

Sorting is not Categorization: A Critique of the Claim that Brazilians Have Fuzzy Racial Categories

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ABSTRACT

As a result of a spate of studies geared to investigating Brazilian racial categories, it is now believed by many that Brazilians reason about race in a manner quite different to that of Americans. This paper will argue that this conclusion is premature, as the studies in question have not, in fact, investigated Brazilian categories. What they have done is elicit sorting tasks on the basis of appearances, but the cognitive models of respondents have not been investigated in order to determine what are the boundaries of their concepts. Sorting based on appearances is not sufficient to infer the boundaries of concepts whenever appearance is not a defining criterion for the concepts in question, as the case appears to be for racial and ethnic categories. Following a critique of the methods used, I review a terminological and theoretical confusion concerning the use of the terms ‘emic’ and ‘etic’ in anthropology that appears directly responsible for the failure so far to choose methods appropriate to parsing the conceptual domain of ‘race’ in Brazil.

I. Introduction

Do people use the same kinds of cognitive resources when thinking about ‘race’ in different parts of the world? The claim has been made that Brazilians have quite different racial categories from those familiar to Americans, even though the phenotypic contrasts in Brazil are very similar to those in the US. Many social scientists now consider this to have been established, and the notion itself approaches that of a cultural common-

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sense. This paper will examine the evidence on which the claim rests, and will argue that it is not very good. This paper will also make the argument that perhaps we should not expect Brazilian concepts of race to be very different from American ones when the evidence, properly gathered, is finally in. A growing body of evidence suggests that humans are essentialist when it comes to racial and ethnic categories, and that the bias to essentialize such categories may be a universal aspect of human cognition.

Such evidence is being gathered at the intersection of evolutionary psychology and the strands of cognitive anthropology that intersect with it (e.g. Tooby & Cosmides 1992; Cosmides & Tooby 1994; Atran 1998, 1990; Hirschfeld 1996; Sperber 1996; Boyer 1994; Brown 1991). These intersections have focused on what is universal to humans, rather than on the variation, and in so doing have brought at least the possibility of more balance to the conceptualization of humans from the anthropological perspective, which so far has been in the tight grip of the assumptions inherited from the Boasian tradition: human universals are unlikely, and the importance of cultural differences is great (cf. Brown 1991). Significantly, the cognitive turn has made students of culture who are informed by these theoretical turns of events more careful about apparent differences and similarities that may exist between different cultures. The main new bias, however, is certainly to ignore differences in order to focus on what appears general. Perhaps predictably, this risks becoming a new dogma that fails to do justice to the reality and importance of cultural diversity (Gil-White 2001c; Richerson & Boyd 2000) — a grave danger because what may sometimes appear as trifling differences can turn out to have profound effects for the historical trajectories and system behavior of different societies (e.g. Nisbett & Cohen 1996; Kelly 1985). But, that danger aside, there is no question that the cognitive revolution has provided a needed counterbalance, as well as better methods. Cognitive anthropologists have moved beyond mere elicitations or demands that informants introspect about their (often unconscious) mental processes. In their stead has grown a concern for procedures that will make people *use* their cognitive models, allowing us to infer their content and structure.

In a cumulative science, improved perspectives and methods call for a reassessment of earlier conclusions. One such area that may call for

reexamination is the literature on the cognitive models of race in Brazil (Harris et al. 1995, 1993; Byrne et al. 1995; Harris 1970; Sanjek 1971; Harris & Kottack 1963). The consensus that has emerged from this research is that racial categories in Brazil are fluid (interpersonally, and perhaps also inter-temporally variable), fuzzy (without clear boundaries), and ambiguous (admitting of different, imperfectly overlapping labels). To me, this was surprising, because I found ethnic categories in Mongolia to be crisp classical categories with necessary and sufficient conditions of membership, and a very strong overlap across most individuals. Specifically, I found that for the overwhelming majority of respondents, membership in an ethnies was a matter of patrilineal biological descent from another member of the category. It could be that Mongolia and Brazil, being different places in far flung corners of the world, have developed very different cultural models for thinking about race and ethnicity. But it could also be that, had I used Harris's methods in Mongolia, I would have reached his conclusions, and that, if I were to use my own methods in Brazil, the conclusions would be very similar to those I reached in Mongolia. This possibility raises the question of whether the difference is an artifact of the methods chosen, and so we must examine and compare in order to decide which set of methods is better at addressing the central question: what is the native system of categories? That will be the purpose of this paper.

Before I go further, I must clarify what may appear as a confusion of domains. Above I have noted a disparity between the claims of Harris and colleagues, who were investigating 'race', and my own investigations, which have to do with 'ethnicity'. In order for my findings to be truly discrepant with those of Harris, 'race' and 'ethnicity' should be synonymous in *some* sense, otherwise Harris's and my own are just different findings corresponding to different domains of cognition and experience. To clarify this issue I offer the following distinctions.

'Race' in the technical sense. To the biologist a race is a *subspecies*, identified by a morphological and genetic discontinuity between two populations in the same species. The lay categories of race that humans variously think they see in their own species do not meet the criteria of a morphological or genetic discontinuity (see Boyd & Silk 2000; Brown & Armelagos 2001). There are no human races in the technical, biological sense.

The lay category of 'race'. This is a category of people made on the basis of phenotypic attributes. It is a category of people whose bodies 'look' a certain way. By using depictions of humans with different kinds of features and asking his Brazilian respondents to label them, Harris was attempting to investigate lay categories of race. But a lay category of race is always more than *just* a category of people with certain physical traits. Membership in the category is held to be explanatory or predictive of other things. This is why 'Brunettes' are not a 'race' — there isn't much in the way of content or expectations, implicitly or explicitly, associated with a member of the category. It is possible, moreover, that a lay category of race is more than just a phenotypic category with explanatory or predictive beliefs attached; the people who use it may believe that they are looking at a biologically meaningful grouping. That is, laypeople may believe that their categories of race are in fact 'natural', even if the intuition does not carry all of the sophistication of the technical understanding described above. Hirschfeld's (1996) investigations suggest this is so, and that the intuitions to process our phenotypic categories as if they cut the human species at important natural joints are active at a very young age and may even be innate. Whether or not in any particular culture some phenotypic categories are essentialized as natural is an empirical question — one that Harris did not pursue for Brazilian categories.

'Ethnicity' in the technical sense. It is not without controversy for me to advance a technical definition of ethnicity because the experts on this topic actually spend much time trying to undermine each other's definitions. However, I have tried to show before (Gil-White 1999:813) that despite what appear to be lengthy, ink-spilling disagreements between totally opposite camps, all scholars of ethnicity actually implicitly agree with the following definition: "ethnic groups are agglomerations of people who, at a minimum, *represent themselves* as vertically reproducing historical units implying cultural 'peoplehood'."

The lay category of 'ethnicity'. Since, as noted above, analysts recognize an 'ethnic group' when they think they have found a set of laypeople who organize themselves around an 'ethnic identity', there is not here, as in race, the possibility of a sharp distinction between the analytic and lay understandings of ethnicity. But at least one can say that the cultural distinctiveness, the purity of vertical reproduction, and the historical depth

of the groups may be greatly exaggerated by native participants in an ethnic category system, and the analyst has no need to go along with such exaggerations and/or reifications. As before, a lay category of ethnicity may be, cognitively, more than just the claim of a cultural distinction and its mode of reproduction. Laypeople may essentialize their ethnic categories as corresponding to natural, biological cuts of humanity. This amounts to saying that laypeople may *racialize* their ethnic boundaries. Again, whether they do or not in any particular locality is an empirical question.

I have argued before that the extant empirical data favors the interpretation that laypeople generally racialize their ethnic categories (Gil-White 2001a, 1999). And sub-populations characterized as representatives of a ‘race’ in a particular times and places easily develop all the trappings of ethnic groups (e.g. the white/black boundary in the US). This convergence between the two domains is reflected in the past — and sometimes current — practice whereby ethnic groups were referred to as ‘races’. And English is far from being the only language to uncertainly apply the same term to both domains (see below). All of this argues for the relevance of cognitive investigations in the domain of ‘race’ to those which study ‘ethnicity’ and vice-versa. If it seems likely from some evidence that the lay domains of race and ethnicity are really — from the point of view of the cognitive processes that underlie them — one single domain, then we must pay attention to findings that appear to open this to question. My data about the cognition of ethnicity in Mongolia appear to contradict rather directly Harris’s data about ‘race’ in Brazil, and therefore finding out whether this is a genuine contradiction or a methodological obfuscation deserves the attention I will give it below.

II. The cognition of ethnicity in Mongolia

A. Brief overview

I begin by reviewing briefly the methods I used in Mongolia, the results, and their interpretation. The presentation is quite abridged as my concern here is with broad methodological priorities, but the full details may be found in Gil-White (2001b).

My respondents are Torguud nomads in the district of Bulgan Sum, Hovd Province, Republic of Mongolia. Torguuds are a relatively small, Mongol ethnic group, and they share the district of Bulgan Sum with

Kazakhs, who make up about 30% of the population in the district. Like Torguuds, some Kazakhs are nomadic herders and others are town-dwellers. Although there are a few Kazakh families in what is essentially Torguud territory, on the whole Torguuds and Kazakhs are territorially segregated — even Torguud and Kazakh town-dwellers live in separate parts of town. Relations between the groups are outwardly friendly, if for the most part distant. Trade between them is very limited, as most conduct a subsistence lifestyle and their economies are almost identical, resulting in an absence of trade efficiencies. Inter-marriage is entirely absent (in a population of 10,000, there was only one reported case of inter-marriage and I was not able to corroborate it). There is very little day-to-day interaction between Torguuds and Kazakhs, especially among herders, and they don't know too much about each other's customs and norms.

The purpose of my investigation was to find out what Torguuds considered to be the rule or rules for being a member of an ethnic group, whether their own or another one. Much ethnographic data from around the world suggests that criteria of blood are paramount when making ethnic ascriptions. However, it has become something of a tradition in the social sciences to claim that ethnic membership is fluid, contextual, situational, and even rationally chosen (see Gil-White 1999 for a review). I decided not to pit criteria of blood against rational choices, but against enculturation. If enculturation from birth into a group — absent a tie of blood — will not confer membership, then rational choices are a moot point. This is therefore a very strong test of the 'primordialist' hypothesis, which maintains that ethnic actors will require biological descent from a category member in order to consider a given individual a member of the ethnic category.

The procedure consisted in asking a sample of Torguuds the following questions:

Question 1: If the father is Kazakh and the mother Mongol, what is the ethnicity of the child?

Question 2: The father is Kazakh, the mother Mongol, *but* everybody around the family is Mongol and the child *has never even seen a Kazakh*, outside of the father. The child will learn Mongol customs and language. What is the ethnicity of *this* child?;

Question 3: A Kazakh couple has a child that they don't want. They give it in adoption to a Mongol couple when the child is under a year old. Around the Mongol family there only are Mongols and the child grows up *never meeting or seeing a single Kazakh*. He is never told of the adoption *and thinks that his biological father and mother are the Mongol adopters*. He grows up learning Mongol customs and language. What is the ethnicity of *this* child?

Because in the first question my respondents unanimously answered that the child would be Kazakh, revealing a patrilineal model for ethnic ascription, the second and third questions were crafted so as to pit descent against enculturation, with question 3 presenting the most extreme contrast.

The first time around a sample of 59 Torguuds were asked these questions in a setup where the child is male, and the questions were asked in sequence. In a second test, a new sample of 41 Torguuds was asked the same questions but the order was randomized, the ethnicities were switched (so that the father was now Mongol and enculturation Kazakh), and the child was made female. A majority in both tests believed that a child's ethnicity was that of his biological parents (principally his/her father's), no matter what the circumstances of enculturation, or what the child may have been told. For question 3, this majority was a strong one in the second test (76%, versus 59% in the first test), which randomized the order of the questions and therefore did not create an expectation that the researcher, by making the circumstances of enculturation progressively extreme, was fishing for an enculturation-based answer.

Although this establishes that one's ethnic ascription is a matter of patrilineal descent, this might be nothing more than a way to flag one's community of biological origin. To know whether the ethnic categories are essentialized one needs to investigate if respondents attach to ethnic membership the expectation of a behavioral disposition. A subsequent test asked a sample of those who had answered that descent always trumped enculturation whether the child in question 3 would behave exactly like members of its community of adoption, or somewhat different. A strong majority (74%) replied that the child in question 3 could not or would not behave exactly like members of its adopting ethnies, but would instead behave somewhat like the ethnies of its biological parents. This,

despite the child having absolutely no contact with this ethnic, and despite being unaware of any such ancestry. Ethnographic data further suggest that even those who gave explicit enculturation-based answers may have implicit *intuitions* to match those of respondents who gave explicit descent-based/essentialist models (Gil-White, 2001a).

B. Implications for research on ethnic and racial categories

The bulk of these findings suggests that ethnic categories may be ‘naturalized’. It may even be that we treat them as ‘folk species’ categories akin to the categories bear and mouse (see Gil-White 2001a for a defense of this argument; for a different interpretation of ethnic and racial essentialism see Hirschfeld 1996, Gelman & Hirschfeld 1999). Here I restrict myself to commenting on what this means for a reassessment of past methodology to understand ethnic cognition.

Categorization processes in living kinds have two interesting properties where the importance of ‘appearance’ is concerned: (1) category membership is independent of appearances (Gil-White 2001a; Medin & Atran 1999; Atran 1998, 1990; Keil 1989; Gelman 1996, 1991; Medin & Ortony 1989); but (2) membership in a given category nevertheless carries an expectation of category-specific morphological uniqueness (Markman 1989, 1990).

The first point says that appearance is not the criterion for *categorization*, although we may use appearances to guide our first guess as to where a given object belongs. This is because category membership is conceived as resulting from possession of the category ‘essence’ (believed to reside ‘inside’), not a set of external features. It is true that the essence is believed to *cause* typical surface features, including appearance, and therefore that common category membership will lead to the expectation of surface similarity between any two objects. But since this expectation is not *defining*, a common appearance is neither necessary nor sufficient for category inclusion. A dolphin, for example, looks like a fish, but isn’t. Appearances are not a matter of fuzzy membership either. A dolphin’s appearance does not make it *partly* a fish. It is not a fish at all. And this is also not a question of modern versus old biology (Atran 1998). It is our readiness to accept the statement or discovery by an expert source that dolphins are not, after all, fish (and our immediate assumption that there must be something intrinsic

to dolphin ‘nature’ that makes this true) that sets living-kind categories apart — not the actual theory we may have about the taxonomic position of dolphins.

To see the distinction between sorting (which involves identification procedures) and categorization (which involves reasoning about definitional criteria), suppose I present you with a new stimulus and ask you “what is this?” Assuming I give you nothing to go on beyond what is apparent on the stimulus itself, you will give me the best answer you can, with the available information. Using the appearance of the object as your guide, you will provisionally sort it into the category that you think it most likely belongs in, and this will be your answer. That however — and this is crucial — does not mean that the cues you use to make this initial tentative sorting constitute the actual criteria for category membership (i.e. the definition of an object inside the category X is not necessarily equivalent to “possession of the cues a, b, c, . . . , which are used to make the tentative sorting”). If presence of these cues is not coextensive with the definition of a member of category X, then I can give you additional information about that object that may lead you to revise the slot that you had put it in at first (for the difference between identification procedures and concept representation, see Smith & Medin 1981:40-43; Armstrong, Gleitman & Gleitman 1983)

My data suggests that if Dorj, a Torguud Mongol, were to encounter a man who was dressed like a Kazakh, spoke Kazakh, and behaved in every obvious way like a Kazakh, Dorj would think to himself: “Lo, a Kazakh.” However, if told subsequently that this man’s biological parents were both Mongol and that he had been adopted at an early age into the Kazakh community, Dorj would then think: “Aha, so this man is really a Mongol!” Dorj at first sorted the man into the Kazakh slot with the available information. He made the best guess he could make under the circumstances. But finding that Dorj makes such guesses does not mean that Dorj thinks “if it talks like a Kazakh and walks like a Kazakh, *then* it is a Kazakh.” Clearly, Dorj doesn’t think this. A sorting guess made on the basis of appearance, and *categorization*, are not the same thing.

It is precisely the fact that appearances are insufficient for categorization, plus the expectation of behavioral regularities resulting merely from the fact of descent from Mongols (in the absence of any contact with

Mongols), which suggests that we are looking here at an essentialized 'natural kind' category. The fact that the 'essence' is taken to be passed on reproductively suggests a species-like category, for in such categories membership is unequivocally established if the condition for having the essence has been ascertained, which condition amounts to biological descent from another category member.

Notwithstanding the fact that appearances are insufficient for categorization in 'living kinds', there is certainly a perceived causal link between the essence and a given typical appearance; two 'living kind' categories will be expected to have different appearances by virtue of their having two different essences, even if appearance is not defining for any one individual. This may create a bias bigger than the baseline category accentuation effect (Hogg, 1988:19) that obtains generally and which makes us see members of different categories as more contrasting than they actually are. In Bulgan Sum I found that my respondents unanimously expected the biological child of Mongols, if adopted into a Kazakh community, to look physically different from its adoptive community. However, when I tested discrimination ability with a set of anonymous photographs, I found that there was considerable phenotypic overlap between the groups (Gil-White, 2001b). This suggests that information about category membership may, in living kinds, exaggerate perceptions of dissimilarity.

III. Reassessing prior methodologies

The above findings strongly imply that when it comes to ethnicity, we need to dig deeper than appearances and classifications of appearances. Given the above-mentioned strong similarities and even common semantic blurring and confusion between lay categories of ethnicity and race, as well as experimental data supporting the idea that races are likewise essentialized (Hirschfeld 1996), the same admonition applies to racial categories.

There is now a corpus of work on the basis of which many scholars believe that racial categories in Brazil are quite different from their counterparts in the US (Harris et al. 1995, 1993; Byrne et al. 1995; Harris 1970; Sanjek 1971; Harris & Kottack 1963). The main contention is that racial categories in Brazil are highly ambiguous with very fuzzy boundaries. In light of my findings, I shall now argue that this conclusion is premature

and, given the methodology employed so far by Harris & colleagues, not yet supported by any data. I begin with a detailed discussion of Harris (1970) since that paper is emblematic of the others.

Harris (1970) elicited 'racial' classifications from 100 Brazilian respondents by using

... a deck of 72 full face drawings constructed out of the combination of three skin tone, three hair form, two lip, two nose, and two sex types (...). Responses were initiated without using terms presumed to be part of the domain... In general it was found that a question involving a request for the 'qualidade' or 'tipo' or 'raça' of the person depicted in the drawing was adequate to prime the response process. The word 'cor' was used only as a last resort.

There is either little concern for priming the domain of race, or else Harris feels confident that how the question is put will not really affect the results. But to assume that the words 'qualidade', 'tipo', 'raça', and 'cor' — which translate into English roughly as 'quality', 'kind', 'race', and 'color'¹ — are interchangeable as stimulants of responses in the same domain is strange. In Bulgan Sum, for certain, I would not get answers in the same domain if I asked some people "what is the *ündecten* (ethnie/race) of this person?" and others "what is the *öngö* (color) of this person?" It appears that which question was used was *ad hoc* and depended on the idiosyncratic comprehension problems of individual participants. Harris does not code his data according to which responses were elicited by which kind of question, and he reports no evidence of their interchangeability or a test of their effects on responses. He obtained quite a wild variation in his responses — restricting ourselves just to those terms used more than 100 times, these are still a grand total of 12. However, this much variation could be at least partly the product of asking four different questions, not all of them priming the same domain, rather than the product of a large universe of Brazilian 'racial' terms, as he claims.

Harris also finds that there is little intersubjective agreement about the boundaries of the different categories. Perhaps. But by showing people pictures and asking them to classify them he is more likely

¹Whitlam, J., V. Davies & M. Harland (1991). *Portuguese dictionary: English-Portuguese, Portuguese-English*. New York: Harper Collins.

testing identification procedures rather than cognitive models of category membership. It is possible for the entire population to be in intersubjective agreement about what makes a ‘branco’ a ‘branco’, but for different individuals to have different thresholds — on the basis of appearance — for making a guess that a person is branco or not. For example, suppose you and I agree that someone is branco (‘white’) if and only if his/her parents are also branco; however, you and I have different thresholds of increasing brownness at which we stop guessing that someone is a branco. Should we be told the facts of descent, you and I will agree on who is and isn’t branco, but when Harris shows us the pictures, we will make different guesses. Because Harris’s study does not delve deeper than the initial appearance-based sorting guesses, the underlying cognitive models are not really explored, and his data are therefore silent on the intersubjective agreement that may exist for these. Aggravating this problem is the fact that using black & white drawings rather than photographs (or at least more realistic color drawings) is likely to increase the ambiguity in people’s guesses because people are likely to have individually varying biases to imaginatively complete the missing information.

Another possibility is that category X is understood by everybody to have the same conditions of membership, *and* that most of the respondents did not even disagree with each other during the identification process, but people name the categories differently. For example, you and I agree on who is a ‘preto’ (‘black’) but when Harris asks us, I say ‘preto’ and you say ‘moreno’, meaning the same category. The looseness of intersubjective variability in *naming* of the category is not the same as looseness in identification of the category or its membership requirements — especially if one usage is more polite than another and some respondents are trying to be polite. In Mexico, for example, people of African descent are usually referred to as ‘negros’ but, more politely, ‘morenos’ or ‘prietos’ (the two latter words have other usages and *that* is what makes them polite). Similarly, in the US, it would not be sensible for a researcher to assume intersubjective disagreement if some respondents identified a picture as ‘African American’, others as ‘black’, and yet others as ‘colored’ — it is anything but far-fetched to suggest that many such respondents would agree if questioned further that they were talking about the same category. Harris interprets his data as pointing to differences in American and

Brazilian racial categories, but a little introspection suggests that applying his methods in the US would result in qualitatively similar data.

Yet another possibility is that some of these words may function sometimes as racial category labels and other times as literal *color* descriptors. This seems plausible for the word ‘moreno’ (‘tanned’ or ‘brown’), and it may account for Harris’s (1963) claim that sometimes full siblings in Brazil receive different ‘racial’ labels. The terms almost certainly don’t function as ‘racial’ labels at all in these cases. It should be obvious that not all phenotypic distinctions are ‘racial’. When we speak of blondes and brunettes we are not, by that token, separating people into different ‘racial’ categories. Likewise, what Harris is referring to almost certainly involves people making non-racial phenotypic distinctions between siblings, and the terms used then function merely as labels to distinguish the surface appearance of siblings naturally considered to be of the same ‘race’.

This is certainly how color labels function *within* ethnic/racial categories in Mexico, where I grew up. In Mexico the upper-class is overwhelmingly ‘white’. Among whites, I am considered ‘moreno’ because I have dark eyes and hair, and white but usually olive skin due to easy tanning. One of my sisters had blond hair when she was young and still has blue eyes and very white skin. Among whites, she was called, as a girl, a ‘güera’, which is the term used by this group for any blonde. However, to an Indian or a mestizo we are *both* ‘güeros’ (‘whites’) and that is what Indians and mestizo’s who don’t know me and who want to attract my attention always call me ‘Hey, güerito!’ — literally ‘whitey’ (although in Mexico no offense is taken, partly because of the double meaning). The term has this connotation especially for members of Mexico’s lower classes. A similar effect may take place in Harris’s data because his respondents are from different socio-economic strata and may thus, as in Mexico, have class-based usage variations for the same terms.

The residents of Bulgan Sum are at the geographic center of Asia, and in their history they have migrated far and wide. As a consequence they display astounding phenotypic diversity — at least half have either blue or green eyes; there are blondes and brunettes; some have epicantic folds over their eyes, others don’t; there are very pale and very dark-skinned people, etc.; and also every conceivable combination of traits (e.g. very dark-skinned people with green eyes and epicantic folds are not

uncommon). All this naturally leads people to employ, as in Brazil, a quite rich vocabulary of phenotypic description. Individuals are said to be yellow, brown, red, white, black. . . . Many a person's nickname is a diminutive form of these color terms. But to describe a girl as 'yellow' (e.g. 'Shar Oxio' or 'yellow daughter') and her brother as 'black' (e.g. Xaraa or 'blackie') hardly means they put them in different racial or ethnic categories. They are both Mongol.

For another angle, consider that the residents of Bulgan Sum are aware through schooling that Africans exist and some of them have even seen one or two in the flesh because they came to this area as tourists. They refer to them as 'blacks'. The fact that some Mongols are also called 'black' does not imply any ambiguity about their ethn racial categories. *Within* Mongols, 'black' refers to a person's hue, not a person's racial category. This is an elementary observation in Mongolia, where the contrast between a Mongol referred to as 'black' and an African is obvious. The contrast between Mongols and Kazakhs, on the other hand, is far from obvious, and corresponds to an objectively fuzzy continuum comparable to the Brazilian contrast between, say, 'pretos' and the intermediate category 'morenos'. But since the conceptual distinction between Mongols and Kazakhs is not fuzzy, we must consider it at least possible, on the strength of the Mongolian evidence, that in Brazil objective phenotypic fuzziness in the racial domain may likewise not imply a fuzzy conceptual boundary.

Finally, when Harris finds that a given drawing was identified by most as 'branco' but by a substantial minority as 'sarara', does this mean that Brazilian racial categories are ambiguous, or that some respondents are giving the basic-level category and others a more specific color description? To use a comparison example, if he showed them a picture of a laptop computer and asked them 'what is this?', would finding that some people answer 'computer' and others 'laptop' mean that they have different ways of parsing the world? No. In this case it would simply mean that some people chose a subordinate category and others a superordinate category, but that they both agree that it is a computer and, more specifically, a laptop. In the same way, perhaps, a person can be a 'branco' and, more specifically, a 'sarara' (in other words, 'sarara' is the kind of 'branco' that she is). Which interpretation is correct, however, cannot be determined

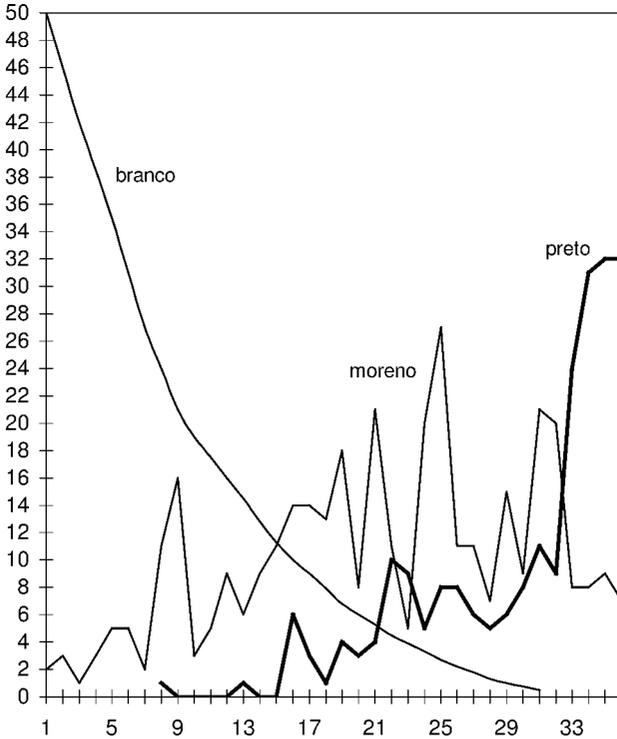


Figure 1. The numbers on the X-axis represent the descending rank-order of the drawings in terms of number of 'branco' responses (Harris's numbers are different because he kept his original numbered identifying labels; the order of the drawings is the same as Harris's, however). For each drawing, the number of 'branco' responses can be found on the Y-axis. The branco curve is a close approximation of Harris's smoothed line. For the other curves, Harris provides the actual data points and they are reproduced here.

unless one looks to see if any of the labels given stand in inclusion relationships to each other. Harris did not attempt this.

The above alternative explanations for Harris's results are certainly not mutually exclusive. A second look at his data will illustrate this.

The figure above superimposes Harris's 'moreno' responses on the 'branco'-'preto' axis. I will refer to the drawings 1-15 as the 'exclusive branco' range because 'branco' is the most common response throughout that range. Similarly, I will call the drawings 32-36 as the 'exclusive preto' range. The 'extended branco' (where 'branco' is more common

than 'preto' but not necessarily more common than 'moreno') is the range of drawings 1-21; the 'extended preto' is 21-36. The 'moreno' range is the set of drawings 15-32. Notice that although the extended branco and preto ranges meet almost at the midpoint, the exclusive preto range is very small because 'preto' responses dip very quickly. This is also because on average, there are more 'moreno' responses inside the extended preto range than in the extended branco range.

How can we explain the above distributions? One interpretation could go as follows. If the term 'moreno' sometimes refers to a racial category intermediate between 'branco' and 'preto' we should see a moreno range in the middle where 'moreno' responses swamp both 'branco' and 'preto'. This is apparent above. If 'moreno', *within* a racial category, simply means 'intermediate' for that category, then we should see a 'moreno' spike in the middle of the branco range, and another one in the middle of the preto range. The first is apparent in the figure above (drawings 8-9). The second might be the spike at 31-32, except that it is not really occurring within the exclusive preto range whereas the first one occurred in the middle of the exclusive branco range. This might be because 'moreno' is sometimes used politely for people who are considered racially 'preto'. So the spike and its base are both raised, the 'preto' curve lowered, and as a result the size of the exclusive preto range is cut shorter. This interpretation is bolstered by the fact that even with the most extreme drawing on the preto side (#36) there appears to be a reluctance to use the term 'preto' (32 instances against 50 of 'branco' for the comparison drawing on the branco side). Harris claimed that there was no relationship that he could find for the moreno responses on the 'branco'-'preto' axis. But superimposing them as I do above (which Harris doesn't do for his readers) suggests this interpretation as a possibility, though it is not one admitting of a statistical test. More data needed to be gathered about the respondents' cognitions for such tests to be meaningful. There is simply too much missing information, and too much potential noise, in this data set.

The main reason for the noise is that Harris has asked his respondents to sort a lot of perceptual stimuli. But sorting is not categorization — that is, not necessarily. To find out if what emerges from a sorting task of perceptual stimuli corresponds to the racial categories we need to investigate how people think about the resulting 'lumps'. If we don't do

this all we have is data about sorting *guesses* that result from identification procedures — these are not necessarily the categories in people’s heads. Harris wants us to accept the data as evidence of a racial categorization system that, by comparison to, say, the American, is very wild. But this is premature. We need to know what the racial categories are. These cannot be inferred willy nilly from appearance categories — it is not written anywhere that racial categories and first-guess piles due to sorting on the basis of appearance should match exactly.

In a later study, Harris et al. (1993), with a different methodology, again fail to rule out the confounding of color descriptors and racial labels. They have two different but roughly comparable samples in terms of phenotype. People in both samples are first asked to self-identify freely (the authors do not report the question used). Then one sample is asked to self-identify again in terms of just the four forced choices of ‘branco’, ‘preto’, ‘amarelo’, and ‘pardo’ (the ‘pardo option’), while the other sample does the same with the forced choices of ‘branco’, ‘preto’, ‘amarelo’, and ‘moreno’ (the ‘moreno option’). The question is: of those who initially freely self-identify as ‘moreno’ how many will reidentify as ‘pardo’ *in the ‘pardo option’*? Harris et al. find that only 37.4% of those who at first freely-identify as ‘moreno’ later reidentify as ‘pardo’ in the ‘pardo option’. They conclude that the racial categories are ambiguous, and criticize an earlier academic effort (Silva, 1988) that led to the Brazilian census categories for having disregarded this by discarding the ambiguous term ‘moreno’ — and adopting ‘pardo’ instead — as the label for the intermediate racial category in the list of forced choices that appear on census forms.

The conclusion is rather startling. In the first place, Silva (1988) argued that ‘moreno’ sometimes acts as a racial label and sometimes as a literal color descriptor (this is a recognition of ambiguity, but of a different kind), and he was trying to avoid confounding the two. Because he thought that ‘pardo’ primed the racial domain better by avoiding this confusion, the recommendation for the census became to use ‘pardo’ instead of ‘moreno’ for the intermediate racial category. One may argue that Silva did not rigorously establish that ‘pardo’ was the best choice, but there is no question that he was concerned with distinguishing racial labels from color descriptors. Harris again seems to have no concern for this, and it leads him to his perplexing practice of treating any color terms applied

to people as ‘racial’ labels without any need for further evidence. He thus seems unaware of the fact that his own data *support* Silva’s contention that ‘moreno’ is sometimes a racial label and sometimes a color descriptor (Telles 1995 makes the same point), and that ‘pardo’ narrowly primes the racial domain. It is precisely under such circumstances that we should expect a substantial proportion of freely self-identifying ‘morenos’ to later reidentify as ‘branco’ or ‘preto’ in the pardo option, as Harris found.

And what about the moreno option? It is plausible that asking people to freely self-identify will cause many to answer in terms of color rather than race (especially if there is not much rigor in how the question is posed), but that being presented with Harris et al’s four forced choices (even when one of them is ‘moreno’) will better prime the basic-level domain of racial categories. This hypothesis would be supported if there was a noticeable decrease in the number of people who reidentify as ‘moreno’ *even in the moreno option*. However, Harris et al. do not even report this crucial evidence.

Some suggestions for fixing the problems with the above methodologies are as follows. First, if the investigation is about race, one must be careful to elicit data in such a way that other domains are not primed. Sticking to the word ‘race’ is a good idea, but even better than that would be a pretest of how different words may lead to different kinds of elicitations. The use of color photographs, rather than black-and-white drawings, should be considered; but if drawings are used, the more information in them the better — realistic color is best. The drawings should be used merely as an instrument for respondents to express, if they can, a set of mutually exclusive categories that fully specify the domain. Proceeding this way is likely to establish the set of mutually exclusive categories at the ‘basic level’, for if respondents are asked to sort all photographs into categories they will almost certainly privilege the basic level (for discussions of basic-level categories see Lakoff 1987; Smith & Medin 1981; Rosch 1978). This however, should not be taken for granted; one should proceed with further investigations. For example, if more than one clump received the same or similar names, or if a set of photographs is called by one name by some respondents, and a different name by others, respondents should be challenged to explain. Following that, one can ask if it is possible to name this or that category with other labels in some

contexts, and what would these be. Careful attention should be paid to possible inclusion relationships to see which of the resulting categories parse the domain at the same taxonomic rank-level. Then one can ask about membership conditions to these categories — a very important question that helps distinguish appearance-based from ‘essentialized’ categories, and determines whether or not the categories obtained are mutually exclusive. Finally, one can compare the results of many individuals to see how much intersubjective variability there is in basic-level categories found and membership conditions given. Whether or not the categories show intersubjective agreement in the phenomenological boundaries is much less interesting than whether the cognitive models of respondents agree, and whether they would resolve borderline cases in the same way should they be given the same background information about the individuals that constitute the stimuli.

It is true that one does not find in the US as varied and prolific a vocabulary for describing people’s phenotypes as one does in Brazil. It is also true that the US system features hypodescent, which prevents the emergence of intermediate racial categories. These are quite hoary observations of the differences between the US and Brazil. However, it is possible that this is the extent of the difference. Harris simply assumed that all of the terms he got referred to ‘racial’ categories and did not bother to examine people’s cognitive models, limiting his study to an elicitation of labels. (This posture explains Harris’s remarkable claim that in Brazil full siblings may be classed in different ‘racial’ categories, an interpretation that stretches thin not only the data he has, but also the concept of ‘race’ as usually understood.) It seems quite possible that the differences in terms of cognitive models of race between Brazil and the US may be smaller than Harris thinks.

IV. Emic and etic: The theoretical pitfalls of having bad terms of art

The problems alluded to here appear to result at least in part from Harris’s reinterpretation of Kenneth Pike’s original distinction between *emic* and *etic*. Harris keeps the terms but his definitions in fact are quite orthogonal to Pike’s approach, have a different theoretical purpose, and importantly influence the methodological road taken. Revisiting these concepts is

helpful both in terms of understanding Harris's approach and its pitfalls, and to clarifying the useful work that cognitive anthropology can do to understand alien category systems. The words *emic* and *etic*, unfortunately, have come to mean almost anything to almost anyone (a sad commentary on the triumph of rhetoric over science in much anthropology). Headland (1990:20-22) reviews an astounding plethora of uses with almost nothing in common. In this discussion, however, I will focus on the *original* meaning, and on Harris's early departure from it.

Before I do, an important caveat. In anthropology it is customary to spill much ink trying to wrest control of the meaning of terms of art from other anthropologists, as if defining a word were equivalent to providing an explanation or making an intellectual contribution. Many proceed as if the meaning of a word — itself apparently of profound importance — was something to be discovered through careful application of quasi-philosophical reflection, and the anthropologist was the professional whose task was to discover it (Headland et al. 1990 is an egregious example of this sort of thing). But this exercise does not advance science and it is not my purpose here. I am not advocating that the terms 'emic' and 'etic' be used in any particular way, and I don't think the words themselves have any mysterious power. I merely want to show (1) that Pike and Harris are talking about completely different things despite the fact that they use the same terms, and (2) that Harris, though he defines the terms differently, nevertheless expects 'emic' and 'etic' to do the same work for him that they do for Pike. This causes serious confusion about what needs to be done in order to explicate an alien category system, and is directly responsible for the methodological problems outlined above. In a nutshell, I will argue that Harris uses emicletic to distinguish between a "folk" and an "analytic" category, whereas Pike uses emicletic exclusively to analyse the boundaries of folk categories.

Kenneth Pike's original innovation was the result of applying his understanding of one system of categories, phonemes, to other systems of categories to see if this could generate any insights. A phoneme is not a sound but a collection of sounds. The 'p' in 'pit' and the 'p' in 'spit' are not the same sound, and they are produced by a different combination of vocal articulations. However, to an English speaker they both *count* as the same phoneme /p/. We therefore have that there is a collection of

different sounds (*phonetics*) that a speaker will parse into a smaller number of functional/grammatical units (*phonemics*). The suffixes were used by Pike as technical terms to distinguish the variation in any domain — *etic* phenomena — that are parsed by people in a given culture into the same categorical slot — or *emic* unit. Thus, Pike was extending an insight of linguistics — the explication of the grammatical properties of the smallest units of speech, which resulted from understanding how a collection of different sounds constituted the same category of grammar — to other cultural ‘grammars’.

In all cultural domains, there is a lumping of objects, performances, or events which are phenomenally and measurably different into the same category. A useful illustration offered by Pike (1990:28-29) is a ‘strike’ in baseball, as there are many different kinds of performances that all count as ‘a strike’.

Individual emic units may have variants without losing their status as the ‘same’ emic unit for a particular person or subculture. — Pike (1990:28-29)

This insight may not appear as a very deep one until one realizes that there may be nothing obvious at all about the actual parsing processes and the categories themselves. For example, to most native English speakers, the contention that the ‘p’ in ‘pit’ and the ‘p’ in ‘spit’ are two different sounds will come as a surprise. Thus, for Pike, the ‘insider/outsider’ distinction is directly tied to the fact that category systems may be opaque to those who use them daily.

Insiders act but mostly don’t understand what they know. I use and categorize sounds spoken by English speakers correctly into the phonemes of English. This is because I am a competent English speaker, an *insider* of the English language. But I don’t have any insight into this tacit knowledge until I analyze my behavior in the manner of an outsider, and treat what I implicitly know as something requiring explanation. — Pike (1990:33-34)

These observations were important to anthropologists because the ethnographer, typically coming from a different culture than the one she studies, is an outsider, and therefore in a good position to find alien category systems as strange and demanding explanation, and also to see at work the categorization processes that go unnoticed by natives. However,

there is a danger that the anthropologist will miss native categories if she superimposes her own. Thus, a determination of the native parsing of the world is indispensable to a proper understanding of an alien culture.

Again, think of phonemes. For English speakers, the sounds which are orthographically glossed as ‘r’, and those which are glossed as ‘l’, belong to two different phonemes. However, to a native Japanese speaker these sets are both included in the *same* phoneme (it is exactly like the ‘p’ in ‘pit’ and the ‘p’ in ‘spit’ for a native English speaker). This explains why Japanese learners of English so often get their ‘r’s and ‘l’s confused, and why this is so saliently strange and amusing to native English speakers but so hard to notice for the Japanese. Similarly, one should expect that people from different cultures will often parse the world of objects, events, and performances in different ways that will surprise and confound an ethnographer. It is also possible that some native categories that *should* surprise the ethnographer never will because they go unnoticed — unless the ethnographer has a method for discovering how natives parse the world. Therefore, it is important, in the effort to understand an alien culture, to establish what people will call ‘the same’. But it is equally important, because much of this may not be conscious, not to trust the natives’ own *reports* of what is the same because, as insiders, natives may not always be able to explain their own categories. Cognitive tasks that make people reveal the underlying and perhaps cryptic categories *by using them* are a much better methodological route than asking informants to introspect on the meanings of things.

To sum up, Pike points out the importance of finding out what objects, performances, or events are lumped as ‘the same’ into one category. To speak of this problem he refers to the measurable variation as *etic* phenomena, and to the categories in which this variation is lumped by natives as *emic* units.

I now turn to Harris’s use of ‘emic’ and ‘etic’, which clearly departs from Pike. Below is Harris’s view, consistent over the years (Harris, 1990:53-54; my emphasis):

Grammatical rules ... have the same epistemological status as phonemes, since the test of their validity, no matter how abstract the formulation, is whether they generate utterances that native speakers recognize as meaningful and appropriate. Such tests, however, *are completely irrelevant* to etic analyses,

which stand or fall on their contribution to predictive or retrodictive nomothetic theories about the evolution of sociocultural differences and similarities.

Do not be fooled by the reference to phonemes — we are no longer talking about the same distinction that Pike has drawn. Harris distinguishes between what makes sense to the native, and what makes sense to the scientist; a distinction between two different languages, rather than two aspects of the same language (which is Pike's distinction). 'Emic' and 'etic', for Harris, stand for two entirely separate *ways* of looking at the world. Emic is the native's parsing of the world, and its measurement; etic is the scientist's parsing of the world (dictated by her hypothesis), and its measurement. This is evident in the fact that Harris considers emic analyses "completely irrelevant" to etic analyses.

Could Pike ever say that emic and etic phenomena are irrelevant to each other? No. In Pike's definitions 'emic' refers to a native category the boundaries of which are determined by looking at the 'etic' variation which it encloses. Remember, he starts with an analysis of phonemes: the phonetic variation is the collection of different variants (different along some domain that we can measure) that native categorizers lump together in the same concept (a given phoneme). Thus, 'etic', for Pike, does not refer to an alternative categorization with entirely different purposes that a scientist may have, but rather to the 'stuff' that the native emic unit is made of. *Pike's project is to find out what natives will consider 'the same'*. The measurement of the (Pikean) 'etic' variation is therefore indispensable — rather than completely irrelevant — to finding out which and how many (Pikean) 'emic' units natives will parse this variation into.

It is apparent that what Harris has really seized on is Pike's reference to an outsider/insider distinction. But notice the difference. By the outsider/insider distinction Pike merely means to observe that the outsider (the scientist) can measure the etic variation, but the native cannot perform this analysis because she is trapped in the perception of the world that her categories impose. Harris, on the other hand, has taken the words 'etic' and 'emic' to *stand for* the categorical worldviews of the outsider and the insider, respectively. For Harris the scientist is the *outsider*, and she has a hypothesis that dictates one choice of categories, and a system of measurement that determines one set of claims; the native is the *insider*, with a separate —

perhaps quite orthogonal — system of categories, and an — also perhaps quite orthogonal — value system and set of priorities that may lead to very different claims. This interpretation becomes even more evident as Harris (1990:53-54; my emphases and brackets) continues:

This does not mean that etic analysis necessarily results in descriptions that are at variance with participants' sense of appropriateness and historical truth. In many domains, but especially technological processes, emic versions of cultural practices and behavior stream events [the native's claims] correspond closely to etic versions of the same [the scientist's measurements]. As shown by Allen Johnson in a study of Brazilian peasant farmers (1974), elicited rules for planting crops on particular kinds of lands and elicited descriptions of past planting activity sometimes corresponds closely with etically observed behavior. But as Johnson emphasized, the facts of correspondence or noncorrespondence raise equally important questions: Why are some rules followed while others are broken? Why do some individuals follow rules while others break them? Why are some rules and concepts widely shared while others vary from individual to individual? (Johnson, 1974:100).

Again, notice the contrast. 'Emic', in the above quote, refers to how the native sees the world, and the language she employs. 'Etic' refers to how the *scientist* sees the world, and the language *she* employs. For Harris 'emic' = what people say they believe and do, and 'etic' = what people *actually* do as measured by an outside observer. So, for Harris, if a farmer says he believes rule X about planting crops, that is 'emic'; the farmer's actual behavior, when, for example, he is observed by a scientist to violate that rule, is 'etic'. But for the question of what rule X itself *is* (and this is Pike's question, and the *raison d'être* for his emic/etic distinction), it matters not one whit whether people in fact go about breaking rule X or not because, for Pike, 'etic' refers not to what people do but to how people *count* what they do. An etic instance of rule X is anything that counts as satisfying it *according to the natives*. A native who steals may obviously count what she did as "stealing" and therefore as breaking the rule "thou shalt not steal". Her behavior is irrelevant to Pike's analysis; what matters is how she counts it.

The point is not that recording what people do, or whether they follow their own expressed rules, is unimportant or uninteresting. The point is simply that this is not what Pike's emic/etic distinction is about. We may be interested in the degree to which people follow their rules, or to which

they behave consistent with their salient concepts (Harris's question), but this is not the same thing as having established what they understand their rules to be, or what the boundaries of their concepts are (Pike's question).² I hope to have shown that we are faced here with two very different research problems, both of which quite unfortunately use the same two terms of art.

Now, if one wishes to conduct a study of how native Brazilians parse the domain of 'race', one could understandably begin by thinking that the Pikean emic/etic distinction will be useful for framing the work ahead. The project will be to look at the Pikean etic variation, and find out what tokens within that variation natives will call 'the same', in order to determine what the Pikean emic units are. However, if one frames the problem merely by using *the words* 'emic' and 'etic', while foisting a different distinction on them, the problem will not be framed in the same way at all. If the distinction used is Harris's, one will say that *etic* is the scientist's system of categories, having no necessary relation to the native's emic system of categories. This distinction may be handy for some purposes, but it does not usefully frame the methodological task ahead for someone who is about to engage in finding the boundaries of a native system of categories (which *was*, in fact, Harris's project). On the contrary, if 'emic' is to 'etic' as the native's claims are to the scientist's measurements, one may conclude that, since we are after the emic (to the *exclusion* of the 'etic!'), the proper methodological route is to *privilege* the first answer the native gives without

²Nevertheless, Johnson's last quoted question *is* important to Pike's distinction. Suppose that the same labeled rule X, encompasses, for individual A, a different universe of actual performances than for individual B. This will sometimes be true. After all, we know that in the domain of speech no two people speak the same idiolect. A speech community is just a group of speakers where the overlap in idiolects is great, and where the overlap between their idiolects and those of people in other communities is small by comparison. In other words, a speech community is a cluster of idiolectic variation. The same is true for the grammar of any other cultural domain. What the ethnographer has to do is, for the domain in question, ascertain the emic system or grammar of each individual in the sample, then measure the degree of overlap between individuals to see if there is reasonable intersubjective agreement. When there is, we may claim to have found the emics of a domain for a particular culture, just as we claim to explain the grammar of a particular dialect. The degree to which a rule is a local 'cultural' rule, in this sense, is the quantitative measure of intersubjective overlap in individual emic systems. All of this, of course, using emic/etic according to Pike.

challenge or further investigation. Who is the scientist, after all, to impose his own 'etic' understanding on the native, if the native does not share it? This stance seems to be directly responsible for Harris's methodological pitfalls.

Here are Harris et al. (1993:453) explaining the application of his emic/etic distinction to the study of race:

In conducting a racial census, researchers can follow an etic or emic strategy. The most rigorous etic approach would employ biologically valid categories based on clusters of alleles as revealed through genetic testing (e.g. blood types, mitochondrial DNA). (...) In a more practical if less rigorous etic approach, racial categories, can be defined by clusters of phenotypical traits (e.g. skin color, hair form, nose width) by trained observers.

In an emic approach, on the other hand, racial identity is established by eliciting respondents' categorizations of themselves or of others using terms that respondents regard as appropriate, whether or not observers find them to be ambiguous or contradictory. — Harris

For Harris et al. an 'etic' approach to 'race' entails a form of analysis where the natives are not consulted, whereas 'emic' is a different form of analysis that requires the researcher to ask the natives about 'race', and privilege their answers.³ The difference between Pike and Harris becomes starkly clear in this passage. Harris calls the studies I have criticized above

³Harris et al. seem to suggest that etic and emic approaches to 'race' are two different ways of studying the same thing. However, when you do what Harris advocates as etic, you are doing one of two different things: either you are studying the clustering of genetic variation, or the clustering of phenotypic variation. When you carry out what he calls an emic analysis, you are studying quite another thing: people's naming of social categories. That these things all get called 'race' is merely a confusion resulting from the use of a colloquial term for three different things that science ought to distinguish. One question has to do with the categories of social relevance and worth that people divide themselves into, and the assumptions and reasoning processes that underlie them, which may or may not be based on phenotypic traits. Another question has to do with the distribution of phenotypic traits, which may or may not objectively cluster. And finally, we have the distribution of genetic variation, which may or may not result in sufficiently distinct and intercorrelated clusters that we would like to call them 'races' the way this word is used for other species in the biological literature (biologists think this use is not justified in humans; see Boyd & Silk 1999, Brown & Armelagos 2001). Simply using the word 'race' for the three questions does not mean they are the same thing. What Harris calls 'emic' and 'etic' approaches to 'race' are not, in fact, two ways of studying the same thing, but in fact different questions entirely that are not even being asked of the same domain.

‘emic’ because he is recording peoples’ answers, rather than taking some other kind of measurement. However, Pike would say that what Harris obtained is a sample of ‘etic’ variation and, since Harris has not tried to establish where the conceptual cleavages and boundaries are in that variation, he has not yet found a single ‘emic’ unit. That is, in Pike’s terms, Harris has elicited many (etic) behavioral responses, but has not found — yet — any (emic) categories. To see this, it suffices to consider merely that Harris has not sought to establish whether any of the myriad terms his respondents gave are synonyms, or whether any exist in taxonomic inclusion relationships to each other, only one rank-level of which people consider to be ‘race’. Pike’s project, remember, is to find out what people will call ‘the same’, and thus leaving the possibility of synonyms and inclusion relationships unexplored reveals that Harris’s methods do not even attempt Pike’s project, even though, ostensibly, Harris claims to be after the native system of categories.

The above interpretation is bolstered by the fact that on at least two recent occasions (Harris et al. 1993; Byrne & Harris et al. 1995) Harris defines emic/etic for his readers with the historical link to *phonetics*. He does not cite Pike despite the fact that it is Pike who made the derivation from phonetics, and despite having had a public debate with him about the use of the terms (Headland et al. 1990). Moreover, Harris uses the definition he has used over the years rather than keep it in line with the linguistic analogy. If Harris is talking about something else, why keep the same terms? And why keep an alleged link to phonetics? Probably because the historical link to phonetics creates the impression that the emic/etic distinction is useful for doing work that explicates alien category systems (since that was Pike’s explicit program), and Harris is ostensibly trying to understand one such system.

Perhaps this is why we fight over words in anthropology: we hope that words alone will do theory for us. But they can’t and don’t. One cannot base a study of an alien categorical system on Pike’s terms (much less only one of them). One has to base it on his linked *definitions*. The lexemes ‘etic’ and ‘emic’ are themselves immaterial and have no special power. It is what they stand for, in Pike’s exposition, that is useful. Harris’s methodology elicited labels on the basis of appearance, but this is not enough for establishing the nature or the boundaries of the categories

unless it seeks to establish whether and which of those elicited terms count as ‘the same’ in people’s minds.

Anthropologists should be wary of applying their own (*Pikean*) emic system — that is, their own culturally-based native system of categories — to an alien culture, but they should be equally wary of making the facile assumption that the native system must be different from their own. Harris may have been too ready to believe that Brazilian racial categories are radically different from American ones, and his studies make little effort to prove him wrong.

V. Conclusion

I found in Bulgan Sum that people believe ethnic membership to be, first, strongly a question of biological origin; second, predictive of behavior despite the fact that a member may have no exposure to his ethnic group and may not even know himself to be a member. This suggests that ethnic categories may be essentialized in the manner of species categories and, like them, not defined by appearances. These latter serve merely to assist first-guess identification, and therefore ethnic categories cannot be studied with methods that rely solely or even primarily on identification procedures without delving deeper into the cognitive models of respondents. Due to the close empirical and theoretical overlap between racial and ethnic categories, and to the plausibility that from the cognitive point of view they are indistinguishable, the same admonition applies to racial categories.

I have critiqued here what I see as problems with previous research that attempted to investigate cognitive models of race in Brazil by eliciting behavioral responses to ‘appearance’ stimuli, and have suggested alternative methodology that is more likely to succeed in the effort to map the conceptual boundaries in naturalized or essentialized domains. I have also sought to trace the historical source of the methodological road taken by Harris and colleagues, which in its essence springs from a theoretical misconception resulting from using the terms emic/etic with different meanings than those originally proposed by Kenneth Pike, while at the same time expecting them to do the same work that, with the original meanings, they did for Pike in terms of addressing the work that needs to be done to understand an alien category system.

The most important lesson here is that the cognitive anthropologist needs methods that go beyond mere elicitation. Especially in domains such as natural categories where appearances underlie first-guess identification rather than conceptual parsing, elicitations of labels based on appearances (no matter how rigorous and careful the preparation of elicitation materials) will fall short in our attempts to map native categorical systems.

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