the man that wants to be a pure scholar, he doesn't grow here, except for half-baked individuals not even capable of recognizing their own incapacity. You offer a rare and precious commodity, and the place to sell it is where rarities of the spirit are sufficiently gathered to establish a market. Harvard, Columbia, Johns Hopkins, perhaps Chicago--that is where you would build a school.

Still, if you're really willing to forego, and Berkeley looks good to you, I'd like to see you here, and I'll do what I can in that direction.<sup>3</sup> But do publish all you can that "philologists" will read, and when your book comes out, place copies around. I'll give you some names here.

Yours

Kroeber

- 
- 1 George G. Heye (see Letter 43, fn. 2), a wealthy amateur anthropologist, had offered Waterman a curatorial appointment at the Museum of the American Indian, in New York. Waterman remained with the Heye Museum only for a few months, before moving on to yet another temporary position.
  - 2 Robert Lowie, who had taught at Berkeley in 1917-18 as a visitor, returned in 1921 for a permanent appointment. See Letter 314.
  - 3 In a letter to Lowie, written a few days after receiving this one of Kroeber's, Sapir expressed some scepticism on this matter: "When you get out to California, I shall certainly appreciate your doing what you can to get me a linguistic professorship. Do you think Kroeber wants me out there? I hae' me doots!" (Sapir to Lowie, April 19, 1921. In Sapir 1965:47-48.)

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171 West 81 St.  
New York

Dear Sapir:

I am sorry if I seemed apologetic. I meant to convey regret. Lowie obviously is the particular man for the particular place. But it would be pleasant

if there were an ideal place to fill. And if there's ever an opening for you in Berkeley in sight, I'll try to make it. Though I believe in the long run you'll have to forge out your assets to the point where they'll force your opening. That, in the end, is more likely to be here in the East than in Berkeley;<sup>1</sup> and to be more satisfactory to you. I'm very keen to see your book. Perhaps I can get the manuscript from Goldie.<sup>2</sup>

Is there anybody I should see while here that I'm likely to overlook, or anything I can do for you?

Boas looks well and rather happy.

Yours

Kroeber

4/29/21

- 
- 1 Kroeber--who was on leave from regular teaching--spent part of April and May, 1921, in New York. The return address is his mother's.
  - 2 Alexander Goldenweiser. Sapir had also asked Lowie to read the manuscript of Language, with particular attention to "weakness in style" (Sapir to Lowie, April 19, 1921. In Sapir 1965:47-48.)

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Victoria Memorial Museum

Ottawa, May 4, 1921

Dear Kroeber,

Will you please let me know how one may best get to the Hupa Reserve<sup>1</sup> from the north, that is, travelling south from Vancouver on the way to San Francisco? It is possible that after spending three or four months on the Sarcee Reserve I may want to spend a month or a month and a half in the fall among the Hupa Indians in order to get a useful comparative Athabaskan basis from which to work. Should there, as I strongly suspect, be tones in Athabaskan, it will of course be of the greatest importance to determine whether the tone alternations of two such widely separated dialects as Sarcee and Hupa correspond or not.

Any information you can give that you think would be useful to me in getting to the Reserve and getting along on the Reserve would be gratefully received by me. Should I go to the Hupa, I am very likely on the way back to travel via San Francisco and shall in that event have the pleasure of seeing you and Lowie before I return to Ottawa.

I should appreciate it very much if you could return the first six chapters of the manuscript<sup>2</sup> in advance of the rest as certain corrections remain to be entered on another carbon copy.

With best wishes,

Yours very sincerely,

[Edward Sapir]

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1 The Hupa are an Athabaskan-speaking group living on a large reservation on the Trinity River in northwestern California. Goddard had published a number of works on Hupa linguistics (cf. Goddard 1905b, 1911), but Sapir felt that better data were needed, especially on tonal differences (which Goddard had not noted at all in the language). While field work of any sort proved impossible for Sapir in 1921, owing to Florence Sapir's deteriorating health, he was eventually able to make a thorough study of Hupa in the summer of 1927. Ironically, Hupa proved to be a toneless Athabaskan language (Sapir 1928a).

2 Of Language (see Letter 326).

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171 W 81, N.Y.

May 15, 1921

Dear Sapir:

You can get off at Grant's Pass, Oregon, then go by auto stage to Crescent City (Tolowa), Requa (Yurok), and Arcata, thence inland 35 miles to Hupa. I think there is also a direct stage line now from the upper Sacramento valley to Eureka and Arcata, but I should have to inquire locally. The simplest way probably would be to come to San Francisco, thence 280 miles north by coast train to Eureka and Arcata. Halfway up about a dozen miles to west and east

the Kato and Wailaki.<sup>1</sup> --From Arcata to Hupa is a cul-de-sac, in any event, unless new roads have recently been built.

When do you leave and when do you reach California?

I finally extracted your ms. from Goldie. I have passed ch. 1-5 on to Lowie to forward to you, and shall follow with the remainder in a day or two. Comment when finished.

Yours

A.L.K.

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1 Two other Athabaskan groups, on whose languages Goddard had also published.

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171 West 81 St.

[New York, NY]

May 23, 1921

Dear Sapir:

I am awfully sorry luck has broken against you.<sup>1</sup> I wish I could advise on the side of income. But my experience has been that that problem must normally be worked out unaided. It shows how inwardly unrelated with the rest of our lives society makes the economic bases to be. After all, socialism springs from a sound psychological foundation, though the fool socialists do their best to undo it. That's cold comfort, but keep up your courage.

My hat is off to you. You have written a remarkable book; I think a great one. I won't specify until I can read it leisurely in print, and perhaps not then. At present I should utter only a string of admirations, and one hates to be fulsome. At any rate, there isn't a passage I would have different.

As to its effect, I would caution you not to be oversanguine as to immediate results. You are likely to get some commendations and slow sales. The mass of philologists will hold aloof, inwardly resenting its revolutionizing character; and there are rather few people really interested in language, and those scattered and unorganized. But the book will get around to them



gradually, and grow, and be the standard classic for a generation. You can count on that.

I see one or two possibilities for more [illegible] scientific development of subjects which space limitation has prevented you from exploiting as I have no doubt you would like to. More about those, if you like, after the volume is out.

I think you should try to get reviews from Jespersen and Meillet. If there is any organ for which you would like me to do one, have Harcourt make the arrangement and count on me. I might get in a prompt opinion that would give the cue to reviewers feeling the need for a guide.<sup>2</sup>

I leave here the 26th, so address me in Berkeley.

Yours

A. L. K.

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- 1 Referring to the illness of Sapir's wife, Florence, and to the expense of her hospitalization, which was severely straining Sapir's resources. In a letter to Lowie at this time, Sapir speaks of "acute distress" and of an earlier letter to Kroeber telling him of Mrs. Sapir's condition (Sapir to Lowie, May 23, 1921. In Sapir 1965:48-49.)
  - 2 Kroeber reviewed Language for The Dial (Kroeber 1922a), but neither Jespersen nor Meillet are known to have reviewed it.

[330]

[Berkeley]

June 27, 1921

Dear Sapir:

I am relieved at what you tell me.<sup>1</sup> The diagnoses indicate little as to cause or treatment, but much as to prognosis. For manic-depressive the chances are high for recovery from the attack and excellent for permanent outcome. The clearing up of the delusions so soon is a most favorable symptom. Don't worry about the feeding compulsion. Such states are not deteriorative, which is the most crucial factor.

There is of course no real "treatment" for psychosis. Analysis may speed a started recovery and especially integrate it. Emergence from the depressive phase is said to be the time of attack, and so appeals to me. I should say analysis then might keep the wound draining, so to speak, and so lead to longer tissue restoration (metaphorically) than under the crust of a quick spontaneous recovery. As regards the ultimate recovery, as also threatened onset, the great point seems to be to get the patient to face the situation. I am sure that too often the desire to forget the experience, which is aided by the environment, tends to lay the foundation for a recurrence. Of course mere reminding is likely to precipitate one even sooner. The job is a delicate one at best and must be felt out with skill and trust. By all means do you keep away from it. A physician wisely does not handle his own family for physical difficulties; a fortiori where you are inevitably involved either as cause or material.

I know no one practising in Canada. I would advise you to write to W. A. White, Sup't of St. Elizabeth's Hospital, Washington, for his recommendation.<sup>2</sup> He was a psychiatrist first, came into psychoanalysis later, and takes it sanely. Most analysts do not. He is widely connected, practical, and his 3000 patients keep him in touch with reality instead of theories mainly. He impressed me with reliance the two times I met him. Tell him your doubts both ways, and if there is any one in reach that will help your wife, he will know him.

I have a strong intuition that when she comes far enough back a strong transference will help her a lot. But it must come at the time when she is plastic--not in the depths nor after a year of crystallizing subsequent to "cure."

Good luck.

Kroeber

- 
- 1 Sapir evidently had relayed the preliminary diagnosis of his wife's condition to Kroeber.
  - 2 William Alanson White (1870-1931), a central figure in the development of clinical psychiatry in the United States. Sapir did get in touch with White, who recommended Dr. E. J. Kempf, a New York psychoanalyst. After an

extensive correspondence with Kempf, Sapir decided against therapeutic psychoanalysis for his wife, partly because of the expense (Sapir to Lowie, March 14, 1922. In Sapir 1965:51-52.)

[331]

[Berkeley]

July 17, 1921

Dear Sapir:

I shouldn't worry much about present diagnosis. It is notoriously difficult in psychiatry, especially in initial conditions. It would be hard to say what is specifically characteristic of praecox except that it has a distinctly deteriorative trend. If your wife is better and they expect to send her home before long, that doesn't look like deterioration.

Neither need you chafe at the doctors not being psychiatrists. Experience, intelligence, and humanity are worth a hundred times more after a psychosis has once broken. After all, there is something quasi lesional about a psychosis, as compared with the functional neuroses. When a watch runs off time but runs, the cause of the error is all important. If a part is broken, inquiry into cause has only scientific interest: the job in hand is to locate the part and replace it. Nature does that or doesn't. The psychiatrist is there to see that she gets every chance. If he's a good man, he knows how, empirically.

Once you get a recovery, analysis may be in order. Conducted now, it might lead to insight, but scarcely to any effect.

Surely, psychoanalysts are neurotics, just as psychiatrists are a bit nutty, according to their own saying. Why not? The fellow that's brimming over with normality will have scant patience with the abnormal, generally, and little understanding.

What you observe in your wife's symptoms is no doubt just, but it applies to psychosis in general. It is always the affective rather than the intellectual sides that are involved.

You asked me recently what I got from being analyzed. It's hard to say. I have felt clearer, surer, readier to accept the inevitability of reality cheerfully, ever since. How far the analysis did it, and how far other experiences, such as the outlet of the new undertaking of practice, I don't know. There is nothing very specific I can point to as an effect. But then there was nothing very specific that was wrong. All I know is that I'm glad I had it. If you feel the need insistently, you almost certainly have the need. If you're rather indifferent or merely curious, treatment would probably be rather sterile. That's what I tell prospective but doubting patients.

Yours

Kroeber

[332]

Brockville, Ont., Oct. 1, 1921.

Dear Kroeber,

I have long wanted to write you about Nadene and Indo-Chinese, but my evidence accumulates so fast that it is hard to sit down and give an idea. Let me say this for the present. If the morphological and lexical accord which I find on every hand between Nadene and Indo-Chinese is "accidental," then every analogy on God's earth is an accident. It is all so powerfully cumulative and integrated that when you tumble to one point a lot of others fall into line. I am now so thoroughly accustomed to the idea that it no longer startles me. For a while I resisted the notion. Now I can no longer do so.

The chief stumbling-blocks in the way of a general acceptance of the synthesis would be: 1. Failure to realize the very exceptional type of language to which Nadene belongs. It is really quite alone in America, so far as I can see. The contrast between it and Eskimo, Wakashan, and Algonkin is tremendous. 2. Failure to realize that the Nadene languages are not one-third as synthetic as they look. Go at analysis half-way decently and get into a critical perusal of connected text and you soon realize that the complex verb breaks down into a cluster of very live elements, each of which has a syntactic or positional

value, not merely as "affix" but as radical element. Haida in particular, I find, is extremely analytic. It knows no "prefixes" and "suffixes" at all except for certain important survivals that Swanton does not even mention<sup>1</sup> (e.g. causative and denominative s- in s-kít "to handle a club, to club" < kít "club", cf. Tlingit s-, transitivizing prefix; also Haida ɬ- = Tlingit ɬ- = Athabaskan "3d modal" ɬ-, also primarily transitivizing). What Swanton calls affixes are all independent stems entering into composition, or even little verbs. There is no "tense suffix" in Haida, merely a series of enclitic phrases consisting of demonstrative+particle verb of being. His "continuative" -gAff, e.g., is simply "that- is (duratively)," his "imminent future" -asañ is really -'a-sa-ŋ "this-will-be (durative)," and so on. It all crumbles to pieces at the least touch. I think the same will prove true of Athabaskan-Tlingit, though here the integration is more thorough. But I no longer seriously believe we have the right to consider anything preceding the "second modal" elements (Ath. ɣ-, n-, and s-) as part of the verb and am beginning to doubt if even these elements and the subjective pronominal "prefixes" are part of the true verb. I think it more than likely that such an Ath. form as \*yasēcik'os\* "I picked up a flexible object" is to be analyzed as \*ya sē c ɬ-k'os "up it-is (that) I handle-a-flexible-object." \*ɬ-k'os is the verb; the rest is a series of somewhat reduced independent elements that follow in a definite order. 3. The third prejudice to overcome is the nature of Indo-Chinese itself. Modern Chinese is a very secondary development. The most typical representative of the earlier stage is Tibetan--which is startlingly Nadene-like. It has those fundamentally important "3d modal" elements of Ath., Tlingit, and Haida (e.g. du- "to be together": s-du- "to cause to be together, to assemble;" in fact, 4 of its most important verb prefixes, which are "voice" elements, seem to me to correspond in form and meaning to Nadene elements-- s- to Tlingit-Haida s-; r- to Tlingit-Haida-Ath. ɬ-; d- (medio-passive) to Tlingit-Ath. d-, survivals also in Haida; nasal prefix to Ath. "3d modal" -n-, -ŋ-, of mysterious value but probably active intransitive). Moreover, Tibetan has vocalic ablaut in its verbs (e.g. ŋ-geŋ-s "to fill", perfect b-kaŋ, fut.

\* Purely theoretic form ad hoc! Not to be mistaken for genuine Ath.

d-gaŋ, imperative k'əŋ). Again, the transitive verb is really passive, as in Tlingit. In both, for instance, you would say "Man-by horse kill" = "The man killed the horse," Tlingit agentive -tc corresponding exactly to Tibetan instrumental -s. In both Indo-Chinese and Nadene, postpositions are of extreme importance and serve to subordinate preceding verbs and clauses. Indeed, reading Tibetan text gives you precisely the same feeling as reading Haida text. I wish I had time to illustrate. In both groups the fundamental element is really a noun, the verb a kind of denominative structure. In brief, I should say that the similarity in feeling between Tibetan and Nadene is at least as close as between Latin and English, probably closer. Thus the theoretical road to a synthesis is clear. And the lexical evidence is startling. You would be amazed at some of my material. Things like:

1. Tlingit k'a "surface"; Navaho k'ā "surface": Tibetan k'a "surface"
2. Chinese t'an "charcoal": Haida s-t'an "charcoal"
3. Old Chinese ti "this": Ath. di "this" (Ath. di really means ti)
4. Old Chinese ti "pheasant": Ath. di "partridge"
5. Nadene k'u "hole" (Tl. k'ú-q'ú "hole", t'a-t'ú-k'ú "cave" = "rock-interior-hole", ya-k'o "to fall into a hole"; Nav. k'o, e.g. ts'e-k'o "rock-hole" = "canyon"): Indo-Chinese k'u "hole" (dozens of forms, e.g. Tib. k'u-ŋ "hole," Karen k'u, with falling tone, "to dig a hole").

These are only a drop in the bucket. Naturally it is a big problem and there are going to be hundreds of knotty points to unravel. But I do not despair. My present plan is to proceed as follows. First, to prepare part I of a Nadene comparative study, to consist of my present lexical material (about 300 comparable radical elements, to which I add constantly). In this I would give reconstructed Athabaskan but also actual Ath. dialectic forms.<sup>2</sup> Before publishing parts 2 and 3, on morphology and phonology, which need much preliminary work, I intend to publish special papers on selected portions of Nadene grammar, e.g., certain archaic post-positions; or demonstrative stems; or general points of syntax.<sup>3</sup> In this way I shall be keeping the problem live and accumulating experience for the definitive Nadene study. Of course I shall have to do Ath., Haida, and Tlingit in the field.<sup>4</sup> I want particularly to gather a large amount of purely lexical material. People do not realize now

scanty is our material, and for my purpose, which is comparative, I need stacks of it. What Goddard gives us is a miserable pittance--and wretchedly analyzed or not analyzed. Did you read his attack on my Nadene?<sup>5</sup> You can have no idea of the laughable errors he commits. It is the work of an utter groundling that does not know his own material. What do you think of a man who expects you to unravel the complex phonology of Nadene by drawing up an alphabetical list of Tlingit "stems" and "matching" them with random "stems" from Kato or Ten'a ad libitum? Very much as though you "matched" Sanskrit words now with French, now with Portuguese. Great method, what? And what do you think of a man who rules out comparisons because he does not "know" the Ath. form in question? Particularly when the form occurs in a book (Chapman's Ten'a) which he has "edited"!<sup>6</sup> I may reply to Goddard, but it is really no use. He is a man of no more than average linguistic ability, completely at the mercy of his local sentimental memories, and absolutely without vision as to the older drift of Ath. He probably imagines his lists of stems are the last word on the whole subject. The degree to which he has failed to analyze his material is shocking in the extreme.

I shall not broach the Indo-Chinese part of the problem till I have moored myself more completely in Nadene. The final plan is: 1. A Nadene comparative grammar to be published in 3 parts (possibly an Ath. etymological dictionary as side-show); 2. a Nadene-Indo-Chinese demonstration; 3. a more general treatment of the evolution of the whole group, showing how old types have been replaced by new ones. As a starter, I am at work now on a paper on Haida phonetics, which may interest you when you see it.<sup>7</sup>

What's the news your way? (Don't blab too much about my Indo-Chinese just yet. It's not wise. "Einem Narren zeigt man nicht die halbe Arbeit," and 99% of one's fellowmen are damned fools.)

Sincerely,

E. Sapir.

P.S. I cannot resist the temptation to give a somewhat livelier idea of the remarkable way in which lexical elements are interwoven in Nadene and Indo-Chinese. I have some cards along, so don't need to trust to memory. I shall

give an idea of the richness of some of my entries by dealing with a group of related words.

In Ath. we have a stem \* $\dot{t}u$ , post-vocalic \*- $\dot{t}u$ , which may be rendered as "coil" or "loop;" e.g. Nav.  $\dot{t}o$  "loop," as vb.: Nav. - $\dot{t}o$  "to catch with a rope," Jic. Apache - $\dot{t}o$ ' "to lasso," Chipewyan - $\dot{t}u$ , - $\dot{t}u$  "to be caught in a net or noose." So far, so good. Here our friend Dr. P.E. Goddard would end. But it is difficult to believe that Ath. \* $\dot{t}o\dot{t}$  "rope, strap" (found in all dialects) is unconnected. How? Without going into details (it would take too long), I may say that I feel justified in analyzing \* $\dot{t}o\dot{t}$  into \* $\dot{t}$ - $\dot{t}o$ '- $\dot{t}$ . How \*- $\dot{t}o$ , - $\dot{t}u$  is related to \* $\dot{t}u$ , - $\dot{t}u$  I cannot yet tell, but I strongly suspect Nadene had both  $\dot{t}$  and ' $\dot{t}$ , and in related stems. As you will see from my Haida paper, Haida has both  $\dot{t}$  and ' $\dot{t}$ ; in Tlingit ' $\dot{t}$  probably became  $\dot{t}$ . I should guess that ' $\dot{t}$  is causatively related to  $\dot{t}$ ; Ath. \* $\dot{t}u$ , - $\dot{t}u$  is intr.: "loop; to lie coiled," \*- $\dot{t}u$  would be "to cause to be coiled, to make a loop, to tie around" (possibly - $\dot{t}o$ ' is a secondary form of \*- $\dot{t}u$ ). Now - $\dot{t}$  we know to be continuative or usitative; and  $\dot{t}$ - is medio-passive. Hence \* $\dot{t}$ - $\dot{t}o$ '- $\dot{t}$  is "what is always caused to loop around, what loops about something," in other words "rope, strap." We learn important things from such an analysis: that "3d modal" elements were welded with verb stems and appear in nominal derivatives; that there was an old alternation  $\dot{t}$ : ' $\dot{t}$  whose significance remains to be discovered. That we are on the right track is confirmed by another common Ath. stem whose formation is precisely parallel to that of \* $\dot{t}o\dot{t}$ . This is \* $\dot{t}\dot{t}\dot{t}$  "fire-drill." Fortunately we are here not dependent on Goddard's material alone. From Petitot we learn that in Hare and Loucheux there is a verb stem \*- $\dot{t}\dot{t}$  (- $\dot{d}\dot{t}\dot{t}$ , - $\dot{t}\dot{t}\dot{t}$ , - $\dot{t}\dot{t}\dot{t}$ ) meaning "to revolve" (words involving it are: "virer au cabestan," "cylindrique," "tourbillon," "tourner," "se tourner"). Hence \* $\dot{t}$ - $\dot{t}\dot{t}$ '- $\dot{t}$  is "what keeps turning itself, what revolves drill-like." This parallelism of  $\dot{t}o\dot{t}$  "rope" to  $\dot{t}\dot{t}\dot{t}$  "fire-drill" is, of course, highly suggestive. It shows that many of Dr. Goddard's "stems" may not be pure father-Adam radicals. And we see that Ath.  $\dot{t}\dot{t}$  fails to correspond to Haida and Tlingit  $\dot{t}\dot{t}$  for a reason. It is a secondary development in probably all 3 groups. Such a sound as  $\dot{t}$ ' appears in cognate words throughout; not so  $\dot{t}\dot{t}$ --which fact alone casts a reflex light on our analysis.



Let us proceed. To Ath. \*lu, -lu is clearly related Ath. \*-lui "to wrap around": Hupa -loi "to tie, to wrap around," Kato -li' "to tie up" (old Ath. form possibly causative \*-'lui). And further, having once allowed Ath. li to analyze itself into medio-passive l- (d-) + 'l, we do not feel we are doing anything ungodly to analyze Ath. \*t lu' "grass" into \*t-'lu' "what is wound (in basketry)." This analysis of "grass" is helped by denominative Ath. verbs, e.g. Hupa- tlo-n, tlo, tlo-w, tlo-i "to make baskets, to twine in basket-making"; Nav. tlo, tlo', tlo-i "to tie (e.g. the hair)."

We may summarize all this as follows:

Ath. \*lu, -lu "coil, loop"

- \*lu "to be caught in a noose"

- \*'lu "to catch in a noose"

- \*'lui "to wrap around"

\*t-'lu' "what is twined" > "grass"

(denominative vb.: "to twine in basketry; to braid hair")

\*t-'lo-i "what is always looped" > "rope, strap"

Now comes the fun. Indo-Chinese lu as follows:

Tibetan lu-ŋ "a strap, slung over the shoulder or round the waist, for carrying things"

Miao lo-ŋ "bridle" (close o)

Tib. metaphorically: caus. s-lu "to cause to be snared," i.e. "to ensnare, beguile, seduce"

Angâmi Naga te-rhu "sly" (rhu < h-ru; Tibeto-Burman hl-, hr- > lh-, rh- is exceedingly common; h- is common as causative prefix, e.g. Tib. s-l- often parallel to lh-)

T'ai group: Siamese roi<sup>(2)</sup> "enfiler" (numbers indicate tones in H. Maspéro's orthography); White Tai roi, loi "enfiler"

And now Chinese: lu- group: luŋ<sup>(2)</sup> (numbers for tones according to usual Modern Pekinese system) "a cage, to snare" < Old Chinese (i.e. 7th Cent. forms, which I have worked out carefully from Karlgren's tables)<sup>8</sup> lu[o]ŋ- (- = level; / = rising; = falling tone); luŋ<sup>(2)</sup> (in luŋ<sup>(2)</sup> t'ou<sup>(2)</sup> "halter," i.e. "snare-head") < Old Chinese lu[o]ŋ-; lu<sup>(4)</sup> "girdle gem" < luo (words with falling tone seem frequently to be old passive derivatives: "what is

looped around one's waist?"); lu<sup>(2)</sup> "thatched hovel" < luo-; lu<sup>(2)</sup> "hempen thread" < luo-; lo<sup>(2)</sup> "conch, spiral, screw" < luâ- (â is a dark-timbred a-vowel); lo<sup>(2)</sup> "lines in the palm" < luâ-? lei<sup>(2)</sup> "to creep, cling to," same character also read lei<sup>(3)</sup> "series, connected" (words evidently refer primarily to creeping vines) < luâi-, luâi/. These Chinese lu, luâ forms are paralleled by another set in lui- (cf. Ath. \*-lui above): lei<sup>(3)</sup> "to bind" < l<sup>y</sup>wie- (< \*lwie-); lei<sup>(3)</sup> "a creeper (as of melon or pea)" (not sure of reconstruction, but phonetic element in character suggests initial lu or l<sup>y</sup>wi); lü<sup>(3)</sup> "silken thread, a hank" < l<sup>y</sup>iu/. And metaphorically we have lup<sup>(4)</sup> "foolish, to impose on" < lu[o]ŋ ; lü<sup>(4)</sup> "deceitful" < l<sup>y</sup>wio- (i.e. "winding about, ensnaring with blandishments;" cf. Tibetan s-lu above).

Other probably connected Tibetan words are: lhub (i.e. causative h-lub) "to bind, tie, fasten (e.g. ornaments to the ear)"; k-lub "to cover (e.g. the body with ornaments);" lwa-ba "a woolen blanket."

Observe how well the Ath. forms integrate with the numerous Tib. and Chinese forms. But we are not done. Very likely connected with Ath. \*-lu is Ath. \*-lu-s "to drag an animal by a rope": Nav. -lõs, -lo'z, -lõs classifier verb "denoting a single animal as an object: the inference is that the animal is led by a rope" (Franciscan Fathers);<sup>9</sup> Hupa -los "to drag, to pull along." With these forms I feel inclined to compare (though here I feel far more hesitant) Chinese lo<sup>(2)</sup> tsz<sup>(3)</sup> "mule" (tsz<sup>(3)</sup> is merely "son," often used to make nouns) < luâ-; also lu "donkey" < l<sup>y</sup>wio-. The parallelism between Ath. and Chinese would be a convergence from related radicals rather than a specific etymological parallel.

But we are far from finished. Perhaps related to Ath. \*-lu is an important classifier verb \*-lɛ, \*-la: Nav. -lɛ, -la, -lɛɛ "to handle a long, flexible object, as a rope, quirt, leather, hide, etc." (Franciscan Fathers). Parallel to this is a set of Indo-Chinese forms in \*la, \*le (Ath. ɛ is often parallel to a; just how related I do not yet understand, possibly reflex of old alternation a: ia):

Tib. causative s-le, lhe (< h-le) "to twist, plait, braid the hair, to make a basket, to knit"; s-le "a coarse basket"; s-le-po, s-le-ba, s-le-bo "a flat basket" (-po, -ba, -bo are "articles"); lha-s, lhe-s < h-la-s, h-le-s

"braid, wicker-work, texture; twisted cake or bun"; lhe-s-ma < h-le-s- "the act of twisting, plaiting" (-ma is "article"); lan-bu "braid, plait, tress of hair" (-bu is diminutive); lan-ts'ar "ornaments worn in the hair"; le-brgan "diapered design of woven fabrics"; le-na "the soft downy wool of goats below the long hair; fine woolen-cloth"; lda-ldi "a kind of ornament of silk or cotton, a fringe or tassel" < d-la- (dl- regularly > ld- in Tib.; very easy to illustrate); ldañ-mgo "the yarn-beam of a loom" < d-lañ- (mgo "head"); ldem-ldem < d-lem "flexible, supple, elastic, pliant"; ldeb- < d-leb- "to bend round or back, to turn round, to double down." I am not so certain of this last, which brings us into a large set of forms in la- and lo- referring to "turning, turning back," which may well be related to our present set but which I prefer, for brevity's sake, not to go into just now.

Now Chinese. We have two series: \*la and \*li. Based on \*la are: lan<sup>(2)</sup> "basket with handle" < lām-; lan<sup>(3)</sup> "rope, howser" < lām/; lao<sup>(4)</sup> (tez/) "netted case" < lāk, same character also read lə<sup>(4)</sup> "joined, to tie up"; lei<sup>(1)</sup> also read lə<sup>(1,2,4)</sup> "to rein in, to strangle" < lək (perhaps better to lu- series above); lo<sup>(2)</sup> "net, sieve" < lā-; lo<sup>(2)</sup> "shallow open basket" < lā-. Based on \*li are: li<sup>(2)</sup> "ornamented girdle" < lyie- (?); li<sup>(2)</sup> "basket" < lyi-; li<sup>(2,4)</sup> "rope to tie a boat," character also read ši<sup>(3)</sup> "a well woven gauze" < x<sup>y</sup>i/ (?), which probably means older \*h-li (loss of post-consonantal l is now well established for Chinese).

Is it not impressive that Ath. and Indo-Chinese \*lu seem to have reference chiefly to "looping," Ath. \*lē, la and Indo-Chinese \*la, li to "handling a long flexible object, twining, basketry"? Now let us return to Nadene. In Tlingit we have łit "fine basket." Remember that Tlingit has no voiced l, only voiceless ł. Further, experience shows abundantly that many Tlingit forms in final consonant (particularly if glottalized) are compounds. Hence we may suspect that łit really means old \*łi-t(a). Reference to Ath. gives us \*-łta "receptacle": Hupa -łta "sack"; Kato -łta "pocket, blanket fold"; Nav. -dzis-łta "pocket" (-dzis "hollow, semi-tubular" ?). This somewhat unsatisfactory parallel is buttressed by Haida łao-łta "box" (almost certainly misheard for t'ao-łta "food-receptacle"). Tlingit łit is therefore probably łi-łta "receptacle (for small objects) of twined basketry." We have further łi "woolen

blanket" < \*li or \*li' (final glottal stops seem regularly to affect the first consonant; I have good evidence for this). Based on \*la or \*lia we have also in Tlingit leq' "tentacles of squid" \*le-q' (with medio-passive le-; Tlingit e is always related to a- forms) < "what is long and twists itself"? (cf. fundamental meaning of Ath. \*le, \*la above).

And in Haida we have dli'n "arm of devil-fish," which I analyze as d-li'-n (d- medio-passive; -n perhaps causative, to which there are good Ath. and Tibetan analogies; -li'- < lia- ?) < "long things that twist themselves about"?

I am quite likely to have misinterpreted here and there, but the Nadene and Indo-Chinese parallels seem highly suggestive to me. Of course, this sort of group-parallelism is not isolated.

I shall refer to another interesting group, without going into details. I spoke of Ath. \*le "to resolve" and its derivative \*lele "fire-drill." To these forms belong Haida li "to surround, move around." In Indo-Chinese we have a well-chambered Tibetan set: re and ril (e.g. re-s "change, turn, time, times"; re-g-re "to roll one's self"; causative s-g-re "to roll"; ril "round"; causative s-g-ril "to wind or wrap round; to roll, wrap, or wind up; to wag (the tail); to roll (a stone)." Here may also belong Haka kut-hrer "ring" (kut- "hand"; h- common Tibeto-Burman causative prefix), though Conradi sets this to Tib. forms in la-, le- referring to "twining."

But I must stop. This letter has got so long that I shall send it to you tomorrow in typewriting and let Laufer have a copy for his criticisms.

Sincerely,

E. Sapir.

P.P.S. I started this as a personal letter to you from Brockville,<sup>10</sup> but it grew so that I decided to have it typewritten and let Laufer have a copy as well.--Mrs. Sapir is improving and has no longer to be tube-fed. Still, she is not well yet and may never be her old self. This thought casts a shadow on all my plans and hopes. I do not really know that I shall ever be able to go to the field again. If I do not, the Nadene program can not go ahead as I should like.--Answer some time and tell me how Goddard's attack looks to you and an outsider. Is it worth replying to?

E.S.

- 1 John R. Swanton, in his grammatical sketch of Haida in Boas's Handbook (Swanton 1911).
- 2 Sapir's comparative Athabaskan and Na-Dene files are preserved in the Library of the American Philosophical Society (MS 4552). No manuscript of a study based on these files (other than what was published as Sapir 1915b) has been located, but one certainly existed. In a letter to Lowie a year earlier, Sapir mentioned a "big comparative study [of Na-Dene] I am undertaking (or rather undertook a long while ago and wrote out to the tune of about 200 typewritten pages)" (Sapir to Lowie, October 26, 1920. In Sapir 1965:40-43.)
- 3 The only paper of this series which Sapir completed was "A Type of Athabaskan Relative" (1923d), "a short... paper which insidiously prepares for far bigger things than its ostensible theme" (Letter 335).
- 4 Sapir carried out extensive field work on Athabaskan, working on Sarcee in 1922, Kutchin in 1923, Hupa in 1927, and Navajo for several years beginning in 1928. His only contact with Haida was his short interview with Peter Kelley in 1920 (see Letter 306). He did no field work on Tlingit.
- 5 A paper entitled "Has Tlingit a Genetic Relation to Athapascan?", which appeared in IJAL late in 1920. The library copy in the National Museum of Man, Ottawa, has numerous marginal comments in Sapir's hand, some of them quite abrupt.
- 6 Goddard had edited J. W. Chapman's Ten'a Texts and Tales from Anvik (1914), for publication by the American Ethnological Society.
- 7 Sapir 1923c, based on his brief work with Peter Kelley (Letter 306).
- 8 Probably in Karlgren 1915.
- 9 From their Ethnologic Dictionary (1910) or Vocabulary (1912) of Navajo.
- 10 On the St. Lawrence River, where the Sapirs had a summer house.

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[No date; presumably October 1921]

Dear Kroeber,

À propos of the recent Ath. \*-lu : \*-le, -la stems I discussed, note the following I have just found:

Navaho -lě, -lǒ', -lǒ "to sew" (Franciscan Fathers note: "make loops?"), which thus combines both sets in one paradigm.

I am going through the Franciscan Fathers' Vocabulary<sup>1</sup> and listing all their verbs in types. Eventually I hope to do this for the other Ath. dialects (Petitot contains a mine of data for 3 languages)<sup>2</sup> and so obtain a solid basis for comparison with the Tlingit types. (Even now one can sense suggestive parallelisms between Ath.-Tlingit and Tibetan types of stem alternation for different tense-modes!) Many dialectic types are naturally merely secondary resultants of phonetic laws; e.g. Hupa -a : -au really means -a' : au < -a' : -ax < -ax : -ax with voiceless : voiced as in -x̣ : -l (a common Ath. type).

Enjoyed your Hawaiian paper.<sup>3</sup> Regards to Lowie & Gifford.

Sincerely,

E.S.

1 Vocabulary of the Navaho Language (Franciscan Fathers 1912).

2 Petitot (1876) is a synoptic dictionary of Chipewyan, Hare, and Kutchin.

3 Kroeber 1921a.

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University of California

Berkeley, November 20, 1921

Dear Sapir:

Lowie has showed me the part of your letter relating to the field assistant.<sup>1</sup> If you wrote me on this subject, the letter miscarried or the passage was overlooked by me. I have been unusually busy and tied up the last couple of months.

De Angulo<sup>2</sup> has quite unusual intellect along with an unstable personality. He gets tremendous pure enthusiasms as a result of which he works some aspect of science through and then drops it. Since I have known him in the past three years his interests in succession have been psychiatry from the psychoanalytic side, ethno-psychology under the influence of Levy-Bruhl,<sup>3</sup> phonetics, and now California general ethnology and linguistics. A few days ago he came back from a month of field work with the Achomawi. He certainly got insight into their

minds, I think a good deal of knowledge on their culture, and laid a foundation for an analysis of the language. Whether he will ever follow the work up to produce a useful monograph I do not know. I have always kept him at arm's length in spite of a good deal of liking for him and a quite thorough admiration because of a fear of his inclination to fall around one's neck when he forms an attachment. Emotionally he is inclined to be vehement and infantile. I suspect myself of an unusual personal resistance towards such warm attachments in the case of scientific and business relationships--perhaps a defense reaction. The factors I mention might not count seriously in your case. I mention them because I know de Angulo much better on the personal side than on any other. You could in any event count on his bringing you unbounded enthusiasm and elan, and an exceptionally keen intellect. Also he is very anxious to break into our game professionally.

Have you any copies of the phonetic committee report of some years ago<sup>4</sup> or can separates still be obtained? Mine have been borrowed and reborrowed until I am stripped clean and we continue to have calls from people who wish to take them into the field, or away from the office.

Answering belatedly your question as to psychoanalysis, I think I can sum up by saying that I believe its effects to be worth while in direct proportion to the strength of the need and conviction which a prospective patient feels. One who is doubtful or halfhearted is likely to get very little from it. On the other hand, whoever wants it badly is sure to profit. I will elaborate one of these days when I have a breathing space.

Sincerely yours,

Kroeber

[P.S.] When that day comes, I'll answer everything outstanding--and add something of my own.

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1 Sapir had written to Lowie two weeks earlier, inquiring about Jaime de Angulo. He began the letter: "I am writing you rather than Kroeber, as Kroeber has not answered a very long letter that I sent him some time ago, so that I presume he is not on hand." (Sapir to Lowie, Nov. 8, 1921. In Sapir 1965:49-50.) Sapir needed a field assistant for his projected work on Athabaskan, Tlingit, and Haida.

- 2 Jaime de Angulo (1887-1950), a flamboyant semi-professional anthropologist and linguist. Kroeber and Radin had first encountered de Angulo at Carmel during the summer of 1919. The three men "discovered common interests in psychology, comparative religion, and Indians" (Olmsted 1966:2), and Kroeber was sufficiently impressed by de Angulo to invite him to teach in the summer program at Berkeley that year and the next. With Kroeber's (and later Boas's) support, de Angulo carried out extensive field research on languages in California and Mexico during the following decade, frequently in collaboration with L. S. Freeland (see Letter 294), whom he married in 1923. For biographical details see Olmsted 1966:1-7).
- 3 Lucien Lévy-Bruhl (1857-1939), known for his speculative works on "the primitive mind".
- 4 Boas et al. 1916.

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Ottawa, Ont.

Nov. 24, 1921

Dear Kroeber,

Thanks for your letter of Nov. 20th in regard to de Angulo. The letter of mine that I said you had not answered was a long one on Indo-Chinese and Nadene. Did you get it? I did not expect you to say you believed my preliminary feeler. I just felt interested to know if you thought there seemed to be enough in it to be worth following up. Of course it is hard for an outsider (part-outsider in your case) to tell. But since I wrote you new and far-reaching points have come to view. All in good time. Just now I am on a short Athabaskan paper which insidiously prepares for far bigger things than its ostensible theme.<sup>1</sup>

Somehow I feel from what you and Lowie write that I had better steer clear of de Angulo. I don't need an enthusiastic genius, merely a Gifford who happens to take linguistics dead seriously and has a real ear. Is there such a creature? I am afraid of a Radin or Sapir who would tire after a burst of enthusiasm and leave me in the lurch or complicate things by following out some brilliant wild-goose chase of his own. Perhaps my real way out is to get some chap near at hand, like a teacher of English who knows a little linguistics,



and break him in gradually for my special needs. Constant contact is rather essential, I fear. Perhaps you have a tip of your own. Is there anybody around you could recommend? Boas has not mentioned any one yet. Ladies not wanted.

I had only one copy of our phonetics report and Waugh, who is out in Labrador now, took that with him. I understand from Boas they printed only a small edition, but I do not know if copies are available or not. It should really be revised and in part simplified. It is very bad also to have 2 systems, but Goddard wanted Part I.

Shall be eager to get the "something of your own."

Mrs. Sapir has improved considerably but is still not home. I hope she can be before New Year's. Their final diagnosis was manic-depressive.

Sincerely,

Sapir.

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1 "A Type of Athabaskan Relative" (Sapir 1923d).

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University of California

Berkeley, November 26, 1921

Dear Sapir:

Under separate cover there goes forward to you today first proof of your Yana Elements. Congratulate us on the resumption of printing. Please return the proofs to me.

Lowie tells me that you would be glad to have a copy of our Source Book.<sup>1</sup> Is there any chance that you might place a review of this in some journal preferably non professional? The University Press has fanatically stringent rules about giving out no copies. I finally got them to modify these to the extent of letting us have a limited number of volumes to send out for review and to teachers in anthropology whose classes were prospective users.

I am sorry not to have replied previously to your interesting philological letter.<sup>2</sup> I find it quite impossible to comment in detail without going over the data intensively and probably following them up with systematic work. Somehow I cannot trust my judgment as to resemblances very far without having some feel of the languages from which compared elements have been detached. So far as I can have an opinion under the circumstances, it is favorable. But in the last analysis I presume that in a case like this my opinion contains as its largest ingredient the conviction that Edward Sapir's judgment on such matters is sound.

Can you come to much of a decision in regard to an article, for instance, in the last number of "Anthropos" in which the language of Bornu was linked with Sumerian?<sup>3</sup>

While I am dictating this the postman brings your book on "Language". Thanks ever so much. There is no letter from the publisher with it. You wrote something last spring about plans for review. What are your present arrangements or intentions?

Sincerely yours,

Kroeber

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1 Kroeber and Waterman 1920. See Letters 263-4.

2 On Sino-Na-Dene (Letter 332).

3 Albert Drexel, "Bornu und Sumer", in Anthropos XIV-XV (1919-20). By "Bornu" Drexel seems to mean Kanuri and other languages of Greenberg's "Saharan" group of the "Nilo-Saharan" phylum.

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University of California

Berkeley, January 11, 1922

Dear Sapir:

The Dial sent me your book and asked for a review, which I agreed to let them have as soon as possible.<sup>1</sup> I would rather write them before I commit

myself to undertaking another. As yet I have not heard from Sandwell.<sup>2</sup> It is not a question of pay, but of finding something to say.

Sincerely yours,

Kroeber

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1 Kroeber 1922a.

2 B. K. Sandwell, editor of Saturday Night, a Canadian periodical.

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University of California

Berkeley, February 13, 1922

Dear Sapir:

At de Angulo's request, I am forwarding to you his draft of Achumawi Grammar as he recently wrote it out.<sup>1</sup> Since then he has had an Indian down and has taken him to his ranch where they are to live together for a while. He will, therefore, no doubt amplify and perhaps revise the present sketch. He would like your reaction. I think you can be wholly frank in your criticism. His interest is genuine and he can take comment rather more impersonally than most people. I consider the sketch a very good job, the chief fault perhaps being a tendency to make the picture of the language too hard and sharp.

Please return the manuscript to me here.

Sincerely yours,

Kroeber

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1 De Angulo had done field work on Achomawi in the previous year (see Letter 334). A revised version of his grammar was published in IJAL in 1930, with de Angulo's wife, L. S. Freeland, as co-author. See also Olmsted 1966.

[339]

Ottawa, Ont.

March 13, 1922

Dear Kroeber,

Under registered cover I am sending you a paper on "The Algonkin Affinity of Yurok and Wiyot Kinship Terms".<sup>1</sup> I had originally planned to have a more elaborate discussion of the sociological part of the paper (Part II) but finally decided not to be too elaborate, as too much would be hypothetical. After all, we must wait until we can get a conclusive idea of the typical Algonkin kinship system--there has been so much dialectic differentiation and, probably, borrowing of certain features from alien tribes, e.g. Siouan. So I decided to stick pretty much to the linguistic evidence and allow some one else, later on, on the basis of this paper, Gifford's forthcoming study,<sup>2</sup> and somebody's specific study of Algonkin terms, to work out the distributional facts and the probable perspectives and borrowings. Besides, I want to go to New York for I don't know how long and dispose of this paper, which has been in an all but finished state for an age, right now. There are too many other things I want to do just now to delve any further into Algonkin kinship terms. Anyway, some 30 linguistic Cal.-Algonkin parallels (see Part I) is quite as good picking as one has a right to expect.

I do hope you can publish it. Don't say no. Your series is the only logical place for it. If I give it to our people here, God knows if it will ever appear. They already have my "Phonetic Study of Haida" and I want to save for my next attack a rather extensive series of "Nootka Texts (Tales and Ethnological Narratives)" that I have been working on for some time.<sup>3</sup> I think it is a paper that should be published--I mean it clinches my older linguistic paper on Californian Algonkin and it throws a rather vivid light on the possible historical value, quite aside from fantastic sociological theories, of kinship terminologies.<sup>4</sup>

If you are disposed to accept it, please let me know soon. There will probably be no trouble in getting the Director's consent to have it published in Berkeley. Just one other point. It might be a good idea to accompany it

with a sketch map showing the location of Wiyot, Yurok, and the various Algonkin tribes mentioned. If it is not convenient for you to prepare the map in Berkeley, I can easily get it done by my divisional artist, who has had plenty of cartographic experience.

Sincerely,

E. Sapir.

- 
- 1 Sapir 1923b.
  - 2 "California Kinship Terminologies" (Gifford 1922).
  - 3 In fact, the National Museum of Canada published neither of these. The paper on Haida phonetics appeared the following year in IJAL (Sapir 1923c), and Nootka Texts (co-edited by Sapir's student, Morris Swadesh) was eventually published by the Linguistic Society of America in its William Dwight Whitney series (Sapir and Swadesh 1939).
  - 4 Kroeber was unable to take the paper (see Letter 340) and Sapir published it in the Journal de la Société des Américanistes de Paris.--The allusion to "fantastic sociological theories" refers to the use W.H.R. Rivers and others were making of comparative kinship data.

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University of California

Berkeley, March 28th, 1922

Dear Sapir:

I am very dubious as to your Yurok-Algonkin kinship paper. I should like tremendously to see it appear in our series. Logically it belongs there. I hesitate because, while we have resumed printing, it is on a reduced scale as compared with the past, and prospects are not very good for extension in the near future.

I doubt very much whether your paper would get by the Editorial Committee. The rules provide that we print manuscripts presented by members of the University or based on materials belonging to the University. The tendency has always been to interpret these rules liberally: to include, for instance, papers written by a non-member which bore on a subject which was the special

topic of research of some member of the University. But I am almost certain that the Editorial Committee, which is the body that has final jurisdiction, would no longer sanction such an interpretation. They have worked very hard to effect economies, even advising in detail how manuscripts might be condensed. Several of our papers have been withdrawn because they did not seem urgent; one of my own, for instance, and the first installment of Paul Radin's Wappo texts. There are several manuscripts which we have had on hand for some months and have not filed because of the congestion.

You will see that under these circumstances I could hardly anticipate anything but a rejection if I submitted your paper. In fact, I rather feel that the situation is so difficult that my recommendation might work in the direction of disturbing in some degree the cooperative relations which the Committee and I have managed to maintain in this trying period when their business has been to cut and mine to save as much as possible.

If you see no other outlet for your manuscript, I would suggest that you let it lie for a while. There is a possibility that conditions may improve after the summer. I hardly think this likely, but have no other recommendation to make. In any event, I will hold your manuscript until you instruct me what to do with it.

I have your brief comment on de Angulo's manuscript.<sup>1</sup> You spoke of returning the latter to me, but it has not yet come to hand.

Sincerely yours,

Kroeber

---

1 "Achumawi Grammar" (see Letter 338).

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New York, N.Y.

April 6, 1922

Dear Kroeber,

Have just received your letter of March 28th. I am sorry you cannot use my kinship paper. It is hard to see where else it would fit in. Kindly return it to me, c/o Mrs. Brockway, 419 W. 118th St., N.Y.

I am down here for a month or two. Mrs. Sapir is now in the Bloomingdale Hospital. I hope she will be perceptibly improved there.

Sincerely,

E. Sapir.

[342]

University of California

Berkeley, April 11, 1922

Dear Sapir:

Under separate cover your manuscript goes back to you.

Drop me a line next week in care of the Cosmos Club, Washington, to say how long you expect to be in New York. I shall arrive there the latter half of the last week in April and am very keen to see something of you. You will find me at my mother's, whose address is in the telephone book.

Sincerely yours,

Kroeber

[343]

{no address or date, but apparently  
New York, June 1922}

Dear Kroeber,

I believe this matter (I mean a parallel one) came up once before.<sup>1</sup> I don't know what attitude other contributors to the series have been taking, but I personally consider it quite beyond discussion that a contributor to a series such as this should (and without previous contract!) be expected to pay the Univ. Let me be the protesting goat, if you like.

Sincerely,

E.S.

---

1 Sapir apparently enclosed with this note a bill he had received for corrections to the proof of "The Fundamental Elements of Northern Yana" (Sapir 1922b). For the previous incident, see Letter 236.

[344]

University of California  
Berkeley, June 12th, 1922

Dear Sapir:

If the University Press has sent you a bill for proof corrections on your recent Yana paper, please keep your temper and forward the bill to me.

Sincerely yours,

Kroeber



[345]

University of California  
Berkeley, July 5th, 1922

Dear Sapir:

I shall be glad to receive the finished portion of your Yana Ethnology and have little doubt that I shall return it to you with the request to complete it.<sup>1</sup>

We are not yet publishing fast but will have picked up a good deal of slack in the next six months. If a new congestion and deficit do not pile up, we shall be able to continue to print.

The difference in status between this paper and the Algonkin relationship is that the present one is based on materials secured through University funds while you were in the University's service. It falls therefore in the same class as Paul Radin's Wappo Grammar which recently came to hand, and which, by the way, will be a valuable memoir once it has had the editing which it should have.<sup>2</sup>

I am glad to hear that Rivet has been able to take on your Algonkin manuscript.<sup>3</sup> My experience is that papers sooner or later get published. All except my California book apparently, and even that Fewkes says is in the printer's hands.<sup>4</sup>

There is nothing yet to report on Wiyot. Conditions among the group are not particularly favorable and it took Miss Reichard some time to make satisfactory connections among the scattered members of the group. She expects to bring an informant to Berkeley in a few days. We shall be able to say more than.<sup>5</sup>

Incidentally she turned up a couple of Mattoles whose speech forms one of the three or four primary units of Athabascan in California, according to Goddard. He had got very little of it however and no texts, and I think he had looked upon the language as lost. He was to be in northern California on his way back from British Columbia this summer and I am hoping he will rescue something from these people.<sup>6</sup>

Good luck to you with the Sarcee.<sup>7</sup> Is there any chance of your visiting us on a round of Athabaskan work? A joint visit by you and Goddard to the Pete tribe and the Sam tribe might be interesting for the rest of us after you both come home.<sup>8</sup>

Sincerely yours,

Kroeber

- 1 Kroeber is evidently replying to a letter from Sapir, not preserved, that was accompanied by the incomplete manuscript of "Notes on the Culture of the Yana".
- 2 Published in the UCPAAE series in 1929.
- 3 Paul Rivet was at this period the editor of the Journal de la Société des Américanistes de Paris.
- 4 Kroeber's Handbook of the Indians of California, which was completed in 1917 (see Letter 232), was published (as Bulletin 78 of the Bureau of American Ethnology) only in 1925. J. Walter Fewkes was at this time the Chief of the BAE.
- 5 Gladys A. Reichard (1893-1955), a student of Boas's, who spent 1922-23 as a Research Fellow at the University of California, working on Wiyot. Her Wiyot Grammar and Texts (1925) was her Columbia dissertation. After taking her doctorate she joined the faculty of Barnard College, where she spent the remainder of her career. Closely identified with Boas and Goddard, she worked extensively with both Salish (Coeur d'Alene) and Athabaskan (Navajo), and was one of the most productive students of American Indian languages of her generation.
- 6 Goddard obtained vocabularies in 1922 from several speakers of the Bear River dialect of Mattole (Goddard 1929). A fuller study of Mattole was carried out by Fang-Kuei Li, a student of Sapir's, in 1927.
- 7 Sapir was able to carry out in 1922 a somewhat abbreviated version of the field work on Sarcee that he had originally planned for 1921 (see Letter 332).
- 8 Goddard had the habit of referring to the California Athabaskan groups he had studied by the first names of his principal informants. See Kroeber 1967.

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University of California  
Berkeley, July 8, 1922

Dear Sapir:

I hope you will soon complete your notes on the culture of the Yana. I consider your manuscript the beginning of an unusually important paper. It carries an exactitude of detail such as is lacking from the great mass of what we are publishing, but which is invaluable and will in time be indispensable.

I do not remember whether I ever told you that some years ago when comparing the culture of northwest California and southwest Oregon, I found fully as much that I was able to use in your fragmentary Takelma notes as in all the rest of the literature put together because statements in the latter were generic and yours were intensive.<sup>1</sup>

Your Yana notes may attract little attention now, but they are the sort of material that will be referred to in a century as an original document of value when most of the other work we are putting out will have long been superseded.

I hope you will complete the paper as soon as you return, if you can.

Sincerely yours,  
Kroeber

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<sup>1</sup> See Letter 228.

[347]

Victoria Memorial Museum  
Ottawa, September 12, 1922

Dear Kroeber,

I have recently returned from the field and find that the unfinished paper on Yana ethnology which I sent you some time ago has not arrived. Would you mind returning it at your convenience, so that I may get at it as soon as I settle down to the winter's work?

I had a successful season at the Sarcee reserve and I am planning to put my texts into shape as soon as possible. I want also to write a preliminary paper on the tone-system of Sarcee before very long.<sup>1</sup>

Yours very sincerely,

E. Sapir

---

1 "Pitch Accent in Sarcee, an Athabaskan Language" (Sapir 1925a). The Sarcee texts were never published.

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University of California

Berkeley, September 22, 1922

Dear Sapir:--

I am sorry to have delayed returning your notes on the culture of the Yana. They go forward today under separate cover.

I wonder whether you have any suggestions as to getting De Angulo's Achomawi printed. He has just submitted to me a revised draft which removes some of the typographical difficulties of his first version, besides touching up several points for the better.

I should much like to see his manuscript issued. Dixon has promised Boas to prepare for publication his Shasta and Achomawi texts; which means, of course, that he must formulate the grammars.<sup>1</sup> He should of course be able to avail himself of De Angulo's work. On the other hand I do not feel authorized to give him free use of the latter's manuscript as long as there is no prospect of anything more happening to this than remaining locked up in a safe. De Angulo is generous enough, but this is his maiden effort and I know he would like to obtain a professional foothold in this country sometime. This hardly puts him in position to be giving away the results of his work without credit other than a mention. He has left me rather as his representative or executor.

You may be interested to know that I recently got Dixon to return to me an Atsugewi notebook which I filled in 1900. It is badly done; but I had better

informants than I realized at the time. I have gone over these old notes and find that the structure of the language is explained very well by De Angulo's sketch. He has not got everything, but I am confident that he has the leading features, and that at any rate the great majority of the interpretations are right.

Incidentally, the relation between the two languages is a very curious one. They are very close together in structure and enormously different in content. Phonetic equivalences are far from simple, and a surprising number of meanings are denoted by wholly different stems. The two groups cannot possibly understand each other's languages. They always say they can, but I fancy this means chiefly that most of them are bi-lingual.<sup>2</sup>

Goddard has been visiting us. He is in his characteristic ambivalent mood about your Sarcee invasion. He says that he did not get everything in Athabaskan and that you are doing it better, but is somewhat hurt and just a bit bellicose at being superseded. In the main, however, his attitude is more one of sadness. I think he would be quite affectionate if he could feel that you were not brushing him aside lightly or with scorn; but that sort of thing can scarcely be conveyed to him in writing.

Sincerely yours,

Kroeber

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1 Neither the grammars nor the texts were ever published.

2 This topic was taken up by David L. Olmsted several decades later in a paper on "Achumawi-Atsugewi Non-Reciprocal Intelligibility" (1954).

[349]

Ottawa, Ont.

Oct. 1, 1922

Dear Kroeber,

Thanks for the Yana MS, which I found on my desk today on my return from a two weeks' trip to N.Y. I intend to get at it in a couple of days or so and

hope to give you the completed MS some time in the near future. Let us hope that my promise is worth something this time!<sup>1</sup>

I hardly know what to suggest about de Angulo's MS. The logical place for it is your series. Aside from that only the International Journal of American Linguistics and the Journal de la Société des Américanistes de Paris suggest themselves as at all likely to care to handle it. Don't you think it might be worth while getting Paul's notes for comparison?<sup>2</sup> Paul talks of composition of elements in Achomawi comparable to what we find in Yana, but de Angulo's MS did not suggest any such analysis. Either he did not succeed in getting beneath the surface or Paul is making a mountain out of a hunch. The point is of real importance for Hoka. I have always wondered to what extent Yana was typical in its polysynthesis of Hoka generally. I do not feel secure about de Angulo's sounds. In a brief vocabulary I obtained at Montgomery Creek there were intermediates, aspirated surds, and glottalized surds (e.g. its'a "tooth"), but de Angulo does not seem to recognize this, if I remember rightly. I have a cheerless feeling that de Angulo's MS looks better than it really is, but, in any event, it is certainly valuable and worth publishing.

Your comparison of Achomawi and Atsugewi is somewhat parallel, I fancy, to that of Kwakiutl and Nootka, though in this case neither scientist nor Indian could even remotely imagine mutual intelligibility. If Paul is right about composition, direct comparison of words may yield rather little but possibly irreducible radical elements may yield more numerous analogies than appear at first.

I saw Goddard a couple of times in N.Y. He's a queer nut. Athabaskan is not his scientific interest but a kind of private mistress. He showed no desire to see my Sarcee stuff and acted very petulantly, I thought--as did Ogburn and Mrs. Benedict.<sup>3</sup> After all, it is symptomatic that he has actually never looked seriously into Morice, Petitot, Legoff, and the Franciscan Fathers.<sup>4</sup> At least he does not seem to know their material at all well. I have decided that his Athabaskan is very imperfectly sublimated libido and that it would be making polyandrous innuendos for me to attempt to discuss my stuff with him. At our Tuesday luncheon he burst out with emotion, "Sapir, I hear you've been telling everybody you found tone in Athabaskan. Well, my best

come-back is to say that in an old paper of mine, which I found the other day, I pointed out, on the basis of kymograph measurements, that in Hupa the 2nd per. singular has a higher tone on the pronominal syllable than the otherwise identical 3d per. But tone is not really important in Athabaskan. If I have erred, it is rather in quantity." This seemed intended to demolish any pretensions I might have. As far as the mere fact of tone is concerned, Father Legoff, long long ago, was as clear and flat-footed as one could be--only he gave very few facts. Goddard's particular observation is entirely correct for Sarcee as well, which is as it should be. As for tone not being important, that's a joke. It is twice as important as anything that Goddard has yet had to say about Athabaskan phonology. To put it bluntly, to attempt to study Sarcee without tone is like studying a language without paying any attention to its vowels but concentrating entirely on consonants. Goddard seemed to suggest that it was a case of tone or quantity. Nonsense. Both tone and quantity are fundamental, and independently so. Goddard simply failed to note either. Later on, I had a longer talk with him at the Museum. I did what I could to make him feel nice--discussing his obviously wretched Wailaki material and his recent Carrier notes with becoming respect and sympathy. When I timidly suggested that tone might be worth looking into, he got flushed and impatient and said, "It's of interest to you, because you're looking for it, but it does not interest me at all." He admitted later that he couldn't hear tone at all--it would be hopeless for him to attempt to note it. What I dislike is his constant attempt to rationalize away his own disabilities as though they did not matter, while he is fiendishly joyous of possible shortcomings in the work of others. Why is he so damned personal about it all? After all, nobody wants to have sexual relations with the Hupa language!

You talk of my "brushing him aside lightly or with scorn." I cannot control his own projections, but I have always been almost ridiculously careful not to antagonize him. His critique of my Nadene, for instance, was painful balderdash, but I refrained from answering.<sup>5</sup> He would hardly have refrained if positions were reversed and he had my very excellent trump-cards. But what's the use? I am very eager to avoid useless polemics. I hope that in the next 10 years my evidence will gradually pile up with crushing serenity.

Just at present I am worrying about getting my Sarcee Texts published. I hope to publish a preliminary paper soon on Sarcee tone. If all goes well, I am with you in San Francisco next summer, en route to Hupa Reservation; perhaps I shall take a phonograph for absolutely objective records, which Miss Roberts<sup>6</sup> or some one else can then work out. There are, naturally, some delicate problems and one's ear is apt to be fallacious. I suspect, for instance, that in Sarcee the true middle is not identical in pitch with the dropped high (a secondary middle due to a running-down cadence). It will be fascinating to be able to compare the tone structures of Sarcee and Hupa in detail.<sup>7</sup>

I was quite encouraged by my wife's progress. I believe there is definite improvement, though she is obviously not well yet. She is more alert and interested, very eager to return home, but somewhat paranoid, I am afraid. I doubt if manic-depressive depression describes her condition. I should not be surprised if she were suffering from a grave neurosis, overlaid by acute depression. But diagnosis is of only incidental interest. God grant that she recover soon.

Give my regards to Lowie, Gifford, and Miss Reichard. Tell Lowie I enjoyed his Chipewyan Windigo story very much. You will not be displeased with what I say about your Mohave story in a forthcoming Dial review.<sup>8</sup>

Sincerely,

E. Sapir.

- 
- 1 The worsening state of his wife's health prevented Sapir from carrying out this promise, and the manuscript remained unfinished until after Sapir's death, when it was completed by Leslie Spier (Sapir and Spier 1943).
  - 2 Paul Radin had worked on Achomawi briefly around 1919-20. See Letter 350.
  - 3 William F. Ogburn, the well-known sociologist (then at Columbia) and Ruth Benedict (1882-1948), who had recently completed her Ph.D. at Columbia and was at this time an Assistant to Boas.—For the biography of Benedict, and details of Sapir's relationship with her, see Mead 1959 and Modell 1983.
  - 4 Missionary priests who had written extensively on several Athabaskan languages. See Letter 319.
  - 5 Goddard 1920. See also Sapir's remarks above in Letter 332.
  - 6 Helen H. Roberts (1888- ), an ethnomusicologist.



- 7 Sapir was unable to work on Hupa until 1927, at which time he discovered (to his surprise) that it lacked tone (Sapir 1928a).
- 8 The stories by Lowie and Kroeber that are referred to were their respective contributions to American Indian Life (1922), a volume of fictionalized portraits of traditional Indian life that had been assembled (and subsidized) by Elsie Clews Parsons. Sapir himself contributed a sketch of "Sayach'apis, a Nootka Trader" (1922d). For the review see Sapir 1922e.

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University of California

Berkeley, October 17, 1922

Dear Sapir:

I look forward to your Yana Ethnology. The Press has just called for an estimate of manuscript likely to be submitted during the current fiscal year.

I had thought of Paul's Achomawi in connection with de Angulo. Paul has notes for sketches of Achomawi, Wintun, and Pomo. He was eager to do the Achomawi first. I am not sure. He went at the language from quite different angles and, I think, without text. He is remarkably quick, but the total number of hours he spent with his Indian was really quite small. I am afraid the two accounts would appear to clash and stultify each other more than would actually be the case. A third study would then be called for to explain the discrepancies between the two. To carry conviction, such a study would have to be definitely more intensive than both the others, and in that event they would be largely superseded. I feel shakey about de Angulo's work at the same point as you do. Yet, I cannot but think that the glottalized stops must be exceedingly weak if they occur. I wrote them constantly, but comparison shows without any consistency. I should not be surprised if it proved that an original glottalized series had become merged in an unglottalized one. I am rather more confident that an aspirated an unaspirated can be distinguished. On a number of points, de Angulo is certainly right--such as the w being unrounded.

He is also correct about lack of composition to the extent that the process is not evident in any simple and clear-cut fashion such as one gets in

Pomo, for instance. On the other hand, I had the feeling about his list of unresolvable nouns that many of these would ultimately break up. Still, I do not see a crevice for analysis, either in his data or mine. I should not trust Paul's statement very far in such a matter. Paul always loves a clue and is impatient of evidence; and I fancy his opinion rests chiefly on suggestive intuitions. These are likely to be sound, but equally likely to be unsubstantiable by the material in his possession. De Angulo rather leans the other way. Although he likes the finespun and acute, he has a passion for the clean, hard, neat, and orderly. His mind is really very much more French in this respect than he is aware of or would like to recognize. His tendency would be to eliminate all the hazy fringes of a language--to cut it off and throw it away. He cannot bear the uncertainty of half-vision, whereas Paul blinks sunshine and is happiest feeling his way through a fog.

I guess we certainly do need a third study some time. Perhaps a way out would be for me to do a little work with language, and then add a series of qualifying notes to both their versions.

I know that you cannot avoid getting under Goddard's skin as long as you touch Athabascan. All you can do to preserve relations is to convey to him a species of personal appreciation. I do not think he has ever been much interested in any work as such; certainly not deeply or other than fitfully. He is probably less interested now than he was fifteen years ago. If you can get across to him that you hold him in some kind of a definite liking and regard him as an individual, I am sure that this will at least compensate to him for any wrecking of his scientific plans. These really are not near his heart at all, but merely one of several means which he uses to buttress the ideal image of his personality toward which his affections are directed. He is a thoroughly narcissistic type, sublimating only on the surface. I realize that my program for maintaining relations with him is more easily outlined than executed, but I believe it is feasible.

Reichard has got some excellent texts of Wiyot and will get the language thoroughly cleared up.

Sincerely yours,

Kroeber

[P.S.] I am glad to hear of your wife's improvement.

[351]

[Berkeley]

November 29, 1922

Dear Sapir:

I am awfully sorry to hear from Lowie that your wife appears to be no better. I delayed answering your last letter<sup>1</sup> in the hope of an improvement. Also I did not know what to say to your suggestions.<sup>2</sup> I hardly know now. My plans for next year are quite unsettled. Lowie will be with us, I think. The Guthe appointment has perhaps not closed the Michigan prospect, as I was at first inclined to assume; but if so, I suspect they hesitate on anti-Semitic grounds.<sup>3</sup> We are recommending an instructorship to cover prehistory and physical anthropology, but more pro-forma because the subjects are untaught than with expectation of the University doing anything. A push for a man in linguistics would fail. The old-line philologists would feel assailed, and there is no demand from students. A combination with anthropology would mean that our departmental teaching was carried by one and a half men instead of two and a half. Our museum eats up enough money to make the University think we are pretty expensive already.

All in all, I see the situation much as I did last winter, when I said that your opening would come at one of the larger eastern institutions. My faith in that continues. If ever the chance should break here, we'll back you.

Let me know if there's anything I can do. And write if things turn a bit brighter.

Yours

A. L. Kroeber

- 1 Not preserved.
- 2 Presumably concerning an appointment at Berkeley.
- 3 Carl E. Guthe (1893-1974), a Harvard-trained archaeologist, was appointed Associate Director of the Museum of Anthropology at the University of Michigan in 1922. Kroeber implies that Lowie had also been negotiating for a position at Michigan, where prospects apparently seemed better than at California.

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Ottawa, Ont.

December 18, 1922

Dear Kroeber,

Is there the slightest use of my submitting "Language" for a second Loubat Prize<sup>1</sup> on the strength of its making liberal use of American linguistic evidence? If the idea is absurd, please tell me, for I should not like to make a fool of myself by applying.

I intend to leave for Cambridge shortly to read a paper on Sarcee.<sup>2</sup> Jenness will go too and will discuss the early history (inferential) of the Copper Eskimo.--I was a little puzzled by your reference to Yana on parent-in-law taboos.<sup>3</sup> Is not the evidence in my paper on Yana kinship clear on this point? See p. 172 (note 12) of vol. 13 of your series.<sup>4</sup> Or does your "Yana" not include "Yahi"? In any event, term no. 43 of N. Yana (p. 160) clearly points to taboo relationship.

Have been working pretty steady of late on Nootka and Sarcee linguistics. Both languages require endless filing of elements.

Mrs. Sapir is completely cured mentally, it seems. She has been corresponding very nicely with me and the children. The lung abcess continues, of course, to be a serious problem, but she seems to be holding her own fairly well. I hardly think it is localized enough as yet to make a drainage operation practical.<sup>5</sup>--How is Miss Reichard's Wiyot coming on?

Sincerely,

E. Sapir.

- 1 The Loubat Prizes (a first of \$1000, and a second of \$400) were awarded every five years by Columbia University for "the best work printed and published in the English language on the History, Geography, Archaeology, Ethnology, Philology, or Numismatics of North America" (American Anthropologist 24:392, 1922). Kroeber was a member of the jury for the 1923 awards.
- 2 The 1922 meeting of the American Anthropological Association was held in Boston and Cambridge, Dec. 27-29. The announced title of Sapir's paper was "Problems in Athabascan Morphology".
- 3 In his review of W. H. R. Rivers, Instinct and the Unconscious (Kroeber 1922b).
- 4 "Yana Terms of Relationship" (Sapir 1918).
- 5 Florence Sapir had been hospitalized in New York for several months. Following an operation to drain her lung abscess, she was able to return to Ottawa in the spring of 1923.

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University of California

Berkeley, December 28, 1922

Dear Sapir:

I have not at hand at the moment the specifications for the Loubat prize. My impression is that eligible works must deal with a specifically American subject. However, there will probably be works submitted that do not fall strictly into this category. For instance, Mitchell's Anthropology Up To Date has been entered. I do not see why you should not follow suit. Why not put the thing up to your publisher and have him make the submission?

Thanks for the correction on Yana parent-in-law taboo. I expect I took my data from Gifford's review in his kinship volume, which, by the way, is just out.<sup>1</sup> I will make use of your annotation in the Handbook of the Indians of California, of which I am now reading the proof for the Bureau--thank God.

I am awfully glad to hear of your wife's improvement. It begins to look as if the pressure of the obscure organic lesion might have resulted in the psychic symptoms. There have recently been claims of some extraordinary

operative cures of psychosis. If your wife's case falls in this class, you have everything to look forward to.

Sincerely yours,  
Kroeber

---

1 "California Kinship Terminologies" (Gifford 1922).

[354]

University of California  
Berkeley, May 11, 1923<sup>1</sup>

Dear Sapir:

Under separate cover I am mailing you today first and revised proofs of your "Text Analyses of Three Yana Dialects". Will you kindly verify all changes, entering any corrections on the revised proof, and returning both sets to us as promptly as possible?

Owing to linotype setting, it will be necessary to read the entire line wherever a change has been made.

Thanking you for a prompt return of these proofs, I am

Very truly yours,  
Kroeber

---

1 This is the only surviving letter exchanged between Sapir and Kroeber in 1923. Perhaps other letters of a more personal nature were destroyed. Whatever the case, Sapir was doubtless preoccupied with his own affairs in this unhappy year. With his wife's condition still worrisome, and he himself laid up with a broken leg, summer fieldwork in the West was once again out of the question. He was able to work for several weeks on Kutchin and Ingalik (Athabaskan languages of Alaska) with two boys at a summer camp in northeastern Pennsylvania. In the fall, Mrs. Sapir's health again took a turn for the worse. She died early in 1924.

[355]

[Mexico City]

March 3, 1924

Dear Sapir:

I am sorry to have been so long keeping your Subtiaba ms., which herewith.<sup>1</sup> My sudden departure the day after I saw you put it quite out of mind. I have read it here<sup>2</sup> with more care than I should have previously, and am convinced. Boas won't be--but then--

I think I told you that in Berlin in 1915, after talking with Lehmann,<sup>3</sup> I put in a few hours scrambling together data from the same area with reference to Hokan. I had precious little of the latter at hand, and did not follow up the indications that appeared. The notes have been in an envelope these years; I threw them in with my papers on coming on and last night got them out. Scant as the data are, they suggest strongly the Hokan affinity of Xinca, west of Lenca, and possibly of Chibcha. Looking over what I had on Zoque-Mixe, I find clues there. I send you the sheet on which I summarized in 1915, plus 1924 pencil additions. Poor as the showing is, I hope it will stimulate you to test out the indications. With Lehmann's big book,<sup>4</sup> it ought to be convenient. I'd like to do it myself, but have decided one either has to be a linguist or not--at my age, anyway; and I have enough faculty in the field to know my limits are such that I should never do anything in it without complete preoccupation.

Give me your reaction anyway; and if ever you are so inclined, follow the thing up. With Chontal and Subtiaba-Tlappanec definitely Hokan, there must be others in the area. And if ever Chibcha could be linked with a widespread N.A. group the liberalizing effect on opinion would be important.

I've twice looked at Maya; each time with some result of intuition of Hokan-Siouan connection but with nothing to lay my hands on.

Last year also I went over the stuff from various Zapotecan languages that de Angulo sent me. But these languages are so worn that you have to know them well even to connect them inter se.

There'll be little chance to collect Tlappanec this trip. Guerrero and Oaxaca are still unsubdued, and where there is sierra, bandits have a way of hanging on after revolution collapses. From what Gamio<sup>5</sup> has said, he apparently has no one to trouble himself with a job like this.

It looks as if it would be pleasant here and I'll absorb a lot, though I am beginning to think it was scarcely wise to hope to do a piece of work in so intricately rich a field in a short time.

Try kidding Reichard next time. I rather liked and much admired her. Her work capacity is enormous. The chief fault I found was the super-impregnation with Boas, so that she neither gave nor received anything in her year with us. What she had, was el puro Boas; and she wanted nothing else. She did her Wiyot the way he would approve; and no doubt her Christmas paper sprang from the same motive. She is hard and efficient and charmless--the opposite of Haeberlin; but equally saturated with the old man; and Haeberlin's successor, almost, in his devotion. She's neither quarrelsome nor dogmatic, but argument with her is useless because she had Boas lock her mind and keep the key.

I spent a long evening over Lehmann. He is learned; often acute; more often pedantic or dogmatic or both; and always unsystematic. He has an incoherent and unreliable mind. Deutsch.

A. L. K.

- 
- 1 The manuscript of "The Hokan Affinity of Subtiaba in Nicaragua" (Sapir 1925b).
  - 2 Kroeber had taken leave from Berkeley for the spring semester of 1924 and was in Mexico City to carry out archaeological work in the valley of Mexico (cf. Kroeber 1925b).
  - 3 Walter Lehmann, a German Americanist scholar.
  - 4 Zentral Amerika (1920).
  - 5 Manuel Gamio, custodian of antiquities in the Museo Nacional de Mexico.



[356]

Ottawa, Ont.

June 5, 1924

Dear Kroeber,

I believe you said you would be unable to attend the Toronto meeting. But you may be interested in this abstract of McDougall's nevertheless.<sup>1</sup> Some time ago I ventured to mention your name to Fallaize, Recorder of Section H, as that of one of 4 Americans and Canadians who might take part in the race symposium in Sections H and J, the others being Wissler, Goldenweiser, and myself. Perhaps you would be able after all to come to Toronto and take part in the discussion.

I have read the first 4 chapters of your "Anthropology"<sup>2</sup> and, if all of it is as good as they are, your book is excellent and far away the best text on the subject. I may write you more at length on it later. I have no real criticisms. Personally I should prefer to see you avoid awkward words like "civilizational" and "eventuate", but I don't know that they matter particularly. I happened to glance at your chapter on Language and to note that death, Tod, and thanatos are grouped together. This comparison is erroneous and should be deleted in a second edition.

A popular paper on "Racial Superiority" is just about to appear in "The Menorah Journal".<sup>3</sup> I have asked the editors to send you a copy. If you don't get one, would you mind letting me know? In it I have attempted in a rather loose and chatty way to make the same points, more or less, that you drive home so lucidly and compactly in Chapter IV of your book.

Sincerely,

E. Sapir.

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1 The British Association for the Advancement of Science met in Toronto, August 7-13, 1924. William McDougall, a Canadian psychologist, organized a special joint session of Sections H (Anthropology) and J (Psychology) to discuss "Racial Mental Differences," and gave the introductory paper. Sapir did not give a paper, but took an active part in the discussion.

2 The first edition of Kroeber's general textbook (Kroeber 1923).

- 3 Sapir 1924a. This was the first in a series of papers on race and race relations that Sapir published between 1924 and 1927.

[357]

Ottawa, Ont.

June 15, 1924

Dear Kroeber,

I have just finished your "Anthropology" and want to say again how much I like it. I don't believe I have ever read an anthropological book that has given me as much pleasure and I am sure it will have a very great success. It stands out because it tacitly eliminates a great deal of that fruitless and cumbersome discussion about social origins and developments that used to be considered peculiarly the province of anthropology and because it not only makes the gesture of anthropology's being a historical discipline but actually ties up the data of anthropology with those of history. So much so that some people will ask, "What after all is anthropology as distinct from culture history?" and your implicit answer is, of course, that there is really no difference between the two disciplines except that anthropology asks a few more fundamental questions about the relation of culture to heredity.

I should like to make a few specific remarks about particular sections or passages. Perhaps some of these remarks may serve as a hint or two in the preparation of the second edition.

Order: Chap. VI has a rather weak position, it seems to me. Should it not come just before Chap. XIII? Chapters VI, XIII, XIV, and XV together make a digest of culture history. All the rest may be considered as General Principles and as Illustrations of Method.

Chapter V: A little diffuse, it seems to me. Section 62 does not quite come to its own in its present position. Could it not be transferred to one of the other chapters or, better, be worked up into a concluding chapter with other points of general interest?

P. 93: Grimm's Law is presented too schematically. Grimm's cycle is really erroneous if taken strictly. Above all, Greek th, ph, kh are not

fricatives, but aspirated surds. They only became fricatives in post-classical times.

Map facing p. 94: "perhaps two families" under Caucasian (why not "Caucasic" to distinguish from "Caucasian" as race term?) is antiquated. It is well understood today that S. Caucasasic belongs with N. Caucasasic. It might even be wise to squint at the synthesis Caucasasic-Asia Minor (Lydian, Lycian, and others)-Etruscan-Basque or "Japhetic", which is emerging. The Caucasasic-Basque similarities are by no means to be laughed away.--"Kolamian" is dropping out of use, I believe. Better "Munda" (reserving "Muṇḍārī" for the specific language).--Khasi, Sakai, Semang, Mon-Khmer, Munda, and Malayo-Polynesian should be accepted even in an elementary book as belonging together. I doubt if Samoan differs as much from Khasi or Muṇḍā as Karok does from Yuman. At the very least, Khasi, Munda, and Mon-Khmer should be grouped together. If Khasi is cut loose, why not also Nicobarese and Wa-Riang-Palaung?--Anamese is not an independent "stock" because most of its elements recur in either Sinitic or Mon-Khmer. It is a matter of judgment whether it should be grouped with one or the other. Mon-Khmer strongly overlaid by T'ai (later by Chinese) seems the most plausible way of settling the problem.--Korean and Japanese, I understand, have been demonstrated as diverging from one stem by a recent Japanese student. Japanese-Korean as an outlier of Ural-Altalic seems decidedly probable.--You have omitted Gilyak, which should be given as parallel to Yukaghir and Ainu. The possibility of Chukchi-Kamchadal belonging with Eskimo-Aleut might well be hinted at.--My suggestion for the map and its legend would be about as follows:

1. Basque (related to Caucasasic?)
2. Indo-European
3. Caucasasic (related to Basque?)
4. Ural-Altalic
5. Semitic
6. Dravidian
7. Austro-Asiatic (related to Malayo-Polynesian?)
  - a. Munda
  - b. Khasi

- c. Mon-Khmer; Palaung group
  - d. Nicobarese
  - e. Sakai; Semang
  - f. Anamese (?)
8. Sinitic
  9. Andaman
  10. Malayo-Polynesian (related to Austro-Asiatic?)
  11. Korean-Japanese (related to Ural-Altai?)
  12. Ainu
  13. Yeniseian
  14. Yukaghir
  15. Gilyak
  16. Chukchi-Kamchadal (related to Eskimo?)
  17. Eskimo

P. 95: Sinitic not well grouped. Better:

- a. E. Sinitic (Chinese; T'ai)
- b. W. Sinitic (= Tibeto-Burman)
- c. Karen
- d. Moso-Lolo-Sihia (outlier of Tibeto-Burman?)

P. 96: Finno-Ugric, Samoyed, and Altaic are not coordinate. It should be Finno-Ugric--Samoyed and Altaic.

P. 96, Anamese: No one today seriously considers it "an offshoot from Chinese". It has many Chinese loan-words and a well preserved Sino-Anamese tradition (i.e. Chinese as pronounced in Anamese tradition), but its Sinitic basis is T'ai, not Chinese. Its tone system is a replica of the old T'ai system (see H. Maspéro). The alternative is not "an offshoot from Chinese" or "a separate stock" but Mon-Khmer (better "Austro-Asiatic") or Sinitic (T'ai branch?).

P. 119: Gender in English is also found in "his, her, its". And note that nouns are classified insofar as they are referred to by he, she, and it. This matter of classification by reference instead of designation is of some theoretical importance.

P. 120, Gender in N. America: Add Tunica and Iroquois.

P. 120, middle ¶: Semitic and Hamitic certainly belong together. This should not be seriously doubted even in an elementary book. Hottentot is probably an old Hamitic offshot. The evidence is not bad at all.

P. 220: amaverunt is "they have loved" not "they will have loved", which is amaverint.

P. 253: N. Pacific Coast tribes, such as Nootka, Kwakiutl, Tsimshian, all use four. Five only in S. periphery (Washington-Oregon).

P. 256: You confuse two Germanic series of gods. Woden is Anglo-Saxon, Thor is Icelandic (corresponds to Þunor in Anglo-S.).

P. 271: "the consistent employment... and end of words" is unclear. Do you mean "glottal stops" by "stops"? If "stops" means explosives, your statement is incorrect.

P. 271, Ths wy 't: It took me a long time to make this out! It is not in the Semitic spirit, which would have required Ths wy 'wt. And Th as one sound is disturbing and obscures your analogy. Select a better example.

P. 272: "The novel step of adding vowel letters" is not strictly correct, for ', y, w, and h had been frequently employed as vowel signs on Semitic soil. It is far more likely that the Greeks borrowed the Semitic device of using consonantal signs for vowels and merely applied the principle more completely and exclusively.

P. 272, l. 11: "and stops" is again unclear.

P. 272, l. 21: I believe coördinate, with glottal stop is not as common as you think. It is a personal peculiarity of your family's, the only faint trace of German phonetics which your speech betrays. You also tend to keep initial vowels syllabically distinct from preceding final consonants. This too is a German carry over. A better example would perhaps be at all as affectedly pronounced.

P. 274, "was of no great moment in Latin": A bad error! Vocalic quantity is at least as important in Latin as in Greek. Cf. venī "come!" and vēnī "I came". In fact in many Greek inscriptions one sign was used for e and ē, one for o and ō, and note that a : ā, i : ī, and u : ū were never differentiated as characters. Historically quantity in Latin proved very much more important than in Greek, because in modern Greek o, ō and i, ī and u, ū and

a, ā have fallen together as o, i, ī, and a, only e, ē being kept apart as e, ī, while Romance carefully distinguishes reflexes of long and short vowels, e.g. French roi < rēge but lire < lēgere, Italian fedē (close e in first syllable) < fidēs but fine < fīnis. What the behavior of Latin and Greek towards alphabetic symbols really shows is how patiently languages will jog along with glaringly inadequate symbols rather than invent outright.

P. 286: "Hebrew and Arabic... these points" needs to be considerably toned down. How much at home consonantal designation of long vowels is in Hebrew can be seen from examples like gālā "he has released", which is written g l h, where -h stands for -ā though h is not etymologically justified, for -y has here disappeared (cf. other forms like gālī-θī "I have released"). In other words h had become a purely conventional method of writing -ā, so much so that the Massoretic pointing had to introduce a "mappiq", a point, in all final h's that were meant to be actually pronounced as such. Contrast 'iššā "woman" (written 'š h, Massoretic 'š· h) and 'iššāh "her husband" (also written 'š h, Massoretic 'š· h' ). Of course your main point about vowel indication is correct, but you must be careful not to credit the Greeks with anything new. Historically there are two Semitic vowel systems--an imperfect old one in which the consonants were turned to vocalic uses and a more refined later one in which diacritical marks were added.

P. 288: But note how beautifully the Dēvanāgarī system suits Tibetan! Every unit character-group (with vowel and preceding or following consonants) forms a word or significant element. Technically the alphabet looks made for Tibetan and clumsily adapted for Sanskrit! Is it possible that the Sabaeen system was taken up in E. Turkestan by some now extinct Sinitic language before it reached Indo-Aryan soil? In that case Tibetan would be a reversion to prototype.

P. 293-325: I wonder if enough concrete material is given in this chapter to make it mean quite enough to the layman or young student. Too heavy a burden of theory for the modicum of fact, I feel.

P. 295, l. 4: "priest" is out of place.

P. 303: "But it has not spread... Arizona" is not strictly correct. I have full data for the mourning ceremony among the S. Paiute of S.W. Utah (Kaibab and neighboring bands).

Chapter XIV: Splendid!

P. 459: Ligurians and Sicilians are really obscure as to affiliation, but Scythians are well known to have been Iranian, though this term was sometimes used more loosely, like our "Siwash" or "Digger".

P. 471, last ¶ of Section 255: Japanese seems to belong to Korean and both, more likely than not, to Ural- Altaic.

P. 474, 2nd ¶: Samoyed belongs with Finno-Ugric.

P. 475: Gilyak is omitted from your list of Paleo-Asiatic peoples.

P. 471-476: In this section something should have been said of recent discoveries in Chinese Turkestan. You nowhere mention Tokharian.

P. 477: Change "Kolarians" to "Mundas".

P. 477, "The Kolarians... Dravidian offshoot": You should not be so cautious about Munda relationships. Dravidian has been more than "tentatively" connected with both Andaman and Australian. The Dravidian-Australian evidence, if I may judge from Trombetti's "Elementi di Glottologia", is not altogether negligible, either. Brahui is Dravidian; "appears to be" is out of place.

P. 479: "Avestan or Old Persian" is misleading, for these are two distinct, though closely related, languages.

P. 480-484: Splendid!

P. 485: "perhaps distantly connected" should be, at the least, "probably distantly connected".--For Anamese, you again overemphasize the quite secondary Chinese element at the expense of the far more fundamental T'ai and Mon-Khmer elements.--To say "probably collateral offshoots" is ever too conservative. Anybody who has handled T'ai, Chinese, and Tibetan material knows perfectly well these 3 sets of languages are quite intimately related. For Chinese and Tib. take for instance: Archaic Chin. ŋá "I" : Tib. ŋa, Arch. Chinese ŋá "five" : Tib. l-ŋa.

P. 487: In speaking of Chinese influences in Anam you leave out two of the most striking points: 1, Anamese writing is based on Chinese; 2, Chinese

is preserved intact, tones and all, in an independent Sino-Anamese tradition, which is of capital importance for Sinologists in reconstructing Ancient Chinese phonology and which does not depend in the least for its maintenance on contemporary Chinese learning. Similar foreign "dialects" of Chinese are Sino-Korean and 2 distinct Sino-Japanese traditions, Kan-on and Go-on, with different readings for Chinese characters. By the way, in speaking of the cultural influence of China, you might refer to these 4 massive Chinese traditions outside of Chinese territory. Nothing better clinches the nature of this influence.

P. 492: You cannot say quite so definitely that the Australian race is distinctive. It is not far from the proto-Dravidian, Veddah, Sakai, is it? And linguistically too connection may be established between Australian and Dravidian. Trombetti<sup>1</sup> claims Dravidian, Andaman, Papuan, and Australian clearly belong together.

Pp. 496-503: The weakest and least well organized pages in the book. Africa deserves more careful consideration than you give it.

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These small criticisms amount to nothing. A splendid book--full of ideas and well-chosen facts, fruitful, integrating finely with other interests than "anthropology".

Sincerely,

E. Sapir.

---

1 A. Trombetti, Elementi di Glottologia (1922). See previous page, and also Letter 359.



[358]

University of California  
Berkeley, June 20, 1924

Dear Sapir:

I am sorry, but Toronto is out of the question for me.

I have read McDougall's outline with interest. I find him a master at loose reasoning.

Thanks for calling my attention to the break in my book in connection with the words for death. If ever there is a second edition I will delete. Sometime in your leisure let me have the straight of the matter please.

I am particularly pleased at your gentle remarks on my chapter on language. I should have guessed that my painting in with a broad brush would have rasped your finer sensibilities more.

Sincerely yours,  
Kroeber

[359]

Ottawa, Ont.  
June 22, 1924

Dear Kroeber,

In looking over some Yana notes I find that in a list of Yana Specimens collected by me and drawn up by your museum you entered:

1/13455 Original no. 19. Seed necklace (wák'u).

My notes indicate shell, not seed, and this seems confirmed by print submitted (photograph no. 15/4786). Could you have this looked up for me to make absolutely certain? And perhaps you can get the shell identified for me.

You may be interested in the following from Trombetti's "Elementi di Glottologia" (p. 420):

Australian 69 birra "war-spear"

13 weera "war-spear"

(nos. refer to Curr)

Papuan 16 bira "arrow"

10 wera "arrow"

(nos. refer to Ray)

If these forms are connected, does it mean that the Australians originally had the bow and arrow and then, substituting the spear for it, used the old word for "arrow"? There seems excellent evidence, by the way, for Papuan-Australian relationship, but perhaps this example is suspect because of its cultural character. Trombetti recognizes a "Dravidian-Australian" group which consists of Dravidian, Andamanese, Papuan, and Australian. But perhaps you may have little confidence in Trombetti when he continues:

Masai en-a-bere "lance" (en- is article)

Yoruba a-bere "ago" (cannot look up Italian "ago" just now).

There is much excellent material and good sense in Trombetti in spite of his being a frenzied monogenist. I am not so sure that his standpoint is less sound than the usual "conservative" one. By the way, the Munda, Khasi, Mon-Khmer, Nicobarese, Malayo-Polynesian resemblances are so striking and so abundant that there is no reason for your conservative mapping in "Anthropology". Do change this in the second edition. You would be perfectly safe.

Sincerely,

E. Sapir.

[360]

[Berkeley]

July 3, 1924

Dear Sapir

Yana 1-13455 is shells--univalves--*Olivella biplicata*. The error must have been in copying from the catalogue.

I appreciate very much your long letter about my book, and hope to profit if they reprint.

I'm between trips just now--back from Patwin ethnology and off in a couple of days for northern coast.

Yours

A L K

[361]

Ottawa, Ont.

Oct. 27, 1924

Dear Kroeber,

Re "Hokan-Siouan":

How about

Wappo mēi "water" : Washo d-ime

me-l "stream" : Esselen imi-la

Yuki mee-l, mī-l "leg" : Hokan \*ime (\*imi)

Yuki mī-t, me-t, mi'-t "sky" : Hokan \*i'ma- (\*a'ma-),  
Esselen imi-ta

? Does not Wappo me-l "stream" : Esselen imila = Yuki me-t "sky" : Esselen imita rather take your fancy? There is clearly an old nominal absolutive suffix -l in Yuki, just as there is in Hokan (Pomo, Washo, Yuman, Esselen, Subtiaba, Chimariko). I have not too bad evidence to show Yuki belongs with Hokan--whether with Hokan proper or some larger group like "Hokan-Coahuiltecan" or "Hokan-Siouan" is hard to say. Yuki seems to have suffered profound phonetic disintegration, so that lexical resemblances are badly obscured.

You will be interested to know that a look into Natchez-Muskogian revealed the fact that we have here a set of petrified adjectival, nominal, and verbal "class" prefixes strangely suggestive of similar prefixes in Hokan (including Coahuiltecan and Subtiaba). Thus, Choctaw la<sup>n</sup>sa "scar" : mi<sup>n</sup>sa "scarred" shows nominal l- (cf. Chontal?) : adjectival m- (cf. Yana, Pomo,

Subtiaba, Salinan); it is a type of alternation analogous to Subtiaba d-aca "grass" : maca "to be green". I do think there's something here.

The Subtiaba paper<sup>1</sup> has turned out longer and more substantial than I planned, and I am not including my Muskogian and Yuki asides. But I could not help going pretty freely into the question of class prefixes for Hokan generally and I have 2 interesting tables showing distribution of these elements. The evidence is often sparse but wonderfully suggestive. I was going to send the paper to Gifford<sup>2</sup> for two Anthropologist instalments, but it looks too long and technical for that. Could you not use it for your series? It is far more "grundlegend" for Hokan than my "Position of Yana".<sup>3</sup>

Sincerely,

E. Sapir.

- 
- 1 "The Hokan Affinity of Subtiaba in Nicaragua" (Sapir (1925b).
  - 2 E. W. Gifford was Associate Editor of the American Anthropologist at this time.
  - 3 Sapir 1917a. The Subtiaba paper was published in the American Anthropologist, as indicated, in two 30-page segments.

[362]

Victoria Memorial Museum

Aug. 21, 1925

Dear Kroeber,

Under another cover I am sending you some N. Yana sheets and cards, some C. Yana cards, and some Yahi cards. These are all entries that I have transferred to my Yana books (First-position Elements), so they are merely duplicates and there is no use my dragging them to Chicago. Perhaps you would not mind putting them away somewhere for safe-keeping as a mass of source material to fall back on in case something happens to my transfer. These sheets and cards are by no means complete records of first-position elements--merely transferred stuff that I happen to have in this duplicate form. Many of the old N. Yana sheets I pasted in my books; the Yahi elements have only been

partly transferred. Then there are some N. Yana elements on cards still to be transferred and a great deal of further N., C., and Yahi still in my notebooks. Second-position elements and grammatical elements to be taken up later.<sup>1</sup>

I quite enjoyed the summer work and had good classes.<sup>2</sup> I expect to leave for Chicago about Sept. 23d.<sup>3</sup>

How have you been? And what plans have you? Give my regards to Lowie. I hope to write to him shortly, but at the moment I am full up in immigration matters and details connected with leaving.

How did you find Margaret Mead? Did she seem nervous about going to Samoa?<sup>4</sup>

Sincerely,

E. Sapir.

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1 These materials are now in the Library of the American Philosophical Society, Philadelphia (MS 3913).

2 Sapir had taught in the summer school at Columbia this year.

3 Sapir had been appointed to an Associate Professorship at the University of Chicago.

4 Margaret Mead, at this time a doctoral student at Columbia, had left on her first field trip to American Samoa earlier in August. She sailed from San Francisco, and doubtless visited Kroeber. Sapir, who had met Mead through Ruth Benedict, feared that she was not up to the rigors of a prolonged stay in the field, and had corresponded with Boas and Benedict about this (Mead 1959:290-1).

[363]

University of California  
Berkeley, September 1, 1925

Dear Sapir:

Your three packages of Yana cards have come and we will store them in the Historic mothproof room in this building.

We are going along as usual except for a perceptible falling off in undergraduate courses and an increase in the number of graduate students.

There are several things I have been wanting to write you about, after I get settled in the year's work. You may hear from me about the time you get established in Chicago.

Sincerely yours,

Kroeber

## APPENDIX I.

## A.

Report on two preliminary meetings of Committee on  
Phonetic Transcription of Indian Languages, held January, 1913  
in New York

In order to expedite matters and afford a basis for further discussion a quorum of the committee of the American Anthropological Association, charged with the drawing up of a phonetic system for transcribing Indian languages, consisting of F. Boas, as chairman, P. E. Goddard, and E. Sapir, met early in January at Dr. Boas' house, Grantwood, N. J., and decided upon certain preliminary phonetic recommendations. All of these are naturally subject to further discussion and to revision. In the short time at the disposal of the committee it could only take up a few matters, and thus many important points are not touched upon in these recommendations. The points discussed are as follows, the Secretary venturing to suggest on his own account here and there, as indicated, by way of supplementary discussion:

I. In studying culture languages such as Nahuatl or Maya, it is recommended that use be made in writing texts and quoting forms for comparison of those methods of writing which are already in use in more or less standardized form. Where systems already in use do not seem sufficient for purposes of accuracy, it is recommended that refinements be made on the basis of such systems, rather than that entirely new methods of transcription be constructed. Dr. Boas pointed out that this move may do much to gain for the committee the valuable support of several Americanists accustomed to certain orthographies, that might be repelled by entirely new methods of transcribing such languages as Nahuatl. Where discussion of such culture languages, however, is avowedly devoted to phonetic matters as such, it goes without saying that the more strictly phonetic system recommended by the committee is to be used. In recording dialects of culture languages it would also seem preferable, on the whole, to use a strictly phonetic system.

II. In writing texts all characters or marks of punctuation that have not a strictly phonetic value should, as far as feasible, be ruled out, in order to avoid distracting the eye with symbols of non-phonetic significance. Thus, no capitals should be used for beginning sentences. Similarly, marks of punctuation should be entirely eliminated or used sparingly at best. Where interlinear translations accompany texts it is easy to do without such marks of punctuation, the punctuation in the translation giving the necessary data for division of sentences. Where continuous text is given without interlinear translation, it will probably be found necessary to use periods and commas.

III. It is recommended, as a general principle, that mixture of fonts be avoided. All italic letters are to be eliminated. Italics may be reserved for calling attention to particular words, syllables, or letters in grammatical discussions or otherwise. Inverted letters should be avoided, if possible. Small capitals and Greek letters had better be avoided, if possible.

(In the Secretary's opinion, it seems ill advised to abandon the use of Greek characters. Accumulation of diacritical marks are sure to result, which could, in great measure, be avoided by the adoption of Greek characters. Owing to the fact that comparatively few languages would require the use of many Greek characters, the actual forms of the words would not be very greatly marred by the mixture of Roman and Greek characters. Further discussion is invited on this point. Small caps also, it would seem, would eventually be difficult to avoid. E. S.)

IV. It is urgently recommended, as a general principle, that the simplest available character be employed for a given sound. Distinctions will have to be made in a general phonetic system that need not apply to particular languages. Thus, it will be necessary to distinguish in a general phonetic system between close and open o. Where a language, however, possesses only one of these sounds, it is recommended that use be made in either case of the simple symbol o. Where several closely related languages or dialects differ on points of just this character, it may seem preferable to adhere strictly to the phonetic system, as it might be inconvenient to explain that the symbol, say o, takes on a new meaning in passing from one language or dialect to another. In these matters, much must be left to the judgment of the investigator. It is obvious, however, that strict adherence to an absolute phonetic system would eventually mean the use in certain languages of extremely cumbersome systems for relatively simple phonetic systems. It can not be too strongly insisted that a phonetic key be appended to every series of texts or grammatical or other works involving the use of phonetic symbols. Where a simpler character is used, in accordance with the principle just laid down, than the proper phonetic symbol of the full alphabet, it should be explicitly defined, reference being made, if possible, to its equivalent in the complete alphabet.

V. It is recommended that the use of characters that do not constitute a complete sound in themselves be avoided. On this principle, the superior n of the Bureau alphabet, employed to denote nasalization of preceding vowel, is to be discarded. All qualitative characteristics of a sound should be indicated in such a way that a sound with all its phonetic characteristics, insofar as it seems advisable to express them at all, be represented as a unified character. This means that all purely qualitative elements be expressed by the letter itself and by diacritical marks above and below the letter. On the other hand, purely dynamic elements, such as main and secondary stress accent and quality, may be expressed by conventional symbols following the letter.



VI. According to this principle, open and short qualities of vowels, nasalizations, intermediate quality of consonants, and such other factors should be expressed in the unified sound-symbol. Opinions may differ as to whether or not pitch accent is to be considered an integral element of a vowel or voiced continuant. Much naturally depends on the particular language. On the whole, however, it seems best to consider pitch accent, where significant, as an integral part of the sound represented.

VII. As a sign for long vowel or consonant, it is recommended that the inverted period (˘) be used after a letter. This symbol has already been used in Americanistic work by Thalbitzer, and to a less extent by J. P. Harrington and Sapir. It has several obvious advantages:- It avoids the irksome necessity of duplicating the whole set of possible characters to express long vowels and consonants corresponding to the short vowels and consonants expressed by the stock of symbols, and it thus saves considerable expense; it is a rather neat looking symbol which does not sprawl the word to the eye as much as it really does in fact; and it affords a simple means for distinguishing long from short consonants, at the same time serving as a common symbol for length in both vowels and consonants. For more than ordinary length a colon (: ) may be used after the letter. Thus, a˘ would denote long a, a: would denote excessively long a. Excessive length of non-grammatical significance, such as is often made use of for rhetorical purposes, may be expressed by +. Characters without explicit signs of length are to be considered as short. No provision has been made for vowels of excessive shortness. The use of the customary breve (˘) over letters would not be in harmony with the principle of denoting dynamic elements stated above. Discussion is invited as to the most practical method of representing excessive shortness.

VIII. Main and secondary stress accents are to be indicated by acute (´) and grave (`) respectively, which are to come after the vowel affected.

(This would involve the frequent use of ˘ after letters, with unsightly sprawling of words. Perhaps no single phonetic usage has been so firmly established as the placing of stress accents directly over vowels and, in the Secretary's opinion, it would be a decided mistake to depart from this usage. Even the use of the acute or grave alone without the preceding sign of length seems to him to involve too great sprawling of words. He would recommend that this point be left entirely to local option. As long as the use of acute and grave accents for purposes of stress is determined, it is relatively unimportant, theoretically speaking, whether the symbol come over or after the vowel. If, for the sake of more pleasing appearance and adherence to well established usage, it seems preferable to cast a set of vowels with stresses above, full liberty should be given to do so. Where printing facilities are more restricted, accents could be placed after the vowel. Thus, long accented a might be written either á˘ or a˘. E. S.)

IX. Pitch accent, where indicated at all, should be expressed by means of diacritical marks over the vowel. Where it is decided to indicate both pitch accent and stress, it is recommended that the stress be indicated by an acute or grave following the vowel, while the same characters over the vowel would indicate pitches. The system of representing pitch here recommended is the same as has been fully described by Father Schmidt in various articles in *Anthropos*. It is briefly as follows:- High pitch would be indicated by acute accent over vowel; low pitch to be indicated by grave accent over vowel; falling accent from high to low to be indicated by a combination of acute (´) and grave (`), i.e., by circumflex accent (^); rising accent from low to high to be indicated by combination of grave and acute accent, i.e., by inverted circumflex accent (˘); middle pitch to be indicated by vertical line above vowel (ˊ); rising accents from low to middle and from middle to high, and falling accents from high to middle and middle to low, can be indicated by appropriate combinations of signs for high, low, and middle accents (∨, √, ∟, ⊘). In actual usage it will not ordinarily be found necessary to indicate so many pitches, and the system can be considerably simplified. Thus middle pitch can generally be understood to be implied when no pitch accent is given. The use of accent over vowels for pitches and after vowels for stresses solves the problem of possible confusion between pitch and stress accents.

(As the majority of languages make no use of pitch accents the use of stress accents over vowels recommended above by the Secretary would not seriously interfere with the system of pitch representation here described. In any event, careful explanation of symbols in particular cases would avoid all misunderstanding. E. S.)

X. Nasalization should be expressed by means of a hook placed under the vowel or consonant. Thus, nasalized a is indicated by ą. This usage is well established in many quarters and should meet with ready acceptance. It seems preferable to the far clumsier sign ~ which also enjoys considerable usage.

(Where it seems necessary to distinguish weak from strong nasalization, it may be useful to employ ą for the former and ~ for the latter. E. S.)

XI. Perhaps the most difficult single problem to contend with is the most effective representation of vowels, particularly as regards differentiation of open and close vowels. If it is decided to adopt diacritical marks placed under the vowel for either the close or open series, it would be advisable to use either a point below the vowel as ọ for the close vowel or a line below the vowel (as օ) for the open vowel, the unmarked vowel being understood to be respectively open or close. The committee did not come to any definite decision on this point, though the consensus of opinion seemed to be in favor of the line below the vowel to indicate open quality, undesignated vowels being understood to be close.

(As between the point and the line, the Secretary prefers the point for close vowels, leaving the undesignated vowel to be understood as open. One difficulty with the line below is that

it may lead to confusion with the custom of underscoring to indicate italics. However, he believes that this whole method of indicating vowels is decidedly amateurish and sure, in the long run, to lead to awkward heaping of diacritical marks. If anywhere, Greek characters would be justifiable here. If we permit the use of Greek characters the problem is solved at once, the Roman letters being used for close vowels and Greek letters for corresponding open ones. The principle of simplification of usage stated above (see IV.) would in the great majority of cases considerably restrict the use of Greek characters. In place of Greek omikron, which would be difficult to distinguish from ordinary o, the Secretary recommends that inverted c (Ϸ) be used for open o. This violates the taboo against inverted letters, but the character has been so much used and is besides so inoffensive looking, that an exception might well be made in its favor. The system suggested by the Secretary works out practically as follows:

<u>Close</u>	<u>Open</u>
e	ε
o	ο
i	ι
u	υ
ø (as in French 'bleu')	Ϸ (as in French 'fleur')
ü (as in German 'Mühe')	Ϸ̄ (as in German 'Mütze')

Thus are disposed of twelve vowels, distinct as regards quality. By extending the system of Greek characters and umlauts, provision could easily be made for still other vowels which it is necessary to distinguish. Thus ĩ is available for high-back-narrow-unrounded (Sweet's terminology), the sound occurring in Shoshonean and Irish; ı̄ could then be used for the corresponding open vowel. The Secretary has heretofore indicated this latter vowel as small capital u. Other characters available are ȧ, ȡ, Ȣ, ȣ, Ȥ, ω, Ȧ, possibly also η and ħ, though these would perhaps be better avoided, inasmuch as η suggests n and has often been used for nasal consonants. Discussion is particularly invited on this matter of indication of vowels, which the Secretary feels would be perhaps the most important single point involved. For certain vowels of intermediate quality, single points could be used above vowels, graphically suggesting vowels acoustically midway between unlauded and unumlauded vowels. Thus ı̇ could be used to indicate Sweet's high-mixed narrow-rounded, as in Swedish hus. Whatever system be finally adopted, he feels that the use of separate characters for at least short and open e and o would be highly advisable. E. S.)

XII. What might be rather vaguely termed subsidiary or weakly articulated vowels of various sorts are best expressed by means of superior characters. Rearticulations (such as often occur in Indian languages, e.g.,  $\bar{a}^a$  in Takelma), vocalic glides, murmured or echo vowels pronounced with feeble energy yet not entirely voiceless (such as often occur in America after glottal stops), voiceless vowels and vocalic resonances of preceding consonants, and whispered vowels may all be expressed by superior vowels. It is difficult to devise a system allowing for all possible types of weak vowels and vowel resonances without becoming pedantic. The exact usage of superior vowels should be carefully explained in the key in every case, so as to avoid possible confusion.

(Secretary: The use of the breathing sign (‘) before superior vowels to indicate voiceless vowels or, if one prefers to term them such, aspirations with definite vocalic timbres, is recommended by way of distinguishing from true whispered, murmured, and glide vowels. Where a vowel of the types here discussed is nasalized, the superior character may be provided with the hook indicating nasalization, though special circumstances may make simplification of orthography seem advisable. The Secretary would like to have suggestions as to how to indicate nasalized breath which has no definite vocalic timbre; perhaps some conventional character based on the sign for breathing and on the hook for nasalization could be invented. Barely articulated consonants or weak consonantal glides are also best indicated by superior characters. E. S.)

XIII. Hyphens should not be used for phonetic purposes. To indicate that two vowels, ordinarily forming a diphthong, are to be separately pronounced, it is recommended to use a period between the two. Thus, instead of a-i, use a.i. Hyphens may, however, be used to indicate morphological analysis. Where in continuous text it seems advisable to indicate somewhat loosely affixed elements (prefixes and suffixes not thoroughly welded with stem) by means of hyphens, double hyphens (=), as Dr. Boas suggests, may be used at the ends of lines to indicate a break in the word, not meant to be of morphological significance.

XIV. It is often necessary in American languages to distinguish between true sonant consonants, "intermediate" consonants (however they be defined), unaspirated surds, and aspirated surds. To begin with stops, b, d, and g, should, of course, be used for true sonants, p, t, and k, for unaspirated surds, and p', t', and k', for aspirated surds. A special device may often be necessary to indicate intermediates as distinct from either true sonants or unaspirated surds. It is recommended that a point above the letter be used to indicate this intermediate character of a consonant. It was not definitely decided by the committee whether the point should be placed over the sonant or surd, though the general consensus of opinion seems to be in favor of the sonant. Thus, the intermediate consonant corresponding to g and k had perhaps best be written  $\dot{g}$ ; similarly, the intermediate sibilant corresponding in position to s and z might be written  $\dot{z}$ . In J. O. Dorsey's system, intermediate

consonants are expressed by inverting letters for corresponding surd consonants. In other words, the surd is taken as the point of departure. Discussion is invited on this point. In the case of consonants that are normally voiced, like l, m, n, and r, and for whose voiceless correspondents there are no simple signs in ordinary use, it is recommended that a line above the consonant be used to indicate voicelessness. Thus, l,  $\bar{l}$ , and  $\bar{l}$  would respectively indicate fully voiced l, intermediate l, and voiceless l (non-spirantal).

(The Secretary thinks that some other sign than the bar above the character will be preferable, as the macron has been, on the whole, rather consistently associated with length. A small circle (.) below the consonant might be used, as has been done, but this has the disadvantage of possible confusion with the use of the small circle under l, m, n, r, and similar letters to indicate the fact that they are syllabic in character. Perhaps small capitals might be used with advantage here, though, on the whole, the tendency among the members of the committee has been to avoid the use of small capitals as far as possible. E. S.)

XV. The principle of simplicity (see IV.) would often demand that intermediate stops be expressed either by b, d, g, or by p, t, k. Where there are unaspirated surds and aspirated surds as well as intermediates, but no true sonants, it would seem advisable to use the sonant characters b, d, g, for the intermediates. On the other hand, where true sonants and aspirated surds are found, besides the intermediates, but no unaspirated surds, it would be in place to use p, t, and k, to express intermediates. These usages should, of course, be carefully defined in every case.

(The Secretary would, on the whole, be inclined to oppose use of the same character in one language in two different senses, even if some rule could be stated for the proper usage, unless, indeed, a distinction of the two series is rendered difficult by subjective or other reasons. In other words, if the intermediate or unaspirated surd, as often happens, is regularly aspirated at the end of a syllable or word, it seems preferable to indicate the aspiration in every case. He would also strongly recommend the consistent use of the breathing after surds in the case of aspirated surds, even where the unaspirated surd does not occur as a morphologically distinct series, as, in most languages, positions are almost sure to arise in which the regularly aspirated surd becomes unaspirated, and it may be worth while expressing the difference. E. S.)

XVI. It is recommended that the convention be used of using a point beneath a consonant to indicate a point of articulation back of the one implied by the consonant itself; and that a line beneath a consonant be used to indicate a point of articulation in front of that implied by the consonant itself. This simple device may be usefully applied in several cases. f and y

would indicate the bilabial spirants corresponding to f and v. (Would not ϕ and β be preferable to f and v? E. S.) t, d, s, and z would indicate the dental or interdental correspondents of the alveolar series (s and z would indicate the interdental spirants in English 'thick' and 'then'), while ʈ, ɖ, ʃ, and ʒ would indicate the cerebral consonants found, for instance, in Sanskrit and Dravidian. k and g (the corresponding spirants will be discussed separately) would denote the corresponding front palatal consonants, while ḳ and ḡ would indicate the corresponding velars. It will be noticed that this system replaces the q, which has been ordinarily used for voiceless velar stop, by ḳ. This has two advantages, aside from the advantage of symmetry of the system itself. In the first place, it corresponds to the use of g for sonant velar stop, which has already obtained usage in America. In the second place it corresponds to the more recent usage of Semitic scholars.

(This system, which has much to recommend it, was independently invented, curiously enough, by Father Schmidt, and has already at least in part obtained considerable usage. Father Schmidt, however, writes ˘ instead of the bar (\_). This seems preferable for two reasons. In the first place, the bar is easily confused with underscoring meant to indicate italics. In the second place, should it be decided to use the bar under vowels to indicate open quality, it would be a rather inconvenient use of the same diacritical mark for two quite distinct purposes, though, after all, no confusion need result. On the other hand, are the interdental spirants so clearly associated in the minds of most people with s and z as to warrant to giving up of θ and δ, which naturally suggest themselves as available characters to indicate them? The Secretary, for one, would much prefer to see θ and δ established in preference to s and z or ʃ and ʒ. Further discussion on this point is invited. E. S.)

XVII. The surd spirant corresponding to k is to be denoted by x. x̣ (or x̣̣) would denote the palatal voiced spirant found in German 'ich', while x̣̣ would indicate the velar spirant. It is to be carefully noticed that this usage, as far as x and x̣ are concerned, is the exact reverse of that which has obtained in many texts and grammars of West Coast languages. All the members of the committee were agreed that this reversal of usage was a distinct improvement. A more difficult question is the symbol to be used for the corresponding sonant spirant of the k series. Inasmuch as the use of k relieves q from its previous function, that character is naturally available for this very purpose. It is therefore recommended that q be used for sonant guttural spirant, q̣ (or q̣̣) for sonant palatal spirant, which need not exactly coincide with the semi-vowel y, and q̣̣ for voiced velar spirant (Arabic 'ghain').

(Is q̣̣ preferable to γ? E. S.)

XVIII. After some discussion, the members of the committee thought it best, after all, to retain the use of c and j for the sibilant consonants in English 'shin' and 'azure' respectively. The use of š and ž, although they have the advantage of considerable usage, seems altogether too clumsy for

simple sounds. If, as will sometimes be necessary, it is required to distinguish various positions of c-consonants, the use of diacritical marks will make the characters š and ž even more cumbersome. The employment of ʃ instead of c, which has come into some use, is intrinsically commendable, but it might be difficult to find a correspondent for j. Moreover, it might be difficult to supply such a character in most fonts, whereas c is easily available. Moreover, if we discard c and j for the c-sibilants, what are they to be used for, for it is obvious that they are too valuable to be dismissed altogether. The use of the dot and line beneath the c and j enables us to distinguish three positions of c-sounds and corresponding j-sounds. ċ would be the c-sound of the cerebral series. c̣ (or c̣̣) would be the c-sound of alveolar position. (The common American Indian sound generally described as between s and c probably belongs here). It can not be too strongly insisted upon that s and c sounds do not differ merely in position, but that the c-sibilants as contrasted with the s-sibilants are characterized by a thickish quality, probably due, as Father Schmidt points out, to the formation of a double chambered resonance. It is not difficult to form a c-sound wherever an s-sound may be formed, and vice versa. Hence, it seems of fundamental importance to distinguish rigidly in orthography between the s and c series, however much this may run counter to prevalent usage.

XIX. A particularly troublesome problem is the proper representation of affricatives involving s and c-sounds. The committee did not decide very definitely on this point, though the consensus of opinion, on the whole, seemed to be in favor of the prevalent usage, according to which these affricatives are written as combinations of t (or d) and s or c (z or j). The difficulty which sometimes arises of distinguishing true affricative, ts, from a consonant combination, t.s, in which each has its distinct value, and similarly for other affricatives of these classes, may be quite readily avoided by the use of the period between the two letters in the latter case. Thus ts would be equivalent to the German z; while t.s would adequately represent such a combination as is found in English footsore. Should it be decided, after all, to use single characters for certain affricatives, the use of č and ǰ instead of tc and dj would go well with the prevalent usage of c and j for the corresponding simple sibilants. The difficulty, however, then arises of how to indicate ts and dz. The obvious answer is š and ž, a usage, however, which runs counter to the prevalent one of these characters above referred to. Full discussion on the best representation of sibilants and sibilant affricatives is invited.

XX. The use of a point under a character to indicate a back pronunciation of the sound indicated by that character, may be extended somewhat to take in certain laryngeal consonants. The hoarse ḥa of Arabic and other Semitic languages, which occurs also in Nootka, may be well indicated by ḥ. Should ' be employed for the glottal stop, ' may be used for a particularly strong and strangulated sounding glottal stop peculiar to Nootka. (This differs, however, it would seem, from the Arabic 'ain.)

(Secretary:- The method of representing the glottal stop was not taken up by the committee. The Secretary recommends the use of ' . The further use of ʔ or ʔ should, in his opinion, be discouraged. At this point should be taken up the matter of glottally affected consonants. In regard to the writing of two types of these there can not be much doubt, provided the ' is adopted as the symbol for the glottal stop. The glottal stop whose release is synchronous with the closure of the following consonant should be written as ' followed by a consonant; if the consonant is followed by breath release, the combination may be adequately written as 'p' and correspondingly for other stops. A consonant whose release is synchronous with glottal closure or takes place just before it, is naturally represented by consonant + ' . This consideration immediately rules out the use of characters like p' for the so-called "fortes"; in Yana fortis p (= p!) and p' are to be carefully distinguished, and correspondingly for other stops. In Ute we often have stopped consonants pronounced with synchronous closure and release of both glottis and point of oral contact. Such consonants, which are by no means identical with "fortes", are naturally represented as p̣ and correspondingly for other consonants. The question remains as to how to indicate the frequently occurring class of "fortes", i.e., consonants with simultaneous closure of glottis but with release of glottis subsequent to that of oral contact. The ! seems to be thoroughly disliked by many. What is to be put in its place? The Secretary, for his part, is prepared to adopt any alternative, though he does not personally dislike the !, provided that such characters as p̣ and p', for which he has other uses, be not offered as substitutes. In this connection the usage may be recommended of a ' over vowels to indicate what the Germans call 'knarrstimme'. E. S.)

XXI. The difficulty with the proper writing of lateral sounds is that there are very few characters available for the purpose. Voiced l-sounds of at least three positions can be indicated by the use of point and line under the l. ɫ would be cerebral, l dental. A method of indicating voiceless l-sounds has been indicated above (ḷ). For the voiceless spirantal l, ɬ may as well be continued. The difficulty is as to how to represent spirantal voiced l's.

(ɬ, however, has often been used for Slavic back l, for which the committee offers no substitute. If small cap m, n, and r are adopted for voiceless m, n, and r, why not small cap l (L) for voiceless spirantal l? Yet, to be consistent, ɫ would represent voiceless nonspirantal l, as in Yana l' or sometimes English felt.)



XXII. No definite conclusion was reached as to the best representation for lateral affricatives or stops with lateral release. If we adopt *ts* and *tc*, it seems natural to write *tl* for the sound which has hitherto been generally represented as small capital *l* (*ł*) and *d* + whatever character is employed to represent voiced spirantal *l* for what has hitherto been written *l*. Perhaps it would be an advantage to use a simple diacritical mark to indicate lateral release. It may be pointed out that Father Schmidt uses *ḷ* and *ḵ* and such other signs for sounds of this category.

(If *č* and *š* are adopted for *tc* and *ts*, *ž* would seem a natural and clumsy substitute for *tl*. If *ł* is replaced by *l*, *ḷ* might be used. E. S.)

Many further points of phonetic detail should be thoroughly discussed before a formal act of recommendation is drawn up for presentation to the Society. Most of these will present themselves to the members of the committee and discussion at the earliest opportunity is strongly urged. Some of the points not yet discussed are: the differentiation of trilled and untrilled *r*-sounds, the proper indication of palatalized consonants, the indication of nasals of the *k* series (the Secretary would recommend for this *ŋ*); representation of different types of resonance (velar resonance has been noted by Thalbitzer for certain Eskimo vowels and by the Secretary in Thompson River *z*); the indication of semi-vowels (the Secretary recommends a continuance of *w* and *y*); and syllabification, should it prove desirable to make special provision for this feature. Correspondence relative to the work of the committee should be addressed to the Secretary, Geological Survey of Canada.

E. Sapir

B.

A. L. Kroeber to Edward Sapir, March 18, 1913: Comments  
on the Preliminary Report

Section 1. Good. I recommend a definite and brief formulation of this principle, which I would extend so far as possible to "non-culture" languages like Dakota for which a standard alphabet is in practical use.

Section 2. Good. A definite formulation is desirable.

Section 3. A bad principle, because it will not be observed, and because it will lead to too many diacritical marks and modified or inverted characters. Too inconvenient for typewriting and makes printing expensive. Italics and

small capitals should be reserved for specific uses. They are too valuable to lose. The same applies to Greek letters.

Section 4. Good. Advise a briefer formulation.

Section 5. Good. The only correct principle. We should go farther and say: one sound, one character; one character, one sound, as was laid down 50 years ago. This principle will eliminate tc, ts, and so on.

Section 6. Good.

Section 7. If length is an integral part of the sound, the dot should not be after the letter, or the principle of section 5 is violated. If the dot is desired as specified, we should word section 5 so as to except stress, length, pitch if necessary, and no other factors. I would not take up the representation of unusually short sounds.

Section 8. Good, with the Secretary's amendment allowing option as to the placing of the accent.

Section 9. Good, but too detailed. Sections 5 to 9 should be worked out as a unit.

Section 10. The iota subscript is historically so unjustified for nasalization, and so without precedent, that it will never be generally adopted, and its recommendation will be a mistake. I cling to the tilde; if this is in the way above, let us put it below the vowel.

Section 11. We can never properly settle this difficult matter of vowel qualities. Let us leave it without recommendation. Usage will work out a system in the end.

Section 12. Superior letters are good for weakly articulated vowels, but I can not assent for surd vowels. Nor is rough breathing followed by superior vowel any better. The aspiration is in surd vowels, not before them. The only solution is setting aside either italics or small caps for all surd equivalents of sounds usually sonant. This would at one simple stroke, without offence to the eye or cost in printing, take care of surd a, e, i, o, u, ŏ, ũ, n, m, ng, l, r, w, y; even of the intermediate stops, which would be italic or small cap g, d, b.

Section 13. Good.

Section 14. I disagree. The point over the letter is bad. The rough breathing after the letter is wrong, violating the fundamental principle of section 5, as the aspiration is part of the release or explosion, not a new sound. If the rough breathing is used, it should be put over the character.

Section 15. Good. I do not agree with the Secretary's suggestions.

Section 16. The use of one supposed diacritical mark to indicate anterior and of another for posterior articulation is excellent. I favor the dot for posterior position. I cannot agree to the line for anterior articulation, as it will conflict with italicization. However the Committee may rule, italics will not go out of use. Those employing them will be forced to drop the present sign, and diversity of usage will be at once established. The supposed semicircle is free from this objection, is already in use, is inoffensive, and I urge it as the solution.

As concerns th and dh, no modification of s and z will be practical. The sounds are too different aurally and conceptionally. People will not use s and z for th and dh. I favor Greek theta and delta.

Section 17. I favor x. I disapprove thoroughly of q for the sonant. There is no historical justification, even in the American field; q for the sonant would be as arbitrary as Powell's use of it for the surd. Greek gamma, which has good and ample precedent, is the solution. If consistency is desired, I would even accept Greek chi and gamma, though the Roman equivalent in use for the former is more practical.

Section 18. I cannot accept c and j for sh and zh. If the characters are too valuable to be lost, italics and small capitals are more valuable. S and z with superior caret enjoy practical and scientific usage. For modifications of them inferior diacritical marks remain. They cannot be misunderstood by any person with elementary scientific training. If the theoretical dissimilarity of s and sh sounds prevents the committee from accepting s and z with caret, I would accept the manuscript s of the French phonetic association; but I do not give much weight to this distraction as affecting orthography, because the unconscious feeling of nearly all European languages has been that the sounds are similar, as shown by their using sh, sz, s with caret, or other modifications of s, for sh, while th is never represented by a form of s.

Section 19. Ts, tsh, and so on for affricatives violates the basic principle of section 5. I hold single characters to be essential. I favor agreement on a single distinguishing mark, either diacritical, or italic or capital font, for all t affricatives of the type ts, tsh, or tl. The much rarer sounds like tth, pf, kx, kl, and so on, could be left to the invention of the occasional authors confronted with them. If no general class designation for t affricatives is adopted c and j with superior caret remain for ch and dj.

Section 20. Good, as far as it goes. The glottal stop must however be decided on by the committee, and so must glottally affected stops. For the former I agree that the apostrophe is normal, intelligible, and justified. For the latter, again on the principle of section 5, I recommend the apostrophe over not after the stop character. The designation of the many different types of glottally affected stops or other sounds, which probably do not often occur synchronously in one language, could then be left to individual authors, just as the various types of r need not all be provided for by the committee.

Section 21. Laterals will make little difficulty if one general designation, such as small caps, is adopted for all surd equivalents of sounds whose usual characters denote sonants, and another designation, such as italics or the macron, for all t affricatives.

For w, y, and ng I favor w, y, and Greek eta, at least in the absence of any further suggestions. Trilled and untrilled r sounds may normally be left to description. Syllabification will take care of itself as long as most writers do not syllabify.

I add the following tabulation of recommendations, all applying to classes of sounds:

<u>After or On Character</u>		
Superior	period (after character)	length
"	acute (on <u>or</u> after)	stress accent
"	grave (on <u>or</u> after)	secondary stress
"	designations (on <u>or</u> after)	pitch
<u>On or Under Character, Never After</u>		
Superior	tilde (or inferior)	nasalization
"	rough breathing	aspiration
"	apostrophe	glottal affection
"	caret (point down)	sh-type
Inferior	period	posterior articulation
"	semicircle (convexity above)	anterior articulation
<u>Different Font</u>		
Small capitals		surd equivalents of sonants
Italics		t affricatives
Greek characters		fricatives, where needed

C.

Edward Sapir to A. L. Kroeber, March 27, 1913: Reply to

Kroeber's Comments

I have read your reply over very carefully. On many points I thoroughly agree with you, and I see that in regard to some of them we are more in accord than Boas, Goddard and I were. On others, I am compelled to disagree with you. In one or two cases, I think your facts are wrong. By way of getting down to something specific, I should like to take up your replies in order with what seem to me appropriate comments. You will observe that I am prepared to

sacrifice my own preferences on several points. Needless to say, I shall expect you to do likewise. It is, after all, only by compromises and sacrifices that uniformity in these matters can ever be attained.

Section 1. Formulation should not only be briefer but less specific in its reference to particular languages. A point of this sort will, in practically every case, have to depend on the temperament of the writer. As regards to your note on Dakota, should it ever, for instance, be my task to study Siouan languages comparatively, I should certainly not in the least be deterred by Riggs' system from writing Dakota as I saw best.

Section 3. I thoroughly agree with you that the principle is bad. I could not agree at all with Boas and Goddard that it was inadvisable to use Greek characters and small capitals. I do not know why they seem to have so little feeling against heaping of diacritical marks. In Goddard's case it is perhaps to be explained by his failure to bother with quantity and accent, anyway, as a rule, so that he would not need to heap diacritical marks as much as some others. For my part, I not only do not object to Greek letters, but I distinctly like them. For some reason which seems inexplicable to me, Goddard, Boas, and also, it may be added, Father Schmidt, find that mixing roman and Greek characters produces an unaesthetic result. I have diametrically the opposite feeling, due perhaps to some early association. At any rate, I feel convinced that Greek characters for certain vowels and spirants are inevitable. Small capitals also should be utilized. How would small capital vowels do for corresponding obscure vowels, small capital consonants for voiceless forms for consonants ordinarily voiced? Some other use perhaps could then be found for small capital forms of consonants ordinarily voiceless. As for italics, I am afraid that I can not agree with you. I think to mix roman and italic characters in one word is indeed a nuisance. First of all, it is unsightly. Secondly, it interferes with styles of type. For instance, if, as is often done, all native words are written in italics, it means that your phonetic italic characters would have to be turned into romans. Hence there is constant confusion resulting between italics as essential phonetic symbols, and italics as mere styles of type. Moreover, it is often desirable to call attention to grammatical elements by italicizing, or, if Indian words are written in italics, by romanizing. Here again phonetic use of italics would lead to horrible confusion. As regards this section, then, I should want to meet you more than half way. Let us save Greek letters and small capitals by all means. Let us taboo italics for strictly phonetic purposes.

Section 7. I do not believe that such principles as are merely matters of policy should be taken too seriously. Stress and length I should not consider as essential towards making up the intrinsic quality of a sound, either being variable without materially affecting the character of the sound as such. I do not quite see what you mean by saying "if length is an integral part of the sound". If you simply mean that long and short vowels are etymologically distinct, I do not see how that affects the phonetic principle laid down in the slightest. We all seem to think that expressing length by means of inverted period after letters would be a great saving. Could you not simply agree to

section 7 without further parley? I am sorry that you have nothing to suggest in regard to unusually short sounds. My colleague, Mr. Barbeau, insists on distinguishing between long, medium, and short, although, as far as I can see, there is no essential difference between his short and medium vowels. For the sake of seeing all satisfied, I should like to have some similar device adopted instead of the breve, which of course complicates printing hugely.

Section 8. Three cheers for local option!

Section 9. I am glad you like this system of representing pitches. It had to be explained in full, so that its intrinsic merits could be made obvious. Most of us, of course, will never need to make use of this system, but you can never tell. Harrington has something choice in that line in Tewa, and we ought to legislate for him.

Section 10. I regret I have to disagree thoroughly with you. The hook referred to is not iota subscript, but is generally printed as a squarish hook (ȁ, Ȃ) which looks quite different from it, though, of course, there are other forms of it which resemble the subscript. Moreover, you are quite wrong in saying that there is no precedent. There is both popular and scientific precedent. Nasalized vowels are regularly expressed in Polish by means of subscript hooks. This method is also used for Lithuanian. As for scientific usage, all Indo-Germanists, at least such as concern themselves with Indo-Germanic philology as such, are consistently using hooks. It is from this usage, indeed, that both Harrington and myself have borrowed it. The tilde above is certainly a big nuisance. It interferes with umlauts, stress accents, pitches, and perhaps other diacritical marks that it might be necessary to adopt. If you put the tilde below the letter you practically abandon your own precedent, and so might just as well adopt the other precedent, namely, that of subscript hook. I may say that Mr. Barbeau, in his Iroquoian work, was at first in the habit of using the tilde and seemed to be quite fond of it. Later on, when he found that it interfered very considerably with stresses and breves, he abandoned it for the subscript hook, which he now swears by. I am afraid you will have to give in on this section. At any rate, let us kill the superior n, which we all seem to find barbaric.

Section 11. I am sorely disappointed at your reply. If there is one thing more than another that I wanted discussed, it was just this matter of vowel quality. Indeed it was perplexity as to how to be able to designate them that brought up the whole matter of a phonetic agreement in my mind. Can you not definitely commit yourself as to certain fundamental principles, at any rate? I did not really expect that we could work out a definitive system of vowels once for all, and perhaps it would not be advisable to try too much. However, we should try, it seems to me, to decide how to distinguish open from close vowels, if nothing else; also whether to use Greek characters for some vowels; and, further, whether to use umlauts. Would you not at least agree on these points:- No italics; make use of Greek characters for certain open vowels; do not attempt to combine quality and quantity in one diacritical mark, as associations of this type, which may be typical for some languages,

may seem quite arbitrary for others. I think we might at least legislate in regard to the 12 vowels that I first listed in my supplementary recommendations.

Section 12. Your remarks are quite to the point on general phonetic principles, but I think you might have difficulty in convincing Boas that there are such things as voiceless vowels. He seems to operate exclusively with consonantal timbres. Of course there are as many voiceless vowels as there are ordinary voiced vowels. The trouble with representing voiceless vowels by means of lower case characters, whether italic or small capital, is that it gives an erroneous idea of the syllabification and rhythm of the word, though, as far as Shoshonean is concerned, voiceless vowels make syllables just as truly as do voiced ones. You see I am playing into your hands somewhat. I got into the habit of writing rough breathing followed by superior vowels for voiceless vowels, because I could not help feeling that these were, after all, quite different from rearticulations, murmured echoes after glottal stops, true whispers, and glides. I confess that I can think of nothing that seems better to me than my present system of writing voiceless vowels, the more so, as it is not always easy to draw a hard and fast line between such and vocalic timbres. Thus, k<sup>u</sup> in some languages is the form which kw takes at the end of a syllable. In others, as in Shoshonean, it is the form which the syllable ku takes under certain circumstances. You see, in the one case it has no etymological syllabic value, and in the other it has. It would be a poor phonetic principle to be guided, however, by etymological considerations. On the other hand, to write k plus italic u or small cap u for such languages as have k<sup>u</sup> developed from kw would certainly be extremely awkward. I am rather embarrassed on this point.

Section 14. As regards representation of intermediates, I agree with you. I do not think the point over the letter is good, though at the time it was suggested in New York, I did not think very much about it. Your point in regard to the rough breathing after letters does not appeal to me particularly. As a matter of fact, the breath release is often felt as a distinct element, and for that very reason these consonants have often been written ph, and so forth, as is still done in Sanskrit and in Indo-Germanic philology. The consonant as such, after all, remains the same whether the release is (strongly) aspirated or not. Personally I think it is most desirable to have a symbol such as rough breathing to indicate marked aspiration. Moreover, such aspiration follows not only stopped consonants, but quite frequently nasals, l's and vowels. In the case of final vowels followed by aspiration, I think it would be quite pedantic to consider the final aspiration as an intrinsic part of the vowel and to want to write it over the vowel. Writing rough breathing over letters complicates the printing problem quite unnecessarily. Of course, if one does not care to indicate aspiration, let him leave out the rough breathing. In devising a system, however, I should not be guided so much by personal practice as by general needs. Provision for aspirational release, as such, should certainly be made.

Section 15. It is useless to argue much about these matters unless one has specific languages in mind. On this point, everything depends on one's ability or inability to hear things, also on how strongly one believes it necessary to distinguish classes of sounds. We need not legislate specifically, however, in regard to just how rigidly the law of simplicity is to be followed.

Section 16. Your point in regard to line beneath the consonant is well taken and applies also to the use of the line for open vowels. I do not suppose that there will be much trouble in getting other to consent to the semi-circle. As a matter of fact, I do not believe we thought about it at the time. I am glad that you favor theta and delta.

Section 17. I agree with you in regard to q. It has nothing to recommend it, although Goddard and Boas were quite enthusiastic about it, and I was far more so at the time than I am now. Of course, we then had it in mind to eliminate Greek characters if we possibly could. As, on more serious reflection, I see no point in doing so, I think we may as well stick to gamma.

Section 18. C and j in their present American usage are really intrinsically admirable, but it so happens that that particular usage is not much followed outside of America, and thus creates embarrassment. J would not be so bad, as its phonetic use corresponds to its popular use in French and Portuguese. S and z with superior inverted carets have wide usage, though there is little to recommend them intrinsically. If necessary, I am quite willing to submit and make shift to use them, if only to approximate more closely to general European usage. Do not forget, however, that students of phonetics, as such, are thoroughly dissatisfied with these symbols and that it might eventually be borrowing trouble to legislate in their favor. Even if c is to be abandoned, I should prefer the old-fashioned s (MS. s) of L'Association Phonétique. How would this sign and j do as substitutes for c and j?

Section 19. I believe our best policy will be to leave the representation of the affricatives optional, as all must depend on the particular language. In other words, tc may be represented either as a single character or as two characters. After all, the affricatives are not fundamental sounds, but combinations of sounds, however much they may be felt to be units psychologically. How would it do to use c for ts, j for dz, c with superior inverted caret for tc, and j with superior inverted caret for dj, present c and j being represented by s with superior inverted caret and z with superior inverted caret? This method of representing four important affricative sounds has been followed by Hübschmann in transliterating classical Armenian. C for ts, moreover, is much used by Slavic philologists. Personally, I think a better principle would be to employ the superior inverted caret to denote the corresponding affricative of whatever stopped consonant is indicated beneath. As Schmidt suggests, if we still use c with inverted caret and j with inverted caret for tc and dj, then c and j remain as corresponding stops. These stops, you will



carefully notice, are not ordinary t and d, but are peculiar more or less palatalized (palatal in its better sense) stops, such as perhaps do not often occur by themselves. Still, there would be no harm in setting aside c and j for such sounds, or, in particular languages, sounds which are acoustically related to them. Ts and dz would then be represented respectively by t and d with superior inverted caret. For lateral affricatives I think it might be just as well to adopt some simple diacritical mark parallel to superior inverted caret for ordinary affricatives. We might use uninverted caret above or uninverted or inverted caret below. I do not believe that Boas will fancy this business much, as he thinks of lateral affricatives more in terms of l than of dorsal t. His standpoint, of course, is well grounded in etymological considerations. On the whole, however, I think it is best to start from the stopped consonant of an affricative combination rather than from the continuant element. I confess that I am all mixed up on sibilants and sibilant affricatives. Italics or small capitals for t affricatives, as you suggest, would be horrible.

Section 20. There is no doubt but that the apostrophe will go through for the glottal stop. I do not believe that Boas would object particularly, though I fancy that he has some ideas about hiatus (to be represented ') as distinct from glottal stop between vowels. Much to your surprise, I am quite willing to adopt apostrophe over stopped consonants for glottalized consonants, leaving it to be explained in any particular case whether these are "fortis" or like those Shoshonean sounds that I ran across. The only trouble is that it will be somewhat awkward to indicate tc', ts', and ʟ' according to this system, if we use superimposed diacritical marks for indication of affricative character of a sound. My stenographer does not think k' as good as k! This may seem surprising, yet it need not be. ! has value psychologically as suggesting something violent, cracked. Besides, k! is far prettier than k', q! than k̄.

Section 21. Laterals will make more difficulty than you seem to suppose. There is a decided difference between voiceless l, and I found it in Yana and as it occurs elsewhere, and voiceless spirantal l, which occurs in many West Coast languages. These sounds are absolutely distinct. I do not think that barred l is particularly admirable for voiceless spirantal l, as it runs counter to current European usage, where barred l denotes velar or some other type of back l, as in Polish. Why not λ for present l? Thalbitzer has used it. Suggest:

ɿ = λ; voiced π (Russian)	ʟ = d <u>or</u> <u>đ</u> <u>or</u> <u>ḍ</u>
ʟ = tλ <u>or</u> <u>ṭ</u> <u>or</u> <u>ṭ̣</u>	ʟ! = ṭλ (poor) <u>or</u> <u>ṭ</u> (!!) <u>or</u> <u>ṭ̣</u>

W and y, of course, will have to go through, regardless of Harrington's recent use of j for y. For ng, I am glad that you are willing to abandon n with tilde. I do not particularly object to Greek eta. Perhaps it would be better to adopt extended n (η), as in Riggs' Dakota, and in Indogermanic, and as is so often done in European systems generally.

As regards your tabulated set of recommendations, I would, then, agree with your superior period; superior acute; superior grave; superior designation for pitch; superior apostrophe; superior caret; inferior period;\* inferior semicircle; small capitals; and Greek characters. All that I disagree on would be superior or inferior tilde; superior rough breathing; and italics for t affricatives.

\*Yet it seems a pity to give up q for k, as q is so much easier in field-work. Incidentally, I don't think we have been thinking enough about field needs. After all, l is easier and faster to write than l̄ or l̂; c than š; j than ž. Which do you prefer?--- Comox q!eq!AL!q!ā<sup>a</sup>L! (as I now write) or kekařkřá.<sup>a</sup>č (!!!!).

D.

J. P. Harrington to Edward Sapir, April 18, 1913: Comments  
on the Preliminary Report

Herewith I present my views on the preliminary recommendations of the committee.

To start with, I do not believe that with our present knowledge the construction of a phonetic alphabet is a possibility. I should not care to serve on the committee if it is the intention that it present decided views or attempt to legislate definitely or in detail. Our final report should be most undecided in tone. It should consist merely of a discussion of various view points and a setting forth of various systems.

Personally I have no likes, dislikes or preferences as regards the use of characters. C, q, x or any other letter appeal to me equally with any value. Even the Franciscan Fathers' q for aspiration or the common ! for glottalization produce nothing but an agreeable sensation. I get used to any symbol very quickly. A strange phonetic alphabet such as Armenian can be learned in a day or two. In fact, I am pleased to see Sanskrit written in devanagari, Nahua in the current orthography, Navaho in Franciscan, etc. Variety is delightful. The greater number of methods of writing š or indicating glottalization, the more interesting. I am glad that every country of the Balkans has its own alphabet and its own nationality. I am glad that various Americanists have various tastes as regards the writing of American languages.

However, we should have variety within sane bounds. We should not encourage poor taste. For recording unwritten languages and language when phonetics is greatly emphasized I think that it is expedient to use the Latin alphabet and use most of the symbols with their standard "continental" values. Standard values really exist. These values have been employed in several

hundred phonetic alphabets and in such text books as those by Sweet, Jespersen, Sievers, Viotor, Passy, Tomson, etc. I outline these values briefly. The vowels have their Latin worth: aridus, servus, discus, corbis, dulcis. Or they have closely similar values. W as in English (new and old), old Frisian, old German. J as in Latin, Italian, German, Dutch, Skandinavian, Slavic, Roumanian, etc. B, b, f, g, h, k, l, m, n, p, r, s, t, z, with their Latin values. C is not very legible and its shifting of value in the Romanic languages has created widespread havoc. Most phoneticians regard  $\int$  and  $\zeta$  as sufficiently good characters for the sounds of French ch and j. The use of q for North German -g- or French j probably rests upon the analogy of the letter with g and too economical a spirit. It can hardly be recommended except as an interesting experiment. The tendency in phonetics is to use v for the voiced dentilabial fricative. Greek  $\chi$  is either of the same origin as x or has at least influenced its origin. The Modern Greek and Russian alphabets use  $\chi$ , x, for the sound in German Bach. That value may be adopted for the roman x. Roman x has also been used for French ch in several alphabets; it had the latter value in the Spanish of a certain period and still has in the pronunciation of several Mexican languages. Phoneticians universally regard it practical to use y with the value which it had when it was taken into the Latin alphabet, and which it still has in Skandinavian, Finnish, etc., and many pronunciations of German,—namely that of German ü. The values of the twenty-two letters, a, b, d, e, f, g, h, i, j, k, l, m, n, o, p, r, s, t, u, v, w, z, will stand whether the committee sanctions them or not.

I. Common sense should determine in every case whether to use what Sweet would term a "nomic" or a phonetic alphabet. For Nahuatl it would be practical to use the Mexican orthography. For Dakota, Riggs' and Dorsey's spelling; for a Lithuanian dialect, Lithuanian spelling, for a Finnish dialect, Finnish spelling; for a south Caucasian dialect, Georgian spelling; for Chukchee, Russian spelling would be practical; for Ainu the Japanese syllabary might be best. But where the use of an alphabet means favoring a freakish, stupid development, such as the adoption of Sequoya's alphabet for Cherokee, this principle should of course be abandoned.

II. I favor capitalization. If the recorder thoroughly understands his text he should not neglect advantageous use of punctuation.

III. As regards mixing of fonts, the use of roman and italic letters in the same word is not very offensive to people generally. The mixing in of small capitals is more offensive, while the occurrence of Greek, Armenian, Russian and Japanese characters in a word would be still more so.

The Greek characters are especially objectionable because they occur only in the italic style. The greater amount of phonetic printed matter will probably always be roman, and a liberal mixing in of fat italic greek characters would strike many people as abominable. As regards inverted letters, if easily written they are o.k., if not, they should be avoided.

IV. O. K.

V. One sound, one letter; two sounds, two letters, etc. Diacritical for modifications of sounds. Discussion as to whether English ch, etc., is one or two sounds is as futile as the Dutchman's headbreaking over whether there are one or two worlds. There is a new world and there is an old world and yet it is all one world. Psychologically people often consider a whole syllable to be a sound. Sievers used to say it was one sound, Finck that it was two, and he says so emphatically in his Gypsy grammar. The discussion, like the doctrine of the Trinity, is nothing but a silly play in words. Largely separate from the question whether an affricative is two or one or two in one is the problem of how they should be written. Two in one shoeblacking has been invented, but I am inclined to think that ts, tʃ, etc., will prove more popular than š, č, or the like.

VI. Stress and tone are more prominent in vowels which form the body of syllables than elsewhere. It would be handy to write stress and tone over or under vowels.

VII. Every system of writing length has its disadvantages. Why not use a diacritical for long, nothing for medium, superior character for short duration?

VIII. If the acute accent is used it ought to be written over the vowel as it usually is, in fact, at present, although printers often set it up after the vowel. A more original and now quite widely spread use of the acute is as an indication of long duration.

IX. I regard Father Schmidt's system of writing tone as impractical and not based on thorough study. Loudness and high pitch is a frequent combination. Should I write the tribal name tēwa; bread, búwá? I regard the construction of a general system for writing tone and pitch as quite beyond all of us. In our report we might point out the difficulties and refer the student to Schmidt's, Jones' and other attempts and show how sadly all of them lack knowledge of the subject and how they use up all the easily made diacriticals which we need for other purposes.

X. Is good--perhaps too good.

XI. The problem is bad enough in the case of i and u,--for e and o it is worse. In our lack of knowledge it is impossible for us to solve the problem. It is probably the most difficult in all phonetics. For writing the world languages there are two schools--those who favor leaving length unwritten, and those who favor leaving quality unwritten. One of these schools will probably ultimately prevail.

XII. "a" violates the rule "one sound, one letter." For some languages I have written not voiced vowels superior (when they were short and inorganic); for others, on the line, would it not be sufficient to state in the phonetic key that the vowels written superior are whispered, murmured, or what not?

## XIII. O. K.

XIV. In our lack of knowledge and statistics I feel sure that it is preposterous for us to attempt to solve this knotty problem of choice of symbols for stops. I do not much fancy the term "intermediate."

XV. In my ignorance I prefer p, t, k, for these "intermediates," no matter what other series of stops may occur in a language.

XVI.    and    would, after all, not be very satisfactory. The k of one language may be as far forward as the k of another. The device has been advocated by many, notably by old Techmer. It is too supple. I prefer    to   , since the latter suggests consonantal functioning to all Indo-Germanists. That    indicates italicizing should not keep us from using it in this new value. That raised comma is our sign for the possessive,    the iota subscript of the Greeks, that we often insert omitted words superior to a line, does not preclude the use of these devices with phonetic values.

   and    are easier and more in keeping than    and   .

XVII. I have mentioned the problem of x and q above. Many symbols have been proposed for North German -g-, notably    by Sievers.

XVIII. I reiterate that c and j have little or nothing to recommend them in the current American values. To use the c thus does little harm except that it is confused with e (How I have sweated and cursed over MSS. using this symbol!), unprecedented in this value, and hard to get used to. But to use the j in this abnormal value works serious consequences. English and most of the French dialects use j with affricative value; literary French and Portuguese use it as the fricative; Latin, Italian, Germanic (except Eng.), Slavic, Ural-Altaic, etc., use it with its proper Latin value, twin sound to the i from which it is derived, as do all the books on phonetics. In our present lack of knowledge I think that the safest thing to do is to use j in its standard value. There is such universal precedent for this that no one could be criticized for doing so at any rate. S, z,   ,    are good enough for the fricatives; ts, tʃ, dz, dʒ for the affricatives.

## XX. O. K.

XXI. It is probably good enough in spite of its original use for the Polish non-palatalized l. Sad to say, neither tʃ, tʃ, nor tʃ looks very pretty for the affricative, nor does   !

The    seems better than the Roumanian character, although I have had trouble in having carefully written    set up. The printers set it as y. I know that nothing could induce some, e.g., Dr. Spinden, to use it. In writing a language such as Spanish, which has many   's, the symbol is distinctly cumbersome (blan  ko!). If y is used as a consonant the confusion would be great, especially in Chinese--imagine having u and n both consonants!

I should be grateful for some description of the formation of Thalbitzer's velar-resonanced vowels.

Would you agree to the proposition that the committee make in its final report no recommendation which is not sanctioned unanimously by all its members? It is an old saying that fools venture in where angels fear to tread. Even if the science of phonetics were a hundred times more advanced than it is, the writing of language would be a most difficult problem. It would be then as it is now, as Dr. Goddard says, to a large degree a social problem. We have but few letters, and sounds are most difficult to group; our letters are bound by tradition and it would be next to impossible to introduce new values even if we had sufficient knowledge to know how to change them. Let us act with the same conservatism and unpretentious and uninsistent spirit that Sievers, Jespersen or Rousselot would show if appointed to a committee, and make it clear in our report that we are uncertain of our ground and not unalterably opposed to anything.

## APPENDIX II.

Materials Relating to "A Bird's-Eye View of American Languages North of Mexico" (paper read by Sapir before section H, American Association for the Advancement of Science, Chicago, December 1920).

See Letters 319 and 324.

## A.

## Abstract

The widespread feeling that Powell's linguistic map has served its purpose and that it needs to be superseded by a classification indicating wider historical perspectives. The difficulties in the way of a revision: difficulties of material; of method. The role of linguistic borrowing in America. The gradual differentiation of linguistic group. Criteria for genetic inferences: lexical resemblances controlled by phonetic law; phonetic patterns; fundamental morphological patterns and processes. convergences and borrowings within a linguistic framework. An attempt to work back to a provisional classification of American languages: 1. Eskimo-Aleut; 2. Algonkin-Wakashan; 3. Nadene; 4. Penutian; 5. Hokan-Siouan; 6. Aztec-Tanoan. Suggested extensions of 4 and 5 into Mexico and Central America. Possible significance of wider linguistic classification for inferences as to ancient movements of population. Certain Proto-American possibilities. Peculiar position of Nadene (group 3).

## B.

## Sapir's Lecture Notes

General recognition that Powell's linguistic

classification has already served its purpose.

Something more historical and inclusive required now

Difficulties in way of serious revision:

1. Not all American languages sufficiently well known yet
2. Persistently descriptive-psychological point of view in spite of all talk of historical method

3. Too much reliance on secondary factors of descriptive order (incorporation, instrumental prefixes, polysynthesis), with no serious attempt to get perspective as to age of different features. Fundamental features never yet isolated for large groups. Importance of subtler features of dynamic order (types of stem, closeness of welding of elements, accent, tone, fundamental phonetic pattern, development of unified form, order); of vestigial vs flourishing features
4. Disturbing factor of borrowed features

Suggested classification far from permanent;

merely represents my present feeling in matter and subject to serious revision; not influenced by race or culture factors

Basis: morphological, helped out by lexical evidence

Characteristics of 6 main groups; generalized for groups,

do not always apply to particular languages in descriptive aspect, though survivals sometimes discoverable:

- I. Eskimo-Aleut: Polysynthetic and inflective; consistently suffixing; extreme welding of stem and suffixed elements; great elaboration of formal aspect of vb. (mode, person); fundamental importance of trans. vs. intr. (subj. intr. and obj. tr.); presence of local cases and 2 syntactic cases corresponding to verbal classification; noun plural and pronominal elements have formal, not merely material, value. No reduplication, inner stem modification, compounding (no nominal incorporation)
- II. Algonkin-Wakashan: polysynthetic and inflective; primarily suffixing (Algonkin suffixes far older than prefixes, which are only loosely added proclitics) -- Wakashan solely so; Algonkin has clearly inflective cast in modal and pronominal verb suffixes and in gender and number and obviative of noun, Wakashan less clearly inflective, but has important stem modifications; fundamental vb. classification into subjective and objective; only obviative in part developed as syntactic case and one local case (also in part). Reduplication well developed, particularly in W.; inner stem changes. No compounding in ordinary sense (partly in Algonkin vb.); incorporation moderately developed in Algonkin. Extreme development of secondary elements, "suffixes," with concrete significance: local, instrumental, adverbial, concrete verbalizing
- III. Nadene: loosely polysynthetic and fundamentally isolating, with quasi-inflective developments. "Prefixing" in a sense, moderately suffixing (most so in Haida); properly speaking, we have monosyllabic elements in definite order that amalgamate more psychologically



than morphologically (cf. Eng. "he came upon it," where "came-upon" is not really "came" and "upon"); "word" here actually midway between short sentence and true word (stem and derivative and formal affixes) (See I and II.). Inner form is in verb stem and implications of order rather than in explicitly formal elements. No true welding of stem and affixed elements except to form new stem entities. True stem is monosyllabic of type c + v (probably also c + v + nasal). Secondary phonetic processes bring about appearance of formal verbal development, but coalescence of subj. pronoun with modal "prefixes" is really independent of verb stem changes. Importance of "voice" and "aspect;" tense not so fundamental. Verb classifies into active and static (including object tr. or subj. pas.). Postpositions well developed, mostly nominal in origin. Composition elaborately developed. No reduplication or formal development of gender, case, number; "relative" forms distinctive, also nominalizing of verb forms by postpositions. Tone developed; intrinsic high-low.

- IV. Penutian: not polysynthetic; decidedly inflective (nearest to Indo-European and Semitic of all American languages in general form). Primarily suffixing; prefixes, where found, are clearly secondary in origin. Suffixed elements have almost exclusively formal significance and are closely welded with stem. Pronominal suffixes, case elements, treatment of plurality, tense and aspect and voice in verb give words formal cast; also note importance of inner stem changes, including reduplication (sometimes final) and vowel insertion (type haga- : hag- ). Frequency of stem-type c + v + c<sub>1</sub> + v. No incorporation typically developed, in general no concrete complications in verb; composition either absent or very moderately developed. Verb classifies into (subj.) intr. and (subj.) tr., perhaps also tr.obj. as originally distinct 3 d category. Tone systems found (Takelma, Maidu (?), Wintun (?)), apparently rising-falling type; significance not yet clear. Chinook as independent polysynthetic development on basis of broken down, analytic development of Penutian. Adherence to group evident from lexical evidence and from vestigial features. -- Tsim. profoundly influenced by group II.
- V. Hokan-Siouan: Polysynthetic and agglutinative; inflective tendency practically absent, in spite of formal subtleties and occasional stem modifications. Both prefixing and suffixing, but most characteristically prefixing in more formal elements (particularly pronouns) (Yana has secondary features). Verb:- active: static primarily (Chim., Sioux, Iroquois). Incorporation and true compounding best developed in this group. Reduplication not so typical as in groups II and IV, sometimes absent; inner stem changes not generally found (Yana exceptional). Great development of concrete affixes, particularly in Hokan (instrumental prefixes -- also Siouan; local suffixes and secondary verb stems -- based on old compounding).

Postpositions well developed. Frequency of stem type  $v + c + v_1$  ( $+ c_1 + v_2$ ): itali, ipali, ama or uma. Tone reported for Achomawi, Mohave, possibly Pomo; said to be a significant and high-low; needs further investigation. Group shows rather little stability.

- VI. Aztec-Tanoan: moderately polysynthetic and weakly inflective at best. Formally suffixing; prefixes either merely proclitic elements or old compounded stems. Suffixes belong to formal category, as in IV. Possibly polysynthesis formed on basis of IV via simple compounding processes, perhaps due to contact with V. Verb: subject-object, as in II, markedly distinct from I - IV, III - V. Reduplication frequent, also incorporation and compounding. Postpositions common. Noun and verb sharply distinct. Case developed, but rather weakly. All in all, mixed rather than specialized type. Impresses me as old Penutian strongly overlaid by Hokan (same process, but infinitely older, as took place in Maidu). Frequency of stem type  $c + v + c_1 + v_1$ . Tone in Tanoan-Kiowa; significance not yet made clear

Proto-American possibilities:

1. Persistence of n- "I," m- "thou"
2. Negative ka, ku
3. Continuative - plural - iterative -l-
4. Diminutive -si, -tsi

Regrouping possible:

1. Eskimo-Aleut  
Algonkin-Wakashan  
Penutian
2. Na-dene
3. Hokan-Siouan

Aztec-Tanoan: transitional between 1 and 3

Valuelessness of polysynthesis as genetic criterion; Na-dene as tremendous wedge in older distribution; 1 and 3 as

2 great foundation groups in America - N and S. Movements of population to be revealed by linguistic research.

## C.

## Outline of Classification

- I. Eskimo-Aleut
- II. Algonkin-Wakashan
  - 1. Algonkian
    - (1) Algonkin
    - (2) Wiyot-Yurok { Wiyot  
Yurok
  - 2. Kootenay
  - 3. Wakashan-Salish
    - (1) Wakashan (Kwakiutl-Nootka)
    - (2) Chemukum
    - (3) Salish
- III. Na-dene
  - 1. Haida
  - 2. Continental Na-dene { Tlingit  
Athabaskan
- IV. Penutian
  - 1. Californian Penutian { Miwok  
Costanoan  
Yokuts  
Maidu  
Wintun
  - 2. Oregon Penutian
    - (1) Takelma
    - (2) Coast Penutian { Coos  
Siuglaw  
Yakonan

- (3) Kalapuya
- (4) Chinook
3. Tsimshian
- V. Hokan-Siouan
1. Yuki
2. Hokan
- (1) Northern Hokan { Karok  
Shasta-Achomawi  
Chimariko  
Pomo
- (2) Yana
- (3) Washo
- (4) Salinan
- (5) Chumash-Yuman { Chumash  
Yuman
- (6) Esselen
- (7) Seri
- (8) Tequistlatecan (Chontal)
3. Coahuiltecan
- (1) Coahuilteco
- (2) Tonkawa
- (3) Karankawa
4. Keres
5. Tunican
- (1) Tunica
- (2) Chitimacha
- (3) Atakapa

## 6. Siouan-Muskogian

- (1) Siouan
- (2) Yuchi
- (3) Muskogian (incl. Timucua ?)

## 7. Iroquois-Caddoan

- (1) Iroquois
- (2) Caddoan

## VI. Aztec-Tanoan

- 1. Uto-Aztekan { Nahuatl  
Piman  
Shoshonean
- 2. Tanoan-Kiowa { Tanoan  
Kiowa

As yet unplaced

- A. { Wailatpuan  
Lutuami  
Sahaptian
- B. Zufi
- C. Beothuk

## D.

## Published Summary (Sapir 1921d)

A Bird's-Eye View of American Languages  
North of Mexico

It is clear that the orthodox "Powell" classification of American languages, useful as it has proved itself to be, needs to be superseded by a more inclusive grouping based on an intensive comparative study of morphological features and lexical elements. The recognition of 50 to 60 genetically independent "stocks" north of Mexico alone is tantamount to a historical absurdity. Many serious difficulties lie in the way of the task of reduction, among which may be mentioned the fact that our knowledge of many, indeed of most, American languages is still sadly fragmentary; that frequent allowance must be made for linguistic borrowing and for the convergent development of features that are only descriptively, not historically, comparable; and that our persistently, and rather fruitlessly, "psychological" approach to the study of American languages has tended to dull our sense of underlying drift, of basic linguistic forms, and of lines of historical reconstruction. Any genetic reconstruction that can be offered now is necessarily but an exceedingly rough approximation to the truth at best. It is certain to require the most serious revision as our study progresses. Nevertheless I consider a tentative scheme as possessed of real value. It should act as a stimulus to more profound investigations and as a first attempt to shape the historical problem. On the basis of both morphological and, in part, lexical evidence, the following six great groups, presumably genetic, may be recognized:

- I. Eskimo-Aleut
- II. Algonkin-Waakshan { Algonkin-Wiyot-Yurok  
Kootenay  
Wakashan-Salish
- III. Na-dene (Haida; Tlingit-Athabaskan)
- IV. Penutian { Californian Penutian  
Oregon Penutian  
Tsimshian
- V. Hokan-Siouan { Yuki  
Hokan  
Coahuiltecan group  
Keres  
Tunica group  
Siouan-Yuchi-Muskogian  
Iroquois-Caddoan

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VI. Aztec-Tanoan { Uto-Aztekan  
                          { Tanoan-Kiowa

This leaves the Wailatpuan-Lutuami-Sahaptin group, Zúñi, and Beothuk as yet unplaced. The lines of cleavage seem greatest between IV. and V., and between III., on the one hand, and I. and II., on the other. Group V is probably the nearest to the generalized "typical American" type that is visualized by linguistic students at large.

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