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FACT AND FETISH IN CREOLIZATION STUDIES:  
HERSKOVITS AND THE PROBLEM OF INDUCTION, OR,  
GUINEA COAST, 1593

Two things strike me in reading some of the work cited by Richard Price (2001) in his retrospective on creolization in the African diaspora in the Americas.<sup>1</sup> First, although Melville Herskovits's research on New World Africanity figures prominently either as source of inspiration, object of criticism, or merely useful signpost, his equally influential writings on economic anthropology and their possible relation to his work on African survivals in the New World are largely absent from the discussion.<sup>2</sup> Second, in a debate oriented largely around differing interpretations of "much the same data" (R. Price 2001:52), and in which attention to "what went on in specific places and times" (Trouillot 1998:20) is paramount, there is little reflection on the status of the facts *as facts* and the modality of inductive reasoning in which historical particularism makes sense. In the case of African survivals, the problem of induction is particularly acute since the data that might be admitted as evidence are rarely straightforwardly evident to the senses.<sup>3</sup>

1. I would like to thank Jennifer Heung, Rosemarijn Hoeffte, Tom Boellstorff, Kevin Yelvington, and the reviewers for the *NWIG* for their comments, criticisms, suggestions (and photocopies!) of some sources, as well as their general collegial assistance. Writing has been supported by the Centre for Women's Studies in the Faculty of the Arts, and the Department of Anthropology in the Research School of Pacific and Asian Studies, both at the Australian National University. Research on Frank Knight and economic theory was supported by the National Science Foundation, Law and Social Sciences Program, SES-9818258. I alone am responsible for all errors and inconsistencies here.

2. See e.g., Jackson 1986 and Fernandez 1990, who make a brief mention of this body of work; Apter 1991; Scott 1991; Trouillot 1998; R. Price 2001; see also Khan 2001; Yelvington 2001.

3. I would like to thank Kevin Yelvington for suggesting this phrasing.

This essay represents a series of speculations on some possible connections between the absence of Herskovits's economic anthropology in discussions of African survivals, and the relative lack of reflection on the mode of reasoning of historical particularism. It is meant in a spirit of critical reflection on the categories of analysis we use when approaching cultural formations in the African diaspora. David Scott (1999:108) has recently maintained that "a critical anthropology of the African diaspora has to be constituted through a close attention to the history of its own categories and to the extent to which it assumes their transparency." Kevin Yelvington (2001:250) has written eloquently about the "paradigm paralysis" in the field of Caribbean studies even as its models become the stock in trade of the new anthropologies of globalization, transnationalism, and hybridity. Aisha Khan (2001:278) critically reviews the "discourses of celebration and lament" that have characterized the search for African "retentions" and the solace some scholars find in creolization models of culture change, a solace only possible if the models remain relatively untheorized.

My provocation here has two components. The first is theoretical and methodological and has to do with the status of facts and facticity in Herskovits's economic anthropology, which became the centerpiece of his debate with the economist Frank Knight in the *Journal of Political Economy* (Herskovits 1941a; Knight 1941). At issue was the relation between deduction and induction, the positing of principles and the discovery of facts. The second is historical and cultural and has to do with the emergence of the idea of the fact as a stand-alone datum independent of any theory for its existence. By linking recent work on fetishism with new scholarship on the scientific revolution, I tentatively suggest that this emergence coincided with that of the idea of the fetish as an object possessed of a power all its own.

As for the debate over African diasporic cultures and their continuities or divergences from African and/or slave pasts, I take a page from the Mintz and Price (1992) score, but put it to a new tune. What strikes me in the debates over creolization is less the contending political and epistemological positions different scholars have taken in relation to certain foundational texts, and more the "unconscious 'grammatical' principles, which may underlie" (Mintz & Price 1992:9-10) not only the influence of African cultures on New World cultures (which was Mintz and Price's original concern), but the very debate itself. There is a meta-grammar here, and it structures the debate's conflicting positions (and possibly its acrimoniousness [R. Price 2001:36] as well). Creolization is so intractable a problem because it re-stages the founding problematic of scientific induction: how do we draw conclusions from facts? Like other complex social phenomena, and the theories built up in tandem with them, it is by no means unique in doing so. Induction, in the specific form in which we have inherited it, I suggest, was not only the product of the scientific revolution in Europe but was also brought into being on the slave-trading West African coast, in colonial ventures that provided raw material for the new sciences and that were key to the

emergence of the distinction between fact and fiction. That distinction, in turn, was triangulated by the figure of the (commodity) fetish.

A word about my own stakes in the creolization debate: having worked primarily in the British Virgin Islands, a place that scored pretty low on Herskovits's (1930, 1966) "scale of intensities of New World Africanisms" (about which I have more to say below), and having worked primarily on law and economics, I originally found the creolization problematic in Caribbean studies to be largely irrelevant to my concerns – or those of British Virgin Islanders. It is important to make the analytical distinction between creolization as a process and creolization as a strategic discourse of elites or others. The BVI is, after all, a place that has placed its bets on a self-conscious strategy of "creolization redux," that is, promoting itself as a de-creolized piece of England in the Caribbean for the purpose of attracting and maintaining its offshore financial services industry (see Maurer 1997a and 1997b). Beyond the dominant discourse, however, offshore financial services in financial practice and legal form are difficult to see as in any way "creolized."<sup>4</sup> However, the manner in which offshore finance encourages both professionals involved in the business and everyday BVI Islanders to pay reflexive attention to culture, status, and monetary and legal forms points up the analytical need for questioning the origins of those forms and their contemporary transformations. Finance capital, as a specific modality of legal, bureaucratic, and economic power, after all, has some of its origins in the shipping requirements of Caribbean slavery (Stinchcombe 1995:57-58). Today, creative reworkings of the formal qualities of economic and legal facts occupy those I study as well as myself, just at a historical moment when, in what are coming to be called creoles and hybrids, they take center stage in new capitalisms around the world (see, e.g., Pieterse 1994; Ong 1999; Yang 2000).

#### CREOLIZATION DEBATES

In their reassessments of the Herskovitsian paradigm, both Andrew Apter (1991) and David Scott (1991) revisit the "scale of intensity of Africanisms" Herskovits put forward (Herskovits 1930, 1966). To both Scott and Apter, the scale of intensities recalls nothing so much as the nineteenth-century scientific racism that Herskovits and his teacher, Franz Boas, were at pains to argue against. For Scott (1991:277), the scale represents "the inaugural moment of a lasting anthropological problematic" according to which the "New World Negro" would be configured as an anthropological object defined by distance from an authentic past and divergence from a distinctive culture. For Apter (1991:237), the scale is

4. For instances of creolized economic forms in Herskovits's work, see Herskovits (1941b:161, 165) and Herskovits & Herskovits (1947:290-92), on labor sharing and rotating credit associations.

almost a “parody [of] the epistemology of liberal social science.” As a “form of knowledge” and a “specific discursive modality” for constructing Africanity, it cries out for deconstruction (p. 244).

Apter (1991:236) attempts just this, by “extract[ing] the interpretive kernel from its scientific shell” in order to achieve “greater clarity about just what it is we are comparing, contextualizing, and historicizing on both sides of the Atlantic.” What Apter reveals is a form of interpretive practice (especially bodily practice, p. 242) from West Africa that itself challenges the trope of fragmentation on which the creolization paradigm is predicated with a Yoruba-derived one of indeterminacy, empowerment, and appropriation. Thus, for example, “slaves took possession of Catholicism and thereby repossessed themselves as active spiritual subjects” (p. 245). His conclusion is that “the revisionary power of syncretic religions derives from West African hermeneutical traditions which disseminated through the slave trade and took shape in black communities to remake the New World in the idioms of the old” (p. 255-56). Apter (1991:243) is not as concerned as Scott with the factual status of the “data” on which his own or Herskovits’s conclusions are based. The assumptions behind the “scale of intensity” may have *distorted* the facts. But recovering a theory of creolization from them does not require “the lofty heights of postmodern criticism” so much as “internal evidence supplemented by empirical data” (p. 244). In taking this position, Apter is perhaps closer to Herskovits (and Mintz and Price) than not.

For Scott, however, it is just such evidence and data that themselves come under scrutiny. Flagging the problems of Boasian empiricism and holism, Scott (1991:274-75) calls for a move away from the “preoccupation with the corroboration or verification of authentic pasts” and toward the discursive formations that figure realities and relationships (p. 278). Scott’s shift leads him to the place of Africa and slavery as discursive constructs in the rhetorical and political struggles of people of the New World African diaspora. The focus becomes an appreciation of the place of the past in memory and the emplotment of positions and persons in cultural and political fields (p. 279). Scott does *not* suggest that his proposed shift in focus lead us to consider the discursive formations conjuring the “facts” of the past, in their status as facts of an actual history, but rather the spaces of memory and traditions of representational and ideological work.

Had he done so, Richard Price’s response to Scott in his recent critical retrospective might have been a little different. Price (2001:53) reads Scott’s “radical critique” as denying “the primary object of historical study – pasts that exist independent of a cultural imagining of them.” Price is correct to argue that Scott’s critique moves from histories that exist independent of imaginations and toward memories and discursive traditions of imagining histories. Price’s own work, of course, is exemplary of such a move (e.g., R. Price 1983, 1990, 1998). But because Scott does not turn his critical apparatus toward the process of fact-making itself, Price is not, strictly speaking, correct to link Scott’s shift to memory and discursive tradition to an outright “discard[ing] ... the facts of eight-

eenth-century demography or colonial statutes or accounts of tortures meted out to recaptured Maroons” (p. 53). What matters to Scott, it seems to me, is the place of memories of the past in the present. The same could be said for Price, given his continued attention to the imagining and resituating of pasts in the present, and one wonders whether the two authors’ positions are as divergent as their responses to each other might suggest.

What matters to me, however, is the place of the facts of the past (or the present, for that matter) as products of a specific discursive political economy. Other interventions in the creolization debates recently have made the models an object of scrutiny but have left the “data” on which they depend relatively unscathed, and, indeed, have left in place the presumed directionality between data and models: data come first, and models are built up “after” them, as it were. Yelvington (2001:250) notes that some recent work attempts “to steer paths through materialistic determinism and cultural production through ethnography and revisionist historiography.” Khan (2001:294) argues that the models have served academic gatekeeping functions that “overdetermine Caribbean realities, overshadowing the contingencies of local contexts and daily life.” Her essay brilliantly demonstrates how creolization is “a fiction that invents the Caribbean” (p. 295). But I am less convinced by the grounding function given to “ethnography and revisionist history,” the supposition that “the gritty realities of people’s lives complicate theorizing experience” (Khan 2001:293) or the call for greater attention to “the ethnographic, the material, the concrete” (p. 293). I find such invocations of gritty reality unconvincing not because I deny the “reality” of the material or the concrete, but because I do not believe the distinction and the presumed directionality between “real” experience and theorizing experience is a tenable one. The notion of experience giving access to some truths – whether lived or theoretical – comes from a specific historical and philosophical tradition, about which I have more to say in the final section of this essay. I am interested in the gritty realities, and the facts like those Price mentions – colonial demography, legal statutes and the like – because of their effects in the present (R. Price 2001:53; Khan 2001:292; Yelvington 2001:250-51), and because of their efficaciousness *as facts* for political and academic agendas. That efficacy tells us something important about the sorts of facts analysts presume, look for, and find in their divergent approaches toward creolization. And that efficacy leads me to want to look more closely at that discarded “scientific shell” of Herskovits’s scale of intensity.

#### HERSKOVITS AND THE PROBLEM OF INDUCTION

“It is quite possible on the basis of our present knowledge to make a kind of chart indicating the extent to which the descendants of Africans brought to the New World have retained Africanisms in their cultural behavior:” thus

Herskovits (1930:149) in the 1930 essay in which he originally proposed a chart for what would become, in a 1945 article, the “scale of intensity of Africanisms” (Herskovits 1966).<sup>5</sup> The shift from descriptive prose to a tabular matrix is significant, for, as Jack Goody (1977:81) has argued, the table imposes a kind of form on otherwise discontinuous items, bringing “greater visibility to categories, at the same time as making them more abstract” (see also Riles 2001:155-56). In the 1945 chart, the column headings refer to different cultural elements: technology, economy, social organization, kinship, religion, magic, art, folklore, music, and language. The row headings refer to places or regions, such as Guiana (bush), Guiana (Paramaribo), Brazil (Porto Allegre), Jamaica (Morant Bay), Jamaica (general), U.S. (rural South), U.S. (Gullah Islands), and so forth. Each cell of the table is occupied by a letter, from *a* to *e*, indicating “intensity” of African influence, or a question mark indicating that no data are available.

The 1930 article in which the scale was first proposed was firmly within the Boasian problematic, framed by “Man’s physical form, and ... his languages and cultures” (Herskovits 1930:145). The 1945 essay, more confident in tone, turns directly to problems of “clarity of purpose in research,” “methodological concepts and techniques,” and “hypotheses which have guided investigation and developed out of experience in the field” (Herskovits 1966:43).<sup>6</sup> According to Herskovits (1966:50), the scale of intensity is a “logically conceived continuum which ranges from retentions that are completely African, or almost so, to those least African and most European.” Furthermore, it is

but a means to an end rather than an end in itself. In this case, the end that is envisaged is that comprehension of process which alone can lead to valid prediction. To be revealing in terms of this end the classification must be derived through induction, and flow from the data, rather than be imposed upon it after the fashion of *a priori* categories that tend to force materials into groupings that do violence to the scientific reality. (Herskovits 1966:51)

In the 1945 article, Herskovits (1966:45) repeatedly stressed the inductive and empiricist orientation of his endeavor. For example, he lauded research in which, despite any current academic fashion, “the data have been followed where they led,” and emphatically asserted that “the most fruitful results can be had only when the facts are studied as they lie, without those preconceptions which ... lead to distortion” (p. 46).<sup>7</sup>

5. Originally published in *Afroamerica*, vol. 1, pp. 5-24, the article was reprinted in *The New World Negro* (1966).

6. Page numbers refer to the 1966 reprint.

7. Herskovits’s wariness of preconceptions and biases of all kinds helps account for his conflicted relationship with certain partisan causes. Yelvington (2000) has documented this in relation to Herskovits’s Jewishness. See also Herskovits’s response to Leslie White’s quoting him out of context regarding evolutionary theory. Herskovits (1960:1050) main-

The 1945 essay is characterized by an almost defensive reaction against sloppy or careless anthropologists, and critics who would challenge either the “scientific” status of the study of culture or the justification for a field devoted to the “New World Negro.” If there is any defensive rhetorical posturing in the 1930 essay, it is toward the scientific racists with which the Boasian school took issue. Nevertheless, the approach remained strictly Boasian: build a rich “body of factual materials” (p. 44) for comparative research that will permit the formation of hypotheses. By 1945, it seems Herskovits felt there had been enough data collection across geographical and disciplinary areas to warrant theory-building. He thus departed from Boas’s equivocation on the proper moment for beginning the comparative project and jumped right into it.<sup>8</sup> Like Richard Price’s (2001) recent essay, the article was a retrospective, and a prolegomenon for new research, a “logical moment for stock-taking, for the explicit statement of theoretical assumptions, and for a refinement of techniques” (Herskovits 1966:44). The chief hypothesis that results from the scale was that “in situations involving change, cultural imponderables are more resistant than are those elements of which persons are more conscious” (p. 60).

Despite the imperative to stick to “just the facts,” Herskovits demonstrated some discomfort with the facts as arranged on his logical continuum. In addition, despite the strong emphasis on historical particularism and induction from the data that “only await gathering to be utilized” (Herskovits 1930:145), Herskovits also equivocated on the role of classificatory schemes. Although he stated that, “Scientific analysis is impossible without classification of data, and herein lies the importance of this series of categories; but it cannot be too strongly emphasized that classification, of itself, can tell us nothing about causes, or relationships, or the processes of change” (Herskovits 1966:51), a closer look at the explanatory notes to his scale of intensities suggests there is more going on than simple fact-collection and classification. This conclusion is warranted by some of his other writings, as well.

There are three dimensions to the scale. First, there is the classification of relative degree of “intensity” (represented by the letters *a* through *e* on the

tained that White’s quotation misrepresented his “approach to scientific controversy and ... sense of the dignity of scholarship” by juxtaposing it to a rather “intemperate” sentence by another author who claimed the theory of cultural evolution to be “inane, sterile and pernicious.” Herskovits was arguing here over the importance of language and, in effect, discursive community in scholarship, a point relevant for my larger argument that Herskovits’s encounter with Knight underscored the role of argument in science and unsettled somewhat Herskovits’s inductive method. I would like to thank Kevin Yelvington for bringing these debates to my attention.

8. Kevin Yelvington (personal communication) suggests that by 1945 Herskovits was extremely confident in the abilities of himself and his students to make such comparisons, and thereby to surpass the Boasian paradigm of deferring comparison until that impossible moment when “all” the facts are in.

table). Herskovits (1966:54) relied for these classifications on his own field research and that of "trained and competent observers." The classifications here are abstractions from the facts derived from field research, and are designed, not to approximate truth, but to minimize argument. As he puts it, the "weightings ... are broadly conceived" to reduce "disagreement" among scientists that would be "to no purpose, since all classifications of such data must be subjective" (p. 54).

Second, there is the classification of group, region, or locality. But even something so apparently straightforward as locality cannot so easily be captured in the classification scheme.<sup>9</sup> In an earlier formulation of the scale, Herskovits noted, he had lumped together some of these groups and areas. Here, however, he achieved a "further refinement" in cases where "districts can be distinguished wherein the pattern and degree of African retentions differ." This further refinement was still not sufficient to capture the complexity of his cases, however, for "in every part of the New World where Negroes live, excepting only the Guiana Bush, class differences operate so as to make for variation" within each group. Hence, in coming to an assessment of each group or area's level of intensity, he relied on "that degree of retention for each group which is closest to African custom." Although people in Bahia generally speak Portuguese with few elements of African grammar or vocabulary, nevertheless Bahia was rated as having a high intensity of Africanisms in its language because "only there have certain African tongues been retained" as opposed to simply a few words or phrases or grammatical structures (all quotations in this paragraph are from Herskovits 1966:52).

Third, there is classification of "cultural elements" into technology, economy, social organization, religion, and so on. Herskovits wrote that while "greater refinement" here might be "more revealing," "the technique of trait-analysis seems to be too mechanical, and to work too great violence to the unity of the cultural elements involved." Furthermore, his designations were "for convenience only, and to consider them as anything more than useful symbols would be to introduce a note of spurious accuracy against which too great warning cannot be given" (p. 54). As with the classification of levels of intensity, the classification of cultural elements was designed with the goal of minimizing argument. It was also designed to avoid "violence" to cultural "unities." And it was designed to prevent the student from ascribing to any of these cultural elements or the ratings they are given on the scale of intensities a "spurious accuracy."

Herskovits's classificatory scheme was heuristic; as such, it sat awkwardly within the inductive method he espoused. While warning against a logical positivism that would insist on the primacy of classificatory schemes or the

9. On the problematic of locality in anthropology, see Boellstorff 2002 and Gupta & Ferguson 1992.

postulation of culture “traits” whose frequencies could then be measured across the societies of the world, (in the fashion Murdock popularized; see Simpson 1973:7), Herskovits’s equivocation about his own axes of classification belied any simple relationship to the “facts” alone. At the same time, following Goody (1977; see also Riles 2001), the tabular form imposed a degree of abstraction and visibility on the categories, and made it seem necessary to find the data needed to replace question marks with letters from *a* to *e*. In other words, despite Herskovits’s protestations to the contrary, once the scale of intensity became a table, the classificatory scheme seemed to precede the data, and not the other way around.

Herskovits’s apparent inconsistency on the question of whether data or theories are primary has strong resonances with his debate with the economist Frank Knight, published in the *Journal of Political Economy* in 1941 and reprinted in Herskovits’s 1952 volume, *Economic Anthropology*. Framed by Herskovits (1952:507) as a study in the contrast between the inductive and deductive approach in the social sciences, the debate represents to me a softening of the strict empiricism that was Boas’s legacy to Herskovits, and a glimpse into the discursive fabrication of the facts of African retention that are at the heart of the creolization debates.

Knight’s criticisms of economic anthropology rested squarely on his vision of economics as a science of principles and laws, not empirical realities. For Knight, Herskovits’s derision of economics for neglecting the vast majority of the peoples of the world was simply beside the point. Moreover, “facts” for Knight paled in importance to logical relationships and the weight of argument. Knight’s own work in economics was very much against the emerging tradition of stochastic modeling (see Mirowski 1989). Like John Maynard Keynes and Joan Robinson, Knight argued that econometricians’ adoption of statistical procedures mistook economics as a science of the epistemologically or logically probable for a science of the empirically observable. Best known for his book, *Risk, Uncertainty and Profit* (1921) which clearly laid out the distinction between quantifiable and epistemological probability, Knight also authored a lesser-known work, “Liberalism and Christianity” (in Knight & Merriam 1945). The latter, published together with an essay by Thornton Merriam, was a defense of liberalism. It was also a defense of the epistemological conception of probability. “Recognizing that truth is a value means recognizing that it is a social category,” he wrote. “Truth is known, tested, and practically speaking defined, by agreement in some community of discourse,” and “real problems of fact are problems of the *worth* of evidence” (Knight 1945:49, emphasis added). Knight considered the use of “utilitarian application of positive science” to be “the worst form of original sin, rationally defined” (Knight 1945:49). As noted in his obituary, Knight believed that “a multiplicity of principles and conceptual frameworks are necessary if we are to know much about human society and hence of ourselves as the ‘social animal’” (Wick 1973:514). As Knight himself put it,

“The position we have to combat seems to rest on the inference, characteristically drawn by the ‘best minds’ of our race, that since natural objects are not like men, men must be like natural objects” (quoted in Wick 1973:514).

Knight’s position against Herskovits thus challenged the latter’s historical particularism, and, indeed, the very need for “facts:”

any intelligent or useful exposition of facts imperatively requires an understanding of principles, while the need for facts in connection with the exposition of principles is far more tenuous, and the “facts” which are really in question need not be facts at all in the sense of actuality for any particular point in time or space, provided they are realistically illustrated. (Knight 1941:516)<sup>10</sup>

Since economics, for Knight, ought to be interested in logical suppositions and conclusions derived from them, “real” facts are not even necessary: “‘authentic’ facts are not necessarily more useful than travelers’ tales based on superficial and largely false impressions – the bane of modern anthropological science – or even outright fiction or poetry” (Knight 1941:517).

While Herskovits’s response did poke fun at Knight’s persistence, as he called it, “in living in a world of [economists’] logical unreality” (Herskovits 1941a:524), he actually conceded a great deal in the debate. Thus:

Only for training purposes do we impress upon the graduate student the principle that the answer to a problem may not be assumed before the data are in. As a scholar grows in experience he comes to understand that the most fruitful attack on a problem is an approach motivated by a flash of insight telling him that a certain body of data should throw needed light on a given problem of his concern in terms of certain derived results. It is the way such interpretive flashes are used, rather than the fact that they are used, that differentiates the scientist from the nonscientist. (Herskovits 1941a:524)

And also:

It is difficult to envisage a human being, no matter how scholarly or how committed to scientific method, who does not exhibit some kind of bias most of the time. In the exact sciences, where data are measurable, we recognize this fact and allow for it, and we call it by the statistical term “error.” In the social sciences, particularly in social theory, we cannot achieve this, and too often merely call names. (Herskovits 1941a:530)

The arguments Herskovits put forward in his debate with Knight recognized the importance of a community of scholars in determining the relative *argumentative* weight of discrete bits of data. “Bias” is integral to the social scientific endeavor. Answers can be assumed before the data is in. And yet, in spite of his construction of the scale of intensity of Africanisms as essentially an argumentative device for creating “facts” for a particular community of schol-

10. Page numbers for Knight 1941 and Herskovits 1941 refer to the 1952 reprint.

ars, Herskovits continued to claim that the real problem was one of collecting good data without forcing them into preconceived logical schemes (or empty spaces on the intensity chart). Such schemes not only threatened to build insupportable flights of fancy but also to do what he called “violence” to cultural unity and scientific reality.

What is interesting to me in this regard is that by 1950 Herskovits was advocating a method based on the collection, indeed, the creation of fictional facts. In “The Hypothetical Situation” (1950), he described an ethnographic technique he had used in West Africa, the Caribbean, and South America. The technique involved posing hypothetical situations to people and asking them to talk about these. In this manner, information on sensitive topics could be obtained in a way that did not jeopardize reputations or relations with other persons or supernatural forces.<sup>11</sup> Herskovits concluded that the technique was “novelistic” in nature, a kind of “projective device” akin to what one finds in literature. “It has long been recognized by students of literature,” he wrote, “that the creative process feeds on experience. Even the most fantastic tale bases its fantasy on reality” (Herskovits 1950:69).<sup>12</sup> As for the possible concern that what the ethnographer will end up with will be pure fiction, Herskovits writes, “No one can make up a social system, a grammar, a theology; at most, he but registers the flow of his imagination over his enculturative experience,” and moreover, after all, “the hypothetical person turns out to be the informant himself, who is freed by the fictional quality of the approach to reveal facets of his personality he would take all pains to conceal were he speaking about himself” (p. 70). Herskovits’s novelistic device recalls Knight’s statement that the facts of poetry are as useful as the facts of particular points in space and time.<sup>13</sup>

#### GUINEA COAST, 1593

Herskovits’s changing theoretical stance reveals a shift from a strict empiricism to an awareness of the place of argument in the making of the facts

11. Since the Herskovitses used interpreters in all their field sites except Trinidad, the method may have been born of necessity and only later theorized. This projective device may have been the only way to collect data on sensitive topics, since informants might have been unwilling to reveal much in the presence of a local interpreter. I thank Kevin Yelvington for this insight.

12. Page numbers refer to the 1966 reprint.

13. Space does not permit discussion of Herskovits’s 1956 essay on the comparative method, which contains strikingly reflexive statements about the nature of anthropological inquiry (e.g., “Do we realize, perhaps, that we ourselves provide materials for the study of the very phenomena which it is our task to investigate?” [Herskovits 1956, p. 72 in the 1966 reprint]).

themselves. Yet the scale of intensity continued to make a fetish of the facts of New World Africanity, informing debates to this day. By way of a speculative linking of arguments going on in other fields, this section, in lieu of a conclusion, proposes that the fact and the fetish, as cultural and analytical objects, belong in the same argumentative space as our discussions over creolization. Irresolutely material, both fact and fetish came into being in the contact zone of the West African coast at the beginnings of the transatlantic slave trade and the blurring of the distinction between persons and things that that trade entailed. Both shared in an emerging logic of commodification and the money-form as infinitely encompassing. And both, I suggest, inaugurate the problem of creolization as a social fact and an analytical enterprise.<sup>14</sup>

As Peter Pels points out, the seventeenth century was book-ended by two travelers' accounts of the West African fetish, the first by Pieter de Marees written in 1602 and the second by Willem Bosman, written in 1702 and much more widely read (Pels 1998:102; see also Pietz 1987:39 and Pietz 1988). It was also, as Pels remarks, the "heyday of the curiosity cabinet and the object displayed in it, the so-called 'curiosity' or 'rarity,'" an object of singular importance in the history of Western conventions of objectivity, empiricism and induction (Pels 1998:102-3; see also Daston 1988, 1994; Raffles 2001).

Indeed, a growing body of literature in the philosophy and history of science demonstrates how Western scientific reason derived from its encounter with the rarity. Not only did rarities raise "classification quandaries" that later resulted in taxonomic systems (Pels 1988:108). They also were central to the epistemological separation of the weight of argument from the supposedly natural facticity of things (Daston 1994). The new sciences of Enlightenment reason depended on the givenness of things-in-themselves that existed prior to any human interpretation or theorization of them (Daston 1988, 1994). "Conclusions that may be based on" such things-in-themselves – evidentiary claims – were effectively separated from the "[data] of experience" – facts, in the modern sense (Daston 1994:262; see also Poovey 1998). The shock of wonder that rarities invoked helped decompose the presumptions of a scholasticism for which every datum was always-already evidentiary for something else, generally, God's design.<sup>15</sup>

14. Herskovits (1941b:69-77) wrote of the fetish that it permeated West African daily life and served to relativize moral assessments of good and bad. It "was for Herskovits a multipurpose fact that was further evidence for Africanisms" (Yelvington, personal communication) in the New World.

15. I am indebted to an unpublished paper by George Collier (n.d.) on the role of curiosities from the New World in transforming European understandings of various "natures," human nature prime among them. See also Greenblatt 1991.

Facticity in this sense owed much to new techniques and understandings of description and recording, as well as a new kind of community of discourse. This community would consist of reasoned men who could verify the facticity of any item of sensory experience by the fact of their agreement about it. Originally framed, the problem of induction began from the problem of determining, through shared experience and consensus about that experience, what exactly was a fact and what was not. It was also, of course, a political problem, as science, thus conceived, posed challenges to established theological and political orders. The textbook example is Hobbes's debate with Boyle over the possibility of a vacuum, a debate coded in terms of, and with profound implications for, the status of royal authority (see Dear 1995; Shapin & Schaffer 1985).

William Pietz's reflections on the etymology of the term "fetish," while perhaps linguistically thin, are revealing because they permit speculation about the relationship between the emergence of the kind of facticity discussed by the historians of science and the kind of fetishism discussed by historians of West Africa. The historians of science cited here all agree that it is during the seventeenth century that the word "fact" took on the particular meaning of stand-alone datum. Prior to this, it referred to something made or achieved, derived from the adjectival past participle of the Latin *facere*, to make or do (*facticius*) (OED). The term "fetish," also derived from *facere*, first appears in Portuguese (*feitiço*) (Pietz 1987:24). As used by fifteenth-century Portuguese traders on the West African coast, it originally meant an object of witchcraft, something made for un-Christian ends. During the seventeenth century the word "fetish" entered other Europeans' vocabularies in the sense of an object possessed of a spirit of its own (Pietz 1987:24), just as "fact" took on the sense of an object possessed of an ontology of its own. The fetish's materiality was invested with a spiritual agency. European travelers, traders, and theologians made distinctions between the idol, which was worshiped as a representation of a deity, and the fetish, which possessed a power in itself (Pietz 1987:36-37). The fact, too, was invested with a spirit: it spoke to competent observers about the order of nature and demanded cataloguing, categorization, and analysis.

The emergence of the fact and the emergence of the fetish, together characterize a Christian European modality of primitivizing Africa and Africans; an epistemology that is predicated on things standing on their own without human mediation; a system of representation that is based on reference to a material reality and produces what we might call reality-effects, tangible consequences for social practice of the presumed referentiality of representations (Pels 1998; Mitchell 2000); and, importantly, a mercantile system both enraptured and confounded by the equivalencies between objects – including persons – that it encountered and enforced on the Guinea Coast. Rarities, of course, were themselves important items of trade, commodities in their own right. "The rarity," writes Pels (1998:103), "is the twin of the fetish: It was not

just born at about the same time, but also duplicated its mercantile features.” Indeed, fact and fetish cohered in an emerging financial system based on the fictions of paper credit (Ingrassia 1998) that were so important to the slaving enterprise. They also cohered in an emerging scientific order based on “promissory naturalism,” the belief that curiosities like African “fetishes” – and Africans themselves – could eventually be subsumed into a classificatory order of natural kinds even if the effort at first seemed impossible (Daston 1994:251).<sup>16</sup>

Part of the European disgust at the African fetish was that it blurred the boundaries between persons and things. As Michael Jackson (1998:77) remarks, however:

They protested too much. Those who were most repelled and outraged by the African’s apparent indifference to the ontological divide between persons and things were, as often as not, engaged in the kind of colonial ventures – such as slave trading – that conspicuously reduced Africans to the status of beasts, chattels, and mere things.

And here’s the rub. If, as Pietz (1985:16) has argued, “the problem-idea of the fetish arose within and remains specific to a particular type of cross-cultural experience first engaging European consciousness in ongoing situations on the West African coast after the fifteenth century,” then the problem of the fetish – and the problem of the fact – belong properly to the field of studies of African slavery, creolization, and the African diaspora.

Moreover, they constitute the field’s founding problematic. Creolization is so intractable a problem because it demands that we come to grips with the question of how we can abstract from a totality certain identifiable bits that we can agree we all “see” doing things like recombining, mixing, moving, and so on. Other social and cultural phenomena present similar problems. Yet what makes creolization so unique is that the phenomenon and the debate depend on the mixing of (cultural, lived) ideas and the very (analytical, theoretical) idea of mixing (cf. Khan 2001). The key term is “agreement,” indexing a community of shared discourse and debate. Consider the meta-grammar of this com-

16. “Fiction” derives from the Latin  *fingere* , to fashion or form. In that sense, its early occurrences in natural history books suggested something closer to the modern notion of fact, when the fact in question was a “natural” product. The *Oxford English Dictionary* thus lists a 1607 occurrence of the term that states that the shrew “is called ... Zissmuss, from the fiction of his voice.” At the same time, Francis Bacon was at great pains to differentiate “fictions” from “facts” – his protestations themselves indicative of the fine shades of meaning separating the two for most of his readers. In his “Advancement of Learning” of 1605, for instance, he admonished those who put stock in belief rather than truth by stressing the modern sense of fiction as falsity, “He that will easily believe rumors ... will as easily augment rumors ... so great an affinity hath fiction and belief’ (Bacon 1974:30; *Adv. Learn.* I, iv, section 8).

munity of discourse. That the languages available for describing creolization share the biogenetic metaphor of Western kinship and the recombinant and equivalence-making properties of the capitalist money-form should give us pause (see, e.g., Maurer 1997b; Strathern 1999). The objects most amenable to study for scholars of creolization have been items of material, non-verbal or linguistic *exchange*; dance or music and principles of movement or rhythm; words and grammatical structures; art and material objects.<sup>17</sup> Perhaps this is because creolization itself emerged in the border-zone of new forms of mercantile and slave exchange and violent cultural mediation. And perhaps this is because the regime of facticity and fetishism that emerged with it concocted certain sorts of things as abstractable and fungible within systems of representation and systems of exchange and circulation.

This leads me back to Herskovits's encounter with Knight's version of the probable. My point is certainly not that Herskovits deconstructed the concept of the fact underlying induction and empiricism. Rather, it is that Herskovits's confrontation with Knight's version of economics – an economics of epistemological, not descriptive, probability – introduced a productive instability into his understanding of just what a fact might be. Namely, it introduced the possibility that a fact might be a fetish, and that an empirical order might be a social convention for a community of scholars seeking to build consensus through the forging of new knowledge practices that just might help them to understand others' and the roots of their own. And it suggested that these two sets of knowledge practices may never have been as separate as they have seemed.

17. Sally Price's (2001) "arthings" and "artworlds," by setting the material in motion in social, ideational, and political systems and networks of critics, dealers, artists, and so forth, thus represents an important supplement to earlier work on art that simply looked for material or aesthetic indicators of presumed African retention. I would like to thank the anonymous reviews for stressing this point.

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