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STIMULUS DIFFUSION

By A. L. KROEBER

I PROPOSE to discuss a particular form of the widely occurring process of diffusion or spread of cultural material. Diffused culture material often contains concrete or specific elements by which the fact of diffusion can be subsequently recognized even in the absence of a record of the event. In some cases it happens that the diffusion is definitely piecemeal; only fragments of a larger complex or system reach the affected culture or are accepted by it. In this event, the fragments or isolated items may be put into an entirely new context in the culture which they enter. Such partial bits may diffuse more widely than the patterns or systems or complexes of which they form a part. In the inter-influencing of cultures, it must frequently happen that a new item or specific trait fills some need or is of obvious advantage in a culture which has not previously possessed it; or at any rate that there is nothing already established with which it would have to compete for acceptance. On the other hand a system or pattern, being a larger thing, is more likely to encounter a corresponding system already in operation. Even if much of a system is of such a nature that the receiving culture might be hospitable or neutral toward it, there may be items within the system which the receiving culture will resist with sufficient vigor to preclude acceptance of the system as a whole.

The type of diffusion which I am now about to examine is in some ways of an opposite kind. It occurs in situations where a system or pattern as such encounters no resistance to its spread, but there are difficulties in regard to the transmission of the concrete content of the system. In this case it is the idea of the complex or system which is accepted, but it remains for the receiving culture to develop a new content. This somewhat special process might therefore be called "idea-diffusion" or "stimulus-diffusion."

Obviously this process is one which will ordinarily leave a minimum of historical evidence. In a great many cases in history, as just pointed out, evidence as to the process of diffusion is much more scant than of the effects. In other words, much diffusion takes place below the surface of historical record. The evidence for it is therefore indirect or inferred, although the conclusions may be none the less indubitable. With idea-diffusion the

situation is different, because while systems or complexes in two or more cultures may correspond in functional effect, the specific items of cultural content, upon which historians ordinarily rely in proving connection, are likely to be few or even wholly absent. Positive proofs of the operation of idea-diffusion are therefore, in the nature of the case, difficult to secure long after the act, or wherever the historical record is not quite full. Theoretically they would be best observed in contemporary culture, were it not that the culture historian necessarily lacks perspective in interpreting the contemporary; he cannot discriminate, in the flux that surrounds him, which features will develop and lead to further effects, and which will prove to have been only transient fluctuations or abortive starts.

Fortunately, however, we possess a few cases that are at least near-contemporary and supported by a fair degree of factual evidence.

1. One of these instances concerns the invention of porcelain in Europe in the early eighteenth century. Chinese porcelain had been coming to Europe for nearly two hundred years and naturally excited admiration. A definite goal was accordingly set: to produce porcelain without the heavy expense of import from China. The problem was to find the necessary materials at home and to develop the required technical skills. After a considerable period of conscious experimentation the necessary kaolin deposits were discovered, first in Germany and then elsewhere in Europe, and the specific technologies needed were developed.¹ The consequence is that we have here what from one angle is nothing else than an invention. Superficially it is a "parallel," in the technical language of ethnology. However, it is equally significant that the invention, although original so far as Europeans were concerned, was not really independent. A goal or objective was set by something previously existing in another culture; the originality was limited to achieving the mechanisms by which this goal could be attained. If it were not for the preëxistence of Chinese porcelain, and the fact of its having reached Europe, there is no reason to believe that Europeans would have invented porcelain in the eighteenth century, and perhaps not until much later, if at all.

2. Another historic example is furnished by the invention of the so-called alphabet, really a syllabary, for the Cherokee language by Sequoya, or John Gist or Guest or Guess, about 1821.² Although part white in blood,

¹ First by Böttger and Tschirnhaus in Dresden, 1708-09.

² Grant Foreman, *Sequoyah*, (a biography), (University of Oklahoma Press, 1938). J. C. Pilling, *Guess*, in *Bibliography of the Iroquoian Languages* (Bureau of American Ethnology, Bulletin 6, 1888). *Sequoya*, in *Handbook of American Indians* (Bureau of American Ethnology, Bulletin 30, Pt. 2, 1910).

he grew up without knowledge of English and without schooling. He did become impressed with the advantages which writing gave to the whites and resolved to provide the Indians with a corresponding instrument. The result was his singlehanded creation of a new system of writing. In this he discarded the alphabetic character of English writing and substituted a syllabic one. It is not clear why he made the substitution. It is true that a syllabary more easily represents the Cherokee language than a syllabary would represent English, because Cherokee lacks the heavy consonant accumulations so characteristic of English. However, the fact that syllabic writing did readily represent Cherokee satisfactorily is in itself no reason which enforced the choice of the syllabic system, for modern linguists have no difficulty whatever in writing Cherokee with a suitable alphabet. It is therefore possible that Sequoya's choice of a syllabic system, which involved a change from his model, rests upon a psychological fact, namely that non-literate peoples have again and again been found able to syllabify their words on request, that is to break them up without difficulty into their constituent syllables, but are in general unable to break up the syllables farther into the constituent elemental sounds or phonemes. They can of course be taught to do the latter, but rarely if ever make the analysis spontaneously.

Sequoya's choice also constitutes rather strong internal evidence that, while he had picked up some facts about the system of English letters—he is said to have had a spelling book in his house—his knowledge remained so deficient that he had not grasped the alphabetic principle. If he had, he would almost certainly have applied this principle with such minor modifications as seemed to him desirable to make it fit the sounds of Cherokee. At any rate the degree to which culture conditions the individual makes it possible if not probable that this is what would have happened if Sequoya had started with adequate control of English writing. He would in that case have been no more than an adapter or applier—a sort of supplementary inventor. That he altered the basic principle of writing stamps him as a person of originality capable of a primary invention.

However, it is clear that if it had not been for the presence of writing in the Caucasian civilization with which he was in contact, Sequoya would certainly never have had the objective or goal of a system of writing arise in his mind. In this sense his original invention was dependent upon culture contact, and is an example of diffusion as well as of invention. It seems that this case exemplifies very well the appropriateness of the terms stimulus-diffusion and idea-diffusion.

Moreover, we have tangible evidence that diffusion was operative, in

the fact that Sequoya included among the symbols of his syllabary many characters of the English alphabet. He did not draw upon the whole of the alphabet, and those letters which he adopted are sometimes turned upside down, or sometimes lower-case instead of capitals. Of course he needed more characters to represent the syllables of his language than twenty-six. His system contains eighty-six symbols. Some of these, besides those taken over from English, appear to be modifications of English letters; others are devised outright, without visible relation to English characters.³ In no case does a character borrowed from English retain its English phonetic value. Thus A is written for the sound cluster "go," B for "ya," C for "tli," D for "a," and so on.

It is thus clear what happened. Sequoya took over from Caucasian civilization not only the goal or objective of his invention but certain of its specific items or content like the shapes of particular letters; but, operating on a new principle, he "misapplied" these borrowed items, so that their value or function in the new system was quite different—wholly arbitrary, or we might say erroneous from the point of view of the system which induced them.

For this reason if we had no information whatsoever about Sequoya and his life history, but had subsequently discovered the Cherokee writing as a system of whose history we knew nothing, it would be difficult to decide whether or not the Cherokee system was a derivative from the English (or Roman) one. Culture historians would almost inevitably seize upon the identical symbols like A, B, C, as possible evidence of connection; but then would be baffled by the fact that these symbols both have a non-concordant value in Cherokee, and form part of a system constructed on a fundamentally different principle. The chances are that historians might therefore in such a case harbor suspicions of influence, but, being unable to account for much the larger part of the Cherokee system by transmission, would consider the case for connection unprovable.

3. It is an interesting fact that there is a fairly close parallel to Sequoya's diffusion-invention in Africa, only a little later, before 1849, among the Vei or Vai of the Liberian coast. Here, too, writing and its utility were ob-

³ 18 characters are English capitals, 2 are numeral signs (4, 6), 3 are inverted capitals (J, V, Y), 7 are minuscule or lower-case English letters, and the remaining 54 are about evenly divided between modifications of English capitals (usually by the addition of one or more strokes) and free inventions. Most of the latter consist of curve combinations somewhat in the manner of rounded and heavily shaded English handwriting, but without being reducible to specific letters of the alphabet.

served by a native, Doalu Bukere, who, having in his youth experienced a few months of missionary schooling in English, set himself the task of devising a system for his countrymen suited to their native language. As the result of a divine dream, or during it, he devised a syllabic system of more than 200 characters, which for a time found enthusiastic acceptance.⁴ It is not necessary to go into this parallel case except to remark that if we did not know its specific history, and if by any chance of history the Vai had been cut off from continuance of European influence but had happened to preserve their writing, its origin when discovered at some later time would also undoubtedly have been a puzzle, and perhaps an insoluble one, for historians.⁵

4. It is a natural step for inference to pass from these historic examples of the origin of systems of writing to those whose origin is still veiled in obscurity. Not that we can use the principle of idea-diffusion to assign a specific origin to Egyptian or Mesopotamian or Chinese writing, but the principle does at least come into consideration as a possibility. Particularly is this true when we find writing appearing on the cultural scene at more or less the same time in countries so close together as Egypt and Mesopotamia. The time-space relation is such as inevitably to suggest a connection. On the other hand, the Egyptian and Mesopotamian characters, their sound values where they represent sounds, and in part the principles employed are so different that all attempts to derive cuneiform from hieroglyphic or vice versa have been rejected as insufficient and forced. It is, however, entirely possible that after writing had developed in one of the two areas, knowledge of the possibility and advantages of writing was carried to the other area; and that because of this stimulus someone in the second area devised a system to fit his native language, customary thought processes, and available technological materials; with the result that the specific system evolved was totally or preponderantly diverse from the one which had stimulated its invention. As between Mesopotamia and China, the geographical gap is considerably greater, and the lapse of time between first

⁴ S. W. Koelle, *Grammar of the Vei Language*, (1854); G. W. Ellis, *Negro Culture in West Africa*, (1914).

⁵ In this case, only ten or a dozen characters bear resemblances to European letters or numerals, and in practically no case is there complete identity of shape. Koelle's specimen of the syllabary appears to be lithographed from his own hand-written copy from native text. The strokes are heavy, and straight, angular, or in simple curves. Ellis's specimen is much more cursive in quality. Whether it, or the original form, has been influenced by Arabic writing, I am incompetent to say. Certainly Koelle's sample does not look so. But he makes clear that the Vei were in contact with Mohammedans, and that the inventor recited Arabic prayers.

appearances is probably also greater. However, the system of strokes composing the characters is undeniably somewhat alike. That Terrien de Lacouperie's old attempt to show a connection through specific similarities of form and meaning of characters is a failure, may be unhesitatingly accepted along with the majority of scholars. Nevertheless, there does remain the possibility of a real connection through the transmission of the idea of writing and of this acting as a stimulus toward an original but induced local invention, presumably in China.

5. Even our own, the so-called Phoenician alphabet, may well have been the product of this same process. It is well known that alphabetic symbols for the complete consonantal scheme occur in Egyptian as a minor factor within a system of several hundred characters, most of which denote syllables, whole words, or ideas without reference to sound. This mixed system had been in use for two thousand years before someone hit upon the idea that ninety-five per cent of the apparatus of the Egyptian system could be discarded and any or all words could be written, at least in their essential consonantal outline, with twenty to twenty-five phonemic characters or letters representing sound elements. In this case the essence of the invention was the discarding of what was unnecessary. Once this idea entered the mind of some Phoenician or other East Asiatic, he might conceivably have taken over the Egyptian consonantal letters, or characters from cuneiform or some other system of writing already in use, and started with these; their form however changing, during the early development of the alphabet, sufficiently that when we encounter the first preserved inscriptions some centuries later, the letters are so altered that they cannot with certainty be referred to Egyptian, cuneiform, or any other original models. An alternative possibility is that the inventor started fresh: that he invented his symbols as well as his scheme; or, like Sequoya, only partially borrowed the already existing letter symbols. If this is what happened it would of course be impossible to derive the Phoenician alphabet from Egyptian or any other writing by the usual method of tracing specific links of evidence, because in that case the links of evidence never existed. This second alternative must be recognized as a possibility; and if continued efforts to derive the Phoenician alphabet from other writings yield only negative results, the possibility of its origin being due to stimulus-diffusion will be correspondingly enhanced.

6. The history of Japan furnishes several cases of possible stimulus diffusion from China. There are of course many known cases of Japanese derivation of cultural items and systems from China. The time required for

the transmissions varied heavily, ranging from about a century to a millennium or more.⁶ This variability must be held in mind. It is not necessarily an argument against a stimulus diffusion having occurred because a Japanese institution appears later by a short interval or by a long interval than the corresponding institution in China. In other words, decision as to the authenticity of a possible connection must be made, in the main, on grounds other than the lapse of time.

Three forms of dramatic art are recognized in Japan; the No or religious drama, the puppet play, and the secular drama with human actors. The last two, however, are best treated as one in the present connection because they grew up and culminated simultaneously and in part had plays written for them by the same authors. As a literary form, therefore, they are essentially identical even though the stage performance is different. On the other hand the No and the secular drama are separate growths. The No has religious associations, is built up very considerably out of citations from extant poetry, and was aristocratic in its sponsorship. The secular drama does not attach to shrines or religious legend, creates its own poetry, and appealed to the bourgeois or plebeian classes. The No originated toward the end of the fourteenth century, reached its culmination early in the fifteenth, and has been preserved ever since as a conscious and cultivated archaism. The secular drama began to develop about 1600, reached its peak about 1700 with Chikamatsu, and then slowly declined in quality although continuing to prosper in appeal. More or less is known about its origins: it grew up locally out of at least two ingredients: public recitals accompanied by music, or romances chanted in a sort of free verse; and dances for entertainment.

The origin of Chinese drama appears to be very little known. Certain literary legends may be discounted. It is, however, clear that this drama suddenly appears in rather fullblown form and with wide appeal under the Mongol Dynasty. However rapid its rise, the first development therefore probably occurred before any literary recognition at all was accorded it. Even subsequently the drama was never admitted to classic Chinese literature. The earliest extant play, but one only, is ascribed to Sung times. The florescence is put under the Mongol Dynasty, with some prolongation into earlier Ming. We can safely say, therefore, that the origin falls into the

⁶ Thus, in approximate centuries, the lag is: block printing, 1; Sung style painting, 2-3; end of retainer burial, 3; official recognition of Buddhism, 5; movable type printing, 5; Neo-Confucian philosophy, 5; money minting, 10; bronze, at least 11; writing, at least 15; abolition of feudalism, 20.

thirteenth century, or at least not later than the thirteenth century, and the culmination by or before 1400.

This allows an interval of a century between the Chinese drama and the Japanese No, and of more than three centuries to the secular Japanese drama. Of the two, the latter is more similar to the possible Chinese prototype. The No is very thoroughly different in form, subject, manner, and status. Nevertheless it is conceivable, especially in view of the constant drift of features of Chinese culture to Japan, that the No represented an original Japanese creation in response to the stimulus of knowledge of dramatic performances in China. This is the more likely because the No was to a considerable extent developed by two individuals, Kwen-ami and Se-ami Motokiyu, father and son, who also brought its narrow and specialized form to highest perfection. They would, in short, more or less correspond to Sequoya as individual inventors. With the No it is not a question of broad currents affecting a considerable segment of the Japanese population.

It is also possible that Chinese stimulus acted upon the later secular drama rather than upon the No; or that it twice affected Japan.

7. There is, however, another possibility for the secular Japanese drama: European influence. The first origins of this drama are dated about sixty years after the arrival of the Portuguese in Japan. By the time the drama had developed well characterized forms, the policy of isolation was in force. However, there remained one permanent Dutch trading colony, and there were imports and exports, naturally mainly of specialties, curiosities, and luxury articles. There was at any rate enough intercourse for the possibility of knowledge of lay dramatic performances being introduced to Japanese consciousness. Certain resemblances between the plays of Chikamatsu and of Shakespeare have been noted. He has been called the Japanese Shakespeare, not only because of his preëminence but because of nameable qualities of resemblance such as in vigor, strength of dramatic conception, and looseness of construction.⁷ These resemblances are too vague to count for much as evidence of connection. It is extremely unlikely that any translations of plays of Shakespeare reached Chikamatsu or his associates, although they wrote a full century later. It is however conceivable that with the knowledge which the Dutch continued to impart to at least sections of the Japanese population, there may have been included some knowledge, not necessarily wholly in the abstract, of dramatic performances. The Dutch themselves possessed a school of drama which culminated about the middle of the seventeenth century. It is clear that the

⁷ W. G. Aston, *Japanese Literature, 1899* (1933), p. 278.

evidence is too tenuous to allow of the case being pressed; but the possibility of a connection by diffusion is sufficient to warrant further investigation. I would not go so far as to suggest that the Japanese secular drama in its entirety was due to European stimuli. Certainly the use of puppets was not derived from Europe but from Asia. In the same way the plays with living actors were gradually crystallizing out of dances and recitals, as a native development, a century before Chikamatsu. I am suggesting nothing more than that after the formation of dramatic patterns on a purely Japanese basis was under way, the development may have been furthered and precipitated by added European stimulus example.

8. This case would accordingly be somewhat parallel to that on which we have some evidence in the history of native pottery in what is now the American Southwest. We possess a rather full archaeological record of pottery in Pre-Pueblo and incipient Pueblo times in the San Juan drainage in the Southwest. Unfired clay or mud with fiber temper was first used in housebuilding, then for lining baskets, then in shapes of its own and with sand replacing the vegetable tempering; only after which does fired pottery appear, and then painting.⁸ If we had only this single piece of archaeological history, we should inevitably conclude that pottery developed independently and on the spot in the American Southwest. Nevertheless, the consensus of American archaeologists has been to give greater weight to the fact that Pueblo culture shows innumerable resemblances to that of Mexico. Maize and probably most of the other cultivated plants are Mexican in origin. Masonry buildings, ball courts, religious ritual, and the like have Mexican parallels and almost certainly antecedents. The mass of such evidence is so great that it cannot be left out of account. It is, therefore, entirely possible that both explanations are true: that the ancient Pueblos or Pre-Pueblos were groping toward pottery when they received the reinforcement of more developed skills reaching them from Mexico.⁹ Or, it is conceivable that the first gropings took place in an endeavor to reproduce pottery which was known from the South but without precise knowledge of the involved skills—something as Europeans groped for a time to imitate Chinese porcelain.

9. Let us, however, return to Japan and the possible effect of European, especially Dutch, influences. The Japanese seem to have remained unconscious of their grammar until the latter half of the eighteenth century,

⁸ E. H. Morris, (*American Museum of Natural History, Anthropological Papers, No. 28, 1927*), pp. 125-198.

⁹ Or, according to excavations at Snaketown, from the Hohokam of southern Arizona, on the route from Mexico.

when Motoori in 1779 started its development. His grammatical works appear not to have been translated, and it is therefore impossible to adduce internal evidence which might be decisive. The Japanese at any rate believe that Motoori originated the conscious analysis of the structure of his native language. Derivation or stimulus from China is out of the question because there is no Chinese grammar. Native Chinese linguistic efforts were in the nature of the case directed to description of the phonetic aspects of writing and to discovery of the tones. These were accomplished in the third and fifth centuries after Christ.

There was, however, during the eighteenth and early nineteenth centuries, a small group of Japanese scholars who specialized in western learning for the national benefit. They worked under tremendous handicaps, both from lack of materials and of instructors. It does seem probable, however, that at least one copy of a Latin or vernacular grammar or philological work would have been among the number of books that reached this class of western scholars. In fact it would be highly improbable that this had not happened. And through this source a stimulus, a realization of the idea that such a study as grammar was possible, perhaps even an actual model, however imperfectly translated or understood, may have set Motoori's mind in operation to make its original creation.

In this instance it is probable that proof or disproof can be brought. A comparison of Motoori's grammatical works with Latin and Dutch grammars of the preceding century might show decisively whether in his concepts and categories he did or did not draw upon them—like Sequoya with his English-shape characters.

10. The following is an instance of direct, not stimulus, diffusion, but it has a certain pertinence. In the thirteenth century, as the Sung Dynasty was tottering toward its end, there developed in China a quite unique form of algebra. This operates on principles pretty thoroughly different from those of Greek, Arabic, and European algebra, and its antecedents are completely obscure. When it emerges into the historic record, it is already functioning in a well characterized pattern. Its development continued for about two generations, reaching its climax just after 1300; beyond which no further additions seem to have been made. The entire duration of the activity, so far as is known, therefore, falls into the period 1245 to 1305. After this it tended to go out of use. Sixteenth century scholars in commenting on it showed that they no longer understood it; and still later, it dropped out of scholarly mention. It was not until after 1800 that the Chinese were able to recover the works of their greatest master in this field, Chu Shih-chieh, partly from Korean sources.

At some time between 1300 and 1600, this algebraic art was carried to Japan. Shortly after 1600 we find the Japanese beginning to take it up and develop it further. The principles are those of thirteenth-century China, but the Japanese quickly raised the art to a higher pitch. The greatest master was Seki Kowa, 1642-1708, who has been compared to Newton, and at any rate was a contemporary of Newton. This algebra continued through the eighteenth century, in fact until 1868;¹⁰ but it seems to have exhausted the fundamental possibilities of its pattern after 1750 and to have gone off into specialties and refinements. The Japanese had apparently completed the activity by the time they decided to westernize.

In this instance there is no doubt that the Japanese began where the Chinese left off. There is continuity of specific activity and performance in spite of the gap of three hundred years. But it is entirely obscure why this algebra stopped developing in China before it had been pushed to its limits, and why three centuries later, after they had presumably long had access to the Chinese works on the subject, the Japanese suddenly took the activity up and carried it farther. It may be added that in both cases the art was of the people. That is to say, it did not emanate from the scholarly class in China or the corresponding aristocracy in Japan. It did not enter into the official educational system of either country. The participants were private individuals and largely of the middle classes. In both countries too the art was essentially an end in itself. It seems to have been used in relation neither with scientific inquiry nor with technological development. This concentration of the activity upon itself very likely helped its intermittent flaring up and dying away.

As already said, this is not a case of stimulus diffusion; the connection is proved. The specific stimuli that led first to the Chinese and then to the Japanese growth are obscure. But the idea of such an algebra lay dormant in Japan for some time, then suddenly became influential, and further development resulted. It is the awakening of the idea or method, its revivification, one might almost say its reinvention, that furnish a partial parallel to the preceding cases.

11. A number of tantalizingly vague parallels between Greece and India have long troubled culture historians. There may be other connections which have not even been suspected. If, for instance, fifty years ago anyone had ventured to assert specific Greek influences in Indian and Far East Asiatic art, he would have received little attention. The discovery of actual remains of Gandhara art in Northwestern India completely changed the

¹⁰ The last great name is Aida, 1747-1817; the last of the line, Hagiwara, 1828-1909.

situation. Here were abundant remains of sculpture from the earlier Christian centuries, and ranging by all conceivable intergradations from almost pure Hellenic or Graeco-Roman statuary to pure Buddhistic in the native Indian manner. Discoveries in central Asia uncovered corresponding links between Greek and Gandhara art and that of China and Japan. It is still difficult for the layman to see any but the most vague resemblances between a Chinese Kwan-yin and a European Madonna. The specific stylistic qualities of European and Far Eastern art remain very fundamentally different in two such pieces of statuary. Nevertheless the archaeologist and historian of art can trace specific connections which cannot be denied. This is not saying that a Kwan-yin is a Chinese attempt at a replica of the Madonna. It does mean that specific influences within the field of sculpture, and probably painting, did get across from the Far West to the Far East. How far the western influences are responsible for the beginnings of plastic art in India and China, and how far they merely shaped and colored native developments that were already under way, is another question, and one that is harder to answer; partly because historians take up most of their time either in proving the specific connections, or in having to speculate about the scanty evidence that remains from the period previous to western influences.

At any rate, one inference may be drawn from this example: that contacts did occur and that they did have influence far beyond what we could directly infer from the preserved documentary literature. In other words the absence of direct historical records as to connections between Greece and India is no proof that there was no connection.

Whether the Hindu drama was derived from or stimulated by the Greek drama has long been a matter of debate. The dates permit of such a derivation. The earliest Indian references are to the first century after Christ, the earliest preserved specimens from the second, and the culmination occurred under Kalidasa soon after the beginning of the fifth. The time interval is therefore ample for connections to have been operative. The internal evidence is inconclusive. Direct historical testimony is completely lacking. Western recorders would not have been much interested whether the classic Greek plays performed in the Greek Bactrian kingdom about 200 B.C. were or were not followed by Sanskrit imitations in India three or four centuries later. Nor would the Hindus, with their culturally self-centered attitude, be interested in the fact that the beginnings of their drama had been stimulated from abroad. The question has been reviewed at length by Winternitz¹¹ on

¹¹ *Geschichte der Indischen Literatur*, 3: 174 seq., 1920.

the basis of previous monographic studies.¹² His conclusion is ambiguous. There does seem to be agreement that if there was influence it was not from the great classic drama of Sophocles and Aristophanes or Menander but more likely from the later Greek *mimus*. We can leave the matter there.

12. At an earlier period we find the Pythagorean theorem appearing in the Hindu *Sulva-sutras*. As usual in India, the date of the *Sulva-sutras* is highly problematical: the range of estimates is from the eighth century before to the second after Christ.¹³ The theorem appears in quite different context, in connection with the construction of altars, and in a number of numerical applications instead of as an abstract geometric theorem. On the other side there are elements in the Pythagorean cult which have generally been construed as non-Greek: the reputed transmigration of souls, for instance, the taboos on certain foods, the whole cult or school-like character of the movement. The question accordingly is in this case a two-way one: did some knowledge of incipient Greek geometry reach India to be embodied in the *Sulva-sutras*; or did Indian philosophy affect Pythagorean mathematics, doctrine, and cult?

13. Soon after Buddha's death monastic orders seem to have been in full operation in India. It was ascetics in retirement from profane affairs that seem at all times to have directed the historic fortunes of Buddhism. In the west there were monastic communities in Palestine at the time of Christ: the Jewish Essenes since about 150 B.C.; and definite monastic organizations became prominent fairly early in the history of Christianity, especially in fourth-century Egypt. The principle got a firm hold in Latin Christianity only some centuries later and did not reach its full development there until the high Middle Ages. So far as I know there is no proved historic link between Buddhistic Monasticism and Near Eastern-Christian monasticism; but the relation of space and time, as well as of intrinsic concept, is such as to make one inevitably think of a connection. After all the fundamental idea of the institution is a simple one, and it need not have impressed more than one or two individuals of unusual intensity of conviction and persuasiveness, for them to apply it in the setting of an entirely different religion, and, when the "time became ripe," for the institution to take root and flourish.

14. I might mention one other possibility of Greek-Indian connections of the type which we are discussing: the development of quantitative meter in India. As is well known, all Greek poetry, so far back as we have record of it, is quantitative. Latin poetry made itself quantitative in direct imita-

¹² Such as H. Reich, *Der Mimus*.

¹³ Further, the older and younger portions may differ in age by as much as three centuries.

tion. Classic Sanskrit poetry is also quantitative. The two great Sanskrit epics, on the other hand, count syllables, but they do not arrange long and short syllables into rhythmic patterns. The basic plan of verse structure is much as in the Romance languages, where form is also determined by the number of syllables but without consideration of whether the syllables are long or short. Roughly, it may be said that Sanskrit poetry of the pre-Christian centuries counted syllables, that of the post-Christian centuries measured them.

Now the question arises whether this development in India represents an internal growth or may also possibly have been stimulated from Greece. The former is usually assumed. However, if on fuller analysis of data it should prove more positively probable that Hindu drama or early Hindu mathematics, or both, were influenced from Greece, the presumption of a connection in the matter of verse form would obviously also be strengthened. Not that a case can ever be proved by parallel ones; but the prospect of an additional connection being provable is necessarily enhanced by previous cases. I admit that origination from mere stimulus is more difficult to conceive in the case of the fundamental form of poetry than for most of the matters so far considered. One would imagine that before a new and strange verse form could appeal sufficiently to anyone for him to wish to apply it in his native language whose poetry was based on other forms, he would have to be subjected to considerable exposure to the alien type. Strictly, therefore, in such an instance we would have stimulus plus exposure. It seems doubtful whether the idea alone and as such could take root in a new special medium. In the case of Latin quantitative meter we know that this was introduced by Greeks or by South Italians who had been under Greek influence; and it seems almost inescapable that there were non-Greek Italians and even some Romans who knew Greek and had been exposed to the swing of Greek poetry in the first half of the third century when the innovation began to be introduced. But from what we know of the general historic setting it can hardly be imagined that the few Greeks in India or the fewer Greek-speaking Hindus set themselves to introduce quantitative meter in India. The mechanism, therefore, remains obscure even if we entertain the possibility of the fact.

Classic Persian poetry of the Middle Ages is also quantitative. This makes four great Indo-European literatures whose poetry is built up on the quantitative principle. It has therefore sometimes been supposed, and was natural to suppose, that quantity as a poetical instrument was an original Indo-European inheritance which spontaneously came to the surface as soon as literature reached sufficient development. However, quantity in

Persian literature was quite evidently taken over directly from Arabic, just as were rhyme and strophic forms and many themes. What happened here, accordingly, is a parallel of what happened in Latin; with only this reversal, that the influenced Romans were the conquering people and the influenced Persians the conquered nationality. Nevertheless, with two of our four cases eliminated, it is clear that the interpretation of the spontaneous growth of quantitative verse out of something inherent in the nature of Indo-European speech, must be abandoned. If we add the fact that early Sanskrit poetry is not quantitative and that the first appearance of quantity in India is centuries later than in Greece, it does look as if the situation called for an examination of the problem whether all Indo-European quantitative verse may not go back to a single origin among the Greeks.

15. However, the problem is not yet finished. Arabic poetry, so far back as we know it, is also quantitative. We have preserved the works of a number of Arab poets from the century before Mohammed. These works show a very definite form indicative of a previous development; but all record of earlier stages has been lost. Through the accidents of the fortunes of historical preservation we therefore have quantitative Arab verse appearing suddenly about 500 A.D. Now how did such a special form-pattern grow up in backward Arabia? Earlier Semitic and Hamitic poetry does not seem to rest on quantity. Its forms are both less strict and quite different. In 500 A.D., however, the Arabs had been just beyond the frontier of the Hellenic civilized world for eight centuries. It does seem at least possible that in some manner of which all record has been lost, the quantitative pattern of poetry managed in these eight centuries to get itself transferred from one language to an entirely different and unrelated one, and from highly civilized to definitely backward peoples. I admit that on first impression such a hypothesis seems fantastic. It violates all our preconceptions as to the embedding of poetic form in speech medium. Further, it must be granted that in this case the mechanism of transfer to a non-Indo-European language is more difficult to imagine than between the common Indo-European languages of Greece and India; though as the quantitative pattern passed from Arabic to Persian, it might also have passed from Greek to Arabic. Moreover, there is in this case no geographical gap as between Greece and India: the Arabs and the Greeks of Syria and the Roman Empire were in actual permanent contact and communications. I therefore submit the possibility for what it may be worth. Further knowledge may strengthen or eliminate it.

16. While we are on the subject of poetical form, a few words may be said about rhyme. The origin of this is a vexed problem. It appears, apparently independently, in Arabic and in early Latin church poetry. It appears

gradually, and considerably later, in the vernaculars of Europe, often after passing through the stage of half-rhyme or assonance. I do not wish to enter into the difficult problem of interrelationships between these literatures. There is intricate evidence that bears on these problems and I am incompetent to handle it. We do know, however, that Persian poetry, beginning about 900, grew up in imitation of Arabic poetry and took rhyme over from it along with other features. Somewhat later, toward the twelfth century, rhyme begins to appear in India, and the later poetry of India, especially in the vernacular, is both rhymed and quantitative. In fact, the Hindus characteristically pushed the device toward its logical limit, demanding double-syllable rhyme and often using triple. In the works which I have consulted I do not recall a direct statement to the effect that Indian rhyme was taken over from Persia. But in view of both the geography and of what we know of relations after 1000, all the probabilities would be against the Hindus having developed their rhyme independently. Presumably the principal historical problem would be whether they derived it from Persian poetry alone or from both Persian and Arabic.

So that I may not be interpreted as advocating a single origin for every set of related phenomena in history, I wish to add that there is one other development of rhyme which I consider unquestionably independent of those so far mentioned. In China rhyme is well marked in the earliest preserved examples of literature. This antedates by a millennium and a half the first known examples in Arabic or Latin. Moreover, the nature of the Chinese language is such, with its phonetically limited number of syllables, which are also words, that both rhyme and syllable-counting were devices that were bound to be obtrusive. A third factor which is ready to hand in Chinese, and available to serve poetic form, is tone. However, the Chinese did not become formally conscious of their tones until the late fifth century,¹⁴ and soon thereafter, by or during early T'ang times, did add them to their repertory of poetic devices.

It is accordingly impossible for rhyme in China to be derived from rhyme in the west; and while the reverse is theoretically possible, I am not even suggesting it. The gap in time and in space is too great. Moreover, it would be unfortunate to adopt a negativistic attitude toward independent origins. All I am arguing in this essay is that independent origins are not necessarily proved because we are unable to prove specific connection by specific historical documents. There is bound to be a category of cases which are indeterminate, or indeterminate at present; and what I am pro-

¹⁴ Discovery attributed to Shen Yo, 441-513.

pounding is that in at least part of these indeterminate situations the principle of stimulus diffusion may have been operative.

17. As Chinese tones have just been mentioned it may be worth dwelling upon them a few moments longer. It is really rather remarkable in the abstract that the Chinese should not have been aware of their tones until the fifth century after Christ. It is of course theoretically conceivable that older Chinese was non-tonal and that the recognition of tones came late because the tones developed late. However, I do not know that any authority has suggested this, and it seems unlikely for as late as post-Christian times. I will therefore venture another suggestion. That the Chinese did not develop a grammar or linguistic analysis of their spoken language is natural enough in view of the extreme paucity of strictly grammatical features in Chinese. They did, however, possess an intricate system of writing their language, and from a fairly early time devoted considerable effort, as well as ingenuity, to organizing their knowledge of the written system. After about the beginning of the Christian era Buddhist influences became strong in China. The Buddhist texts were in Sanskrit, and along with the texts, or following them, there was introduced some knowledge of grammar as worked out in Sanskrit. This form of grammar would have been both difficult and sterile to apply outright to Chinese. But I suggest that what may be called philological curiosity and interest were stimulated by it; that the Chinese for the first time became speech-conscious as well as writing-conscious; and that the result was the discovery of the tone system which is so characteristic of their speech. Theoretically this case is of some interest because if my suggestion is valid, Panini, who lived in Northern India probably between the sixth and fourth centuries B.C., is brought into historic connection with Shen Yo, who discovered tones in China toward 500 A.D. The results of their activities are necessarily so different on account of the divergence of the languages, that ordinary inferential historical evidence would prove nothing as to the connection if the connection did exist. Historic documentation could give us evidence upon this problem only if it happened to be so precise as to give us details as to the training and educational influences to which Shen Yo and his predecessors were exposed.

It is, of course, on the whole easier for a foreigner than for a native to become conscious of the structure of a language. In principle, therefore, it is entirely conceivable that the first recognition of tones in Chinese was not made by Shen Yo, to whom the discovery is attributed, but by Indian or other non-Chinese Buddhist monks who learned Chinese in order to translate their scriptures into it, and that Shen Yo is simply that Chinese scholar who first became aware of what the foreign missionaries had recognized

and thought it worth-while or profitable to announce to his countrymen.

According to the usual accounts, the addition of the new or tonal poetry to the older verse forms came in with the T'angs. This would be roughly a century after Shen Yo's so-called discovery. T'ang literature was tonal poetry written by men trained in scholarship. It is therefore entirely possible that the addition represented a conscious experiment: a transfer from formal philology to formal poetry. On the other hand, it may be believed that in China as elsewhere changes did not always originate at the top; that there may have been developments which went on below the surface and were given official and literary recognition only after they became an accomplished fact. It might therefore be that both the philological recognition of tone and the use of tone in poetry are only common functions of such a sub-official growth. Which alternative is the truer one could perhaps be readily determined by any competent Sinologist interested in bringing together all the relevant data.

18. The whole history, the world over, of the growth of linguistic self-consciousness to the point of the development of an analysis of structure, or what is ordinarily called grammar, seems to go back to a small number of origins. Arabic grammar is derived from the Greek, probably via Syrian; and Hebrew grammar is patterned after Arabic and follows it in time. The various European vernaculars one after the other had their grammars determined after the analogy of Latin and Greek, or of one another. Modern comparative linguistics is little more than a century and a half old and essentially represents the extension of analysis of languages first examined individually. If we tentatively accept the suggestion just advanced that Chinese philology is derived by stimulation from Indian sources, and Japanese from European, there remain not over two wholly separate first origins of grammatical study: one in Greece, the other in India. This immediately brings up the question whether these two cannot be connected.

Priority in time certainly goes to India. The date of Panini has been variously estimated from the eighth to the fourth century.¹⁵ Whatever his absolute time, Panini represents a refined development, not a first beginning. His grammar is very thoroughly worked out, skillful, and technically competent. It must have had predecessors; and he refers to predecessors. In Greece we find the first timid grammatical conceptualizations appearing toward the end of the fifth century. By the time of Aristotle the system has developed somewhat but is still far from complete. It is not until the second

¹⁵ 350 B.C. is the most usual estimate, the fifth century has some support, and the seventh and even eighth centuries have been suggested. His predecessor, Yaska, is mostly set somewhere between 700 and 400.

century before Christ that Greek grammar in its full classic form was worked out by Crates and Dionysios. It is probably significant that this completion was the work of a Cilician and a Thracian: that is, of men to whom Greek presumably was an acquired rather than a mother tongue, or who at any rate were probably bi-lingual. It is psychologically less difficult to analyze a system in whose use one has not become automatic.

However, I hesitate to draw the inference that Greek grammar owes its development even partly to stimulation from the earlier Sanskrit example. The case would be much stronger if we had positive knowledge of other diffusions in the same direction, either direct or idea diffusions. Internal evidence, in the shape of apparently borrowed categories, seems also to be lacking. Perhaps it has not been looked for; at any rate it has not been adduced. And finally we have the hesitant developmental steps within Greek itself. *Per se*, this argument need not be conclusive. I have refused to accept it as decisive in the case of Southwestern pottery. But as a reinforcement of lack of other evidence, it must have some weight. It would perhaps be going too far to make a positive pronouncement in favor of complete independence of the Greek and Indian growths of grammar. It is always impossible to predict what new evidence, or the analysis of old evidence from a new point of view, may bring forth. Still, the situation appears to resolve itself preponderantly in favor of no connection.

I am fully aware that the principle of stimulus or idea diffusion can be abused. It could easily be invoked for wildly speculative leaps of historic fantasy. However, this cannot be helped. Those who will speculate on minimal evidence will no doubt continue to do so whether they use the principle of stimulus diffusion or some other principle as a pole with which to vault. If stimulus diffusion does take place, it is a process which it is necessary to recognize. Some focusing of attention on it as a principle will no doubt help to delimit its nature and its scope. Any over-estimations of the principle may be expected to show themselves as such, and ultimately to help in the delimiting. After all, in the last analysis it is a matter in each case of how much evidence there is, and whether the evidence is construed with ordinary reasonableness.

It is also well to remember that while diffusion in space, like transmission in time, is an exceedingly common process, it is not something that operates automatically. There are selective factors making for and against diffusion, of which we are beginning to have some comprehension. There are also a number of mechanisms involved in the process; and these it is obviously desirable to distinguish, as far as possible. Idea diffusion is only one

of these mechanisms, and probably a rather special one. After all, diffusion happens so frequently and so continuously that we know more about its results than about its operation. We can often be sure that diffusion has been effective, as evidenced by internal part-for-part similarities, when we can only guess its route or carriers or reasons. More understanding of the types of mechanism through which the generic diffusion process operates will certainly be worth having; even though in the case of the particular mechanism here discussed we may mostly be on difficult ground. Stimulus diffusion may be provable in only a minority of the cases in which we can suspect it. But we do have some indubitable instances of its operation. I suggest nothing further than the desirability of open-mindedness toward other possible instances. With more awareness of the mechanism and more experience in dealing with it, we should gradually become better able to distinguish the probable and the improbable instances of its operation.

Finally, the process is of interest because it combines development within a culture with influence from outside. It contains the element of invention in the wider sense, as well as that of diffusion of a special kind. What is really involved in every true example of stimulus diffusion is the birth of a pattern new to the culture in which it develops, though not completely new in human culture. There is historical connection and dependence, but there is also originality. Analogically, ordinary diffusion is like adoption, stimulus diffusion like procreation, with the influencing culture in the rôle of the father; though by strict rules of historical evidence paternity is sometimes clouded. In essence, stimulus diffusion might be defined as new pattern growth initiated by precedent in a foreign culture.¹⁶

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¹⁶ Two other possible instances might be mentioned. One, which I owe to the suggestion of Paul Benedict, is the historically wholly isolated script, or rather scripts, of the Lolos in China. The other is the rise of Christian iconoclasm in Byzantium about a century after this empire came into contact with image-condemning and puritan Mohammedanism.