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The Cognition of Ethnicity

native category systems under the field-experimental microscope

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Abstract: Reports a series of studies conducted in the field with methodology approximating lab experimental methods to the degree possible. The studies were designed to investigate how human actors process and think about ethnic categories. The motivating hypothesis is that humans essentialize ethnic groups because these resemble ‘species’ in several of their salient properties, most importantly a tendency towards category-based endogamy, and descent-based membership. In order, the studies reported here tested (1) whether ethnic membership is a matter of enculturation or descent; (2) whether ethnic categories are essentialized, with associated behavioral expectations; (3) whether physical differences between the two local groups drive the essentialism, or whether essentializing the categories in the first place drives this perception of physical differences. It was found that membership is held to be a matter of blood, that a majority of respondents essentialize the ethnic groups, and that the distribution of phenotypes may not justify the extent to which respondents intuit that the contrast ethnic groups differ physically (which suggests that categorization drives perception).

I. Introduction

A. Experimental psychological anthropology

This paper is principally concerned with what kind of category is an ‘ethnie’ in people’s ordinary intuitions, and those methodological issues which arise when one attempts to approximate lab-experimental methods in the field for the purposes of investigating native systems of categories. When people think about their own ethnie, or about outgroup ethnic categories, what cognitive resources (i.e. assumptions and biases) are mobilized? Specifically, the studies contained herein investigate and test the possibility that ethnic categories are essentialized in ways similar to ‘species’ categories.

The methodology used here attempts to approximate the lab experimental methods of psychologists because there are great benefits to be had in making

anthropology more psychologically sophisticated and/or psychology more field-oriented. Although Humphrey (1996) has observed that anthropologists ignore psychological research at their peril, she remarks in the same breath that psychology cannot simply substitute for deep ethnography because without the latter we cannot contextualize what we learn from lab experiments with university undergraduates. But anthropologists can do better than simply inform their field research with what they learn from the psychological literature. For example, they can address psychological questions using a wide variety of methods as many in the cognitive anthropological tradition have done (D'Andrade 1995), or they can take to the lab themselves in order to explore questions that derive directly from anthropological concerns, as Hirschfeld (1996) has done. Finally, and perhaps most interestingly, they can take the lab to the field, to do what psychologists do with populations that practically never get to be psychological subjects (e.g. Atran, Estin, and Medin 1997). This is a new and promising avenue that has, for the most part, not been pursued in the venerable tradition of psychological anthropology that goes back to Bronislaw Malinowski and Gregory Bateson, where psychological methods have been mostly shunned (e.g. Briggs 1970; Epstein; 1992; Hallowell 1955; Heider 1991; Levy 1973; 1984; Lutz 1988; Piers & Singer 1953).

The use of field experimental methods by cognitive anthropologists can be a natural complement to lab experimental psychology. Field experiments yield cross-cultural comparisons, but they are based on data whose clarity is inferior to that which can be obtained in a lab. Lab experimental psychology produces lots of hard data but forages for the most part in a very narrow and restricted population: university undergraduates in the industrialized world (cf. Medin & Atran 1999:2-8), which population is of suspect ecological validity for pan-human questions. Psychological methods in anthropology, however, should not drive out the discipline's greatest methodological contribution—the ethnographic method—because “relying on tightly controlled paradigms alone may actually prevent informants from displaying forms of knowledge that would be most incisive for the questions under consideration” (Coley *et. al.* 1999:206). Rather, anthropologists should consider a deep intermixing of field experiments with long stays and qualitative, rich, descriptive work, that allows us to inject rigor to specific questions while preserving the contextual background with which to provide an adequate interpretation of what the results *mean*. Certain costs must be paid in this approach: the difficulties of a naturalistic setting will obviously make such field experiments fall short of the strict requirements that would make a lab-based psychologist truly comfortable. On the other hand, the role of the anthropologist as a *natural historian* of a particular community gives her a more secure foundation for interpreting the results—the gain here is in ecological validity.

As an ethnographer who has tried to approach experimental methods in the field, I present here the methodology and data of recent work as both an argument for, and an example of, this kind of research, with consideration of some of the important methodological issues that arise. Space limitations restrict me to the specific studies conducted, but interested readers may find some of the ethnographic data in Gil-White (in

press), and the rest will be published in a forthcoming paper. For those readers of two theoretical papers of mine that rely heavily on these results (Gil-White 1999, and Gil-White, 2001a), this presentation also serves the purpose of clarifying how my experimental data was collected, and specifying its content in detail.

The work presented here falls under the rubric ‘cognitive anthropology’, of which there is more than one tradition. Sanjek (1971) distinguishes between the ‘psychological approach’ of scholars such as Romney & D’Andrade (1964) and the ethnographic approach of Frake (1964). The first presents informants with cognitive tasks that they must solve for the researcher. The second records ethnographic instances of the occurrences of certain behaviors and their immediate antecedents and consequents (a very good recent example is Fessler’s 1999 study of ‘shame’ in Indonesia). On the above methodological distinction, the present set of studies fall in the ‘psychological approach’ column.

But cognitive anthropology can also be cut into various strands on the basis of theoretical thrust. From this perspective, the work reported here is a descendant of ethnobiological studies that sought to describe and compare the folk-classifications of the natural world in various populations (Berlin 1992; Brown 1984; Hays 1983; Hunn 1977; Berlin, Breedlove, & Raven 1974). This tradition has recently intersected with evolutionary theory and developmental cognitive psychology (Atran 1997, 1990; Medin & Atran 1999) and provided an account for the universality of many features of folk-biological taxonomies on the basis of pan-human cognitive processes selected for in the ancestral environment. This work is the most immediate ancestor for the studies below, which sought to investigate possible similarities in how humans reason about ‘species’ and ‘ethnic’ categories.

Finally, in terms of object of study—ethnicity and how it is organized mentally—there are similarities between this effort and those of Harris (1970, 1993), Byrne *et al* (1995), and Sanjek (1971), including the fact that such studies applied experimental methods in the field. However, those studies inferred cognitive models from performance in tasks that tested identification procedures, and it is now known (Armstrong, Gleitman, & Gleitman 1983) that this can yield very different results from procedures that test the cognitive models directly, as I did. Given this, it is quite possible that the conclusions concerning the supposed fuzziness and instability of Brazilian racial categories may have to be revised (Gil-White, 2001b), and it is not yet clear that those data reflect cognitive models different from the ones presented below.

B. Theoretical background for this study

In the anthropological, sociological, and political science literatures there has been for considerable time now much debate over the nature of ethnic groups (for a review, see Gil-White 1999). Are ethnic groups primarily *instrumental* associations of rational political actors, or do such human agglomerations result from *primordial*

sentiments which are relatively independent of ‘rational’ considerations as economists and other methodological individualists typically conceive of ‘rational’? This overarching issue leads to the research question: how do ethnic actors on the ground conceive of recruitment into the group? What is the membership charter? That is, what are the rules, if any, for joining an ethnic group as a true member? Specifically, I sought to determine whether ethnic actors themselves saw recruitment as an issue of (rational) ‘choice’, or as one of (primordial) ‘destiny’.

I found that respondents in my fieldsite tend to see recruitment as a matter of biological descent rather than enculturation—let alone choice. This result in turn prompted a new question: if ethnic status is a matter of descent, is this merely a *marker* of descent, or does it carry intuitions about the ‘nature’ of a person, with corollary intuitions about the implications for that person’s behavior? Is ethnic membership taken to result from possession of a given ‘essence’ particular to that group (e.g. in the way that species categories are taken to possess causal essences)? If so, an ethnic status is a matter of ‘blood’ and not merely of ‘descent’. Another study was carried out in order to explore these questions. The results suggest that actors are indeed essentialist with respect to ethnic groups. Finally, another study included here tried to examine the degree of phenotypic overlap in the two contrast groups found in my fieldsite, in an attempt to see if the essentialism found was plausibly caused by the perception of phenotypic differences. This study found considerable overlap between the groups but cannot give a satisfyingly definitive answer to the question of whether the perception of phenotypic differences is a cause or an effect of the essentialism.

The implications of this collection of results for the circumstantialist/primordialist debate are discussed in Gil-White (1999). Their implications for an understanding of (1) the brain’s processing of social categories, and (2) the evolutionary emergence of ethnic groups (including the functions they arose to serve), are discussed in Gil-White (2001a).

II. Setting

My fieldsite is populated by Torguud and Kazakh semi-nomadic pastoralists (Torguuds are a small Mongol ethnic group in Western Mongolia) in the district of Bulgan Sum, Hovd province, Republic of Mongolia. The Torguud Bangyakhlan clan (which hosted me) winters in a large floodplain valley of the Bulgan river, not far away from the district center, but in the summer clan members climb up to the high ground in the Altai Mountains. Torguuds are the largest ethnic group in the area and Kazakhs constitute about 30% of the district population. 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.

The site was chosen because it offers desirable controls for a number of variables. Since the study is testing for essentialism, one should worry about plausible confounds that could lead people to give essentialist answers (race, class, ecology/occupation, religion), which confounds would cast doubt on the interpretation that respondents are reasoning about ethnic categories per se. However, none of these plausible factors are present at this site. In terms of phenotype, the two groups have overlapping distributions without a very sharp phenotypic cline. Thus, it is unlikely that that local actors essentialize the ethnic groups because they are faced with sharp phenotypic or ‘racial’ differences. Class is also not an issue because the economic distributions of both groups are very similar, and as a whole both groups are quite poor. There is no socioeconomic structural ranking of the groups. Ecological differences are marginal: both groups include nomads and town-dwellers, and the pastoral nomadism of both groups is very similar, right down to the species they herd (there are small differences in migration patterns and species distribution). Finally, the two local religions (Tibetan Buddhism among Mongols, and Islam among Kazakh) are both proselytizing rather than essentializing religions (thus, unlike say, Judaism or Hinduism). Any essentialism found in this site is more likely related to the nature of ethnic-category cognition than to local correlates of ethnicity.

III. Study (1)

This study sought to establish the criteria that people use to determine whether someone is or not a member of a particular ethnic group.

A. Methods

I administered a 3-question instrument with a within-subjects design in the summer of 1997 to 59 Torguud participants (remember, Torguuds are Mongols). Of these, 28 are male and 31 female, and the ages ranged from 10 to 72. Each of the following four cohorts represents almost exactly 25% of the sample: 10-19; 20-30; 31-39; 40-72. The sample was not random and had a significant neighborhood bias due to a difficult terrain, long-distances (nomads have very low-density dispersal), and slow transportation (horseback), which made proximity to the researcher and likelihood of inclusion in the sample very highly correlated. This problem is greatly attenuated in the second study, but I see no reason for concern in this study that spatial proximity to myself and the variable of interest (notions of ethnic recruitment) should be related in any way. All manner of micro-contextual variables—the across-subject uniformity of which is easily obtained in the lab—could simply not be controlled for here, and therefore vary from subject to subject (temperature, time of day, place of testing, etc.). Because of the effort involved, and because I did not think they would affect the results in the least, these

variables were not coded. One presumably important variable, however, *was* coded: whether subjects were interviewed alone or in a group.

The set of three questions was designed to investigate whether Torguuds conceive of ethnic membership primarily in terms of blood or of enculturation. The questions were asked in the order shown below (note that the emphases were actually used sociolinguistically in the verbal rendering).

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?

A few test runs with question 1 established that, for Torguuds, ethnicity is strongly patrilineal, which is surprising neither for Mongols nor pastoralists, who are usually patrilineal (Khazanov 1994:143). However, this usually refers to clan and sub-clan ascription, and material inheritance; but here we see that fathers also transmit ethnic ascription. Thus, in questions 2 and 3 I made the father Kazakh but the environment of enculturation Mongol in order to pit enculturation against descent. I should note that these questions do not test directly whether my respondents believe 'choice' can be exercised for ethnic membership. I reasoned that if a much weaker criterion—enculturation from birth—was largely irrelevant for ethnic membership, then the issue of choice was moot. Question 3 is somewhat similar to the 'switched-at-birth' paradigm used in some psychological studies (Solomon et. al. 1996, Springer 1996, Hirschfeld 1996:107-115). However, those deal with either occupational or phenotypically contrasting categories.

The Mongolian word *ündesten*, translated as 'ethnie' or 'ethnicity', is one that locals regularly apply for such groups as Mongols and Kazakhs. It is a good match to our own usage even down to the polysemic ambiguities. The word may sometimes be used to denote groupings that in the West are called 'races' but which are also uncertainly distinguished from ethnic groups, and a great many of my informants applied it to groups such as Torguud or Uryanxai which we would call either Mongol sub-ethnies or 'tribes'. A scattered few objected that *ündesten* is not proper to denote *Torguuds*, who are a *yactan* (translated as 'tribe') but the applicability of *ündesten* to the Mongol/Kazakh contrast generated no such controversy.

The representation of these questions was assisted by simple genealogy diagrams (see appendix A), hand-drawn in real time for every subject, and whose logic was explained prior to asking the questions. The procedure was the same for all interviewees (excepting idiosyncratic clarifications). I should note that the operation of these diagrams was very easily understood by my respondents, and this is not surprising given that Mongolia has almost 100% literacy, even among its rural nomads.

A post-interview cross-examination always demanded to know why subjects had answered in the manner they did. This was to see if they had a rule, or could produce one, and also—when necessary—to point out what appeared to be an inconsistent rule at work (a practice which, on the assumption that people’s cognitive models are consistent, reveals to both researcher and subject that one or more of the questions have been misunderstood). Whenever (1) people changed their sequence of answers under cross-examination (not very common but it did happen), (2) both the first sequence and the second revealed internally consistent models, and (3) I was not highly confident that I could discern which one they really believed, I erred against my preferred hypothesis by recording the less primordialist of the two sequences.

The questions 1-3 are increasingly biased against the subject persisting with a descent-based answer by increasing the importance of enculturation and decreasing the importance of the biological parents. I was trying thereby to make obvious to respondents both the contrast that the instrument was forcing, and the fact that this contrast was particularly extreme in the last question. To assist this perception, the “but” in question 2 was highly emphasized sociolinguistically by raising my voice along with my index finger, while making big eyes that looked straight into the interviewee’s in what I hoped was an ominous expression. I was hoping not only to draw close attention to a set of circumstances absent in the question 1 that might now justify a different answer, but also to imply that this was, from my perspective, the expected answer (for I wished to bias the experiment against my favored hypothesis)

B. Results

For economy of presentation, whenever two samples of binomial data are compared, I present not the 2×2 tables but rather the proportion of ‘successes’, and the sample size, for each sample. All such comparisons are Chi-square analyses and they were done with exact, rather than asymptotic, statistics due to the small sample sizes. All p-values are two-sided.

As the results summarized in **Table 1** below show, the great majority of respondents were unfazed by my overt implications and insisted that the child in question

2 was Kazakh. Even in question 3, a majority believes the child is Kazakh. Thus, it is apparent from the responses that most subjects considered descent, and not enculturation from birth, as decisive for ethnic status. There were no effects of sex¹ or age.²

Table 1: Appearance vs. descent. The cells give the counts that correspond to a given answer (row), to a given question (column). The per-question proportion of each answer is given in parenthesis.

Answer	Question 1	Question 2	Question 3
Descent (child is Kazakh)	59 (100%)	49 (83%)	35 (59%)
Rearing (child is Mongol)	0	10 (17%)	24 (41%)

N=59

Note: In question 1 three people answered *erliiz* or ‘half-breed’, but they explained that this was a *Kazakh erliiz* and had one, not two ethnicities. The word *erliiz*, then, though translated as ‘half-breed’ stands for mixed descent, not mixed ethnic status.

The question, as listed above, was always ‘what is the ethnicity of this child?’ It was thus ‘open’ and they could answer anything at all, and were not choosing from among options supplied by me. However, the only answers ever given were those shown above. Question 1 establishes notions of linearity, and questions 2 and 3 pit descent against enculturation.

Since the long distances and slow transportation make it laborious to obtain 59 respondents, many of the people in my sample come from just a few extended families. This might introduce a possible confound into the results if close kin tend to have the same ideas in this domain, and if primordialist families are overrepresented. In general, it did not seem to me that kinship was a good predictor at all, as individual families appeared to have the same proportional heterogeneity as the larger sample. But I reanalyzed the data for *ezen* (family heads) only, as there aren’t too many close kin among them. The table below shows the means for *ezen* only. It is apparent that the trends are all the same, and the percentages quite similar. I am thus fairly confident that—despite its shortcomings—the sample is not fatally flawed.

¹ Question 2 means: males = .82 (n=28), females = .84 (n=31); Pearson chi-sq. = .03, $p = 1$

Question 3 means: males = .57 (n=28), females = .61 (n=31); Pearson chi-sq. = .105, $p = .795$

² Question 2: Spearman’s rho = .3, $p = .22$; Question 3: Spearman’s rho = -.036, $p = .840$

Table 2: *Ezen* (family heads)

Answer	Question 1	Question 2	Question 3
Descent (child is Kazakh)	15 (100%)	13 (87%)	8 (53%)
Rearing (child is Mongol)	0	2 (13%)	7 (47%)

N=15

The following simple taxonomy is useful: the sequence ‘Kazakh, Kazakh, Kazakh’ corresponds to a ‘hard’ primordialist ETAM (Ethnic Transmission and Acquisition Model). These respondents assign a Kazakh ethnic status to the biological child of a Kazakh no matter what the circumstances. The sequence ‘Kazakh, Kazakh, Mongol’ corresponds to a ‘soft’ primordialist ETAM. For these respondents ties of blood are paramount but truly extreme circumstances allow them to bend the primordial criterion. The sequence ‘Kazakh, Mongol, Mongol’ is that of a ‘soft’ circumstantialist—i.e. who believes circumstances of childhood enculturation will determine ethnic status. The breakdown of ETAMs is as follows:

Table 3. Distribution of ETAMs (Ethnic Transmission and Acquisition Models)

Model	Proportion	Proportion among primordialists
Hard primordialist	0.59	0.71
Soft Primordialist	0.25	0.29
Soft circumstantialist	0.17	
Total*	1.01	

*Total has rounding error

A ‘hard circumstantialist’ contemplates ethnic switching on the basis of adult rational choices, and any such individuals are here collapsed into the ‘soft circumstantialist’ category. I do not know the exact proportion of the ‘hard’ model, but even if all of the ‘softs’ are ‘hard’, this still leaves us with less than a fifth of my respondents espousing a non-primordialist position. Add the reasonable expectation that some will not be hard circumstantialists and this evidence looks bad for the rational-choice model. An ethnic claim must be accepted by others to have social efficacy, and so given what these data reveal about most people’s beliefs concerning the possibility of choice, any adult’s crossover claims would fall mostly on deaf ears. Cross-examination revealed that some hard circumstantialists certainly do exist—at least two respondents fit this description, but an accurate determination of this ETAM’s relative frequency must await further research.

C. Discussion

1. Methodological issues

I drew a new diagram for each respondent in order to help anchor attention to the items under consideration as they were introduced one by one. A prepared drawing would have probably distracted them as their attention wavered from what was being explained to other items in the drawing. One may object that the cost here is that the diagrams for different respondents will not be identical, even if they are equivalent in number and relative position of items, and in all of their functional and conceptual relationships. Why add to the aggravations of uncontrolled factors in the field?

My view is that the constraints of the field may be a blessing in disguise, so increasing them may add more blessings. Consider that the lab psychologist's ability to intersubjectively control many variables with great precision allows the identification of very tiny effects about which perhaps too much is made. The field researcher is free of such temptations and is thus constrained to look at reasonably large effects, which arguably is where the focus of social-scientific investigations should be. A sloppy experiment that yields a big result should increase our confidence in the causal power of the underlying phenomenon: here is a causal force that will resist effacing by a sloppy experimental procedure. Of course, if the sloppiness is biased to produce it, the result is spurious, but I do not believe this is the case here, and certainly not for the drawings. In fact, the bias is in the opposite direction. My hypothesis was that people would give primordialist answers, which, in this case, amounts to answering that the child is a Kazakh in questions 2 and 3. However, notice that the 'M's (see appendix) which surround the child and which represent the fact that he is living in Mongol territory and surrounded by Mongols, are much larger than the 'K' above the father (this was true for all my hand drawings despite individual variations). So if there is any bias in the drawing, it is a subliminal perceptual cueing that should favor the circumstantialist answer.

This brings up a related point: the order of the questions was deliberately chosen so as to again create a bias in the instrument against the preferred hypothesis. The questions, and my own sociolinguistic emphases, were designed to convey that the new circumstances presented in questions 2 and 3 required a new kind of answer (one based on enculturation), and that I in fact expected this answer.

The larger point made by all of this is that the field researcher should consider biasing his instrument against his preferred hypothesis. Only reasonably large effects will be detectable under such conditions anyway, and it compensates for the sloppiness of the experiment to stack the deck against the preferred hypothesis. This stratagem allows for an increased confidence in the qualitative interpretation of the results even when controlling most variables intersubjectively is difficult or impossible.

2. Possible objections

These results admit of three objections. Descent ascription trumped enculturation because: (1) the father was an outgroup member rather than a coethnic of my respondents; (2) the child is drawn in the genealogy diagram with the same symbol—a triangle—than is the father (see appendix), and it was this perceptual bias that led respondents to assign to the child the same ethnic category as the father's; (3) given that respondents tend to answer in terms of blood to the (contextless) question 1, presenting them with this question *first* in fact biased the results for questions 2 and 3 in favor of my hypothesis. Study 2, below, carried out in the period June-December 1998, attempted to address these issues.

It was prohibitively difficult to test each objection one-by-one with three independent experiments, so I decided to vary the three relevant variables simultaneously. This stratagem gets me a comparison between Study 1 and Study 2 which falls short of being a rigorous experiment in which the samples from each study would function as the two conditions. However, given other data that I gathered, it does not seem likely that doing the tests in an optimally rigorous fashion would have added much.

It is implausible that the father being an outgroup member, or the child being drawn with the same symbol as the father matters to my respondents' answers because in the cross-examinations I was usually given some paraphrased version of the following rule: "The child is Kazakh because one takes the *ündesten* [ethnic] of the biological father; it doesn't matter what his culture or language is." Most had no trouble producing this rule, and there were never any further qualifications such as "well, but this is true only when the father is a Kazakh, not when he is a Mongol," or "except that it is different for male and female children." It thus seems clear to me that my respondents (1) understood the problem as intended, (2) were applying a *general* rule, and (3) were not merely matching triangles with triangles. The bulk of my ethnographic data also supports the view that my respondents apply their models *generally*. In other words, they think this is the way one inherits ethnic statuses, period (it is a question of how the world works), and told that other groups around the world do it differently (e.g. matrilineally) arouses surprise and disbelief.

As for the effect of question order, it admits of a different interpretation, given earlier, that it biases the answers *against* the primordialist hypothesis. In Study 1, the data on the effect of being interviewed alone *vs.* in a group supports this. In **question 2**, the proportions that answered 'Kazakh' (descent-based answer) were: 'in group' = .89 (n=45), 'alone' = .64 (n=14); Pearson chi-sq. = 4.6, $p = .047$. And in **question 3**: 'in group' = .69 (n=45), 'alone' = .29 (n=14); Pearson chi-sq. = 7.19, $p = .01$. This is consistent with the view that being alone with the interviewer, with this design, often intimidates the respondent to give the answer that the researcher clearly appears to expect. In a group, however, an individual knows what others are likely to say, and the expectation of the researcher appears to exert much less influence.

Thus, anticipating that the results of Study 2 largely confirmed those of Study 1, I recognize that this nevertheless falls short of a rigorous dismissal of the three objections raised above. However, the advantages of field experiments lie in that we have other data, whether from ethnographic observations or from post-questionnaire cross-examinations, and it is all relevant. Where we fail to meet the standards of the lab, we should take full advantage of other resources, and our conclusions should rest on an evaluation of the totality of the data available to us.

IV. Study (2)

A. Methods

In this procedure (1) the father is Mongol and the mother Kazakh, and, consequently, the circumstances of enculturation are likewise Kazakh; (2) the child is a daughter rather than a son, drawn with a circle, not a triangle; and (3) the order of the questions is randomized. The questions are identical to those in Study 1 except that one must substitute ‘Mongol’ for ‘Kazakh’ and vice-versa. The changes in the diagrams are depicted in the appendix.

A longer stay in 1998, and a ‘hosting’ tradition common among nomads (many families hosted me for a few days in exchange for photographs), resulted in a wider net and thus in a sample without any kin-bias, and a very attenuated neighborhood bias. As before, the sample was collected as I met people who had not participated in the earlier study, and as circumstances proved opportune for administering the questionnaire. The sample is composed of 41 Torguud individuals, 21 of them male. The ages range from 15 to 70, but are not as evenly distributed as in the Study 1 sample.

B. Results and discussion

The table below follows the same rationale as table 1, but remember that it is now those who answer ‘Mongol’ who are using a criterion of descent.

Table 4: Appearance vs. descent

Answer	Question 1	Question 2	Question 3
Descent (child is Mongol)	37 (90%)	33 (80%)	31 (76%)
Rearing (child is Kazakh or erliiz)	4 (10%)	8 (20%)	10 (24%)

N=41

Note: To provide the strongest case of the hypothesis that people think in terms of blood, I have grouped all answers that were not unequivocal, patrilineal descent-based answers in the ‘rearing’ answer category. This includes a few who answered *erliiz*, despite the fact that I have good reasons to suspect that they believe the child’s ethnicity to be the father’s.

It seems that whether the child in the questions is male or female matters not at all. The increase in primordialism—relative to the previous study—in question 3 [‘study 1’ mean = .59 (n=59), ‘study 2’ mean = .76 (n=41); Pearson chi-sq. = 2.86, $p = .13$] is also consistent with my interpretations, for it suggests that presenting the questions in the order 1-2-3 had indeed depressed primordialist answers in Study 1, question 3. Admittedly, this increase may not be real. The issue can be examined further by comparing, in study 2, question 3, the answers of those who saw question 2 first, as against those who saw question 3 first [‘2-3’ mean = .67 (n=21), ‘3-2’ mean = .85 (n=20); Pearson chi-sq. = 1.87, $p = .28$]. This effect, though even more questionably real, is again consistent with the interpretation just offered.

The sample sizes in the relevant cells are too small to test the interaction of question order \times context (‘context’ refers to whether the respondent was interviewed alone or in a group). Despite this shortcoming, we can compare the effects of context in this study as against those in the previous one. In Study 2, there is no effect of being interviewed alone vs. in a group [**question 2**: ‘alone’ mean = .70 (n=10), ‘in group’ mean = .84 (n=31), Pearson chi-sq = .93, $p = .38$; **question 3**: ‘alone’ mean = .80 (n=10), ‘in group’ mean = .74 (n=31); Pearson chi-sq. = .14, $p = 1$]. This is consistent with the idea that the ‘leading question’ effect—biasing answers towards the circumstantialist view that the experimenter appears to prefer—should occur only when those interviewed alone see the questions in the order 1-2-3. In study two, randomization of question order results in a very small subsample being exposed to this sequence, and therefore to a much reduced difference in the mean answer of those asked alone vs. those asked in a group. The bulk of this evidence thus supports that the order of the questions mattered, and that, in Study 1, it had biased responses *away* from the expectations of the primordialist hypothesis.

The total proportion of hard primordialists, and their representation among primordialists, increased, as shown below.

Table 5: Distribution of ETAMs

Model	Proportion	Proportion among primordialists
Hard primordialist	0.71	0.81
Soft Primordialist	0.17	0.19
Soft circumstantialist	0.12	
Total	1.00	

The proportion of hard primordialists does not correspond exactly to the proportion answering ‘Mongol’ in question 3 because in this study there were a few ETAMs that did not surface in Study 1. For example, one participant responded ‘Mongol, Kazakh, Mongol’ to questions 1, 2, and 3. This is a person with a patrilineal, primordialist bias, but who leans in a circumstantialist direction if the circumstances are extreme and the ethnics of enculturation also has a tie of blood to the child (question 2), but not when there is no such tie (question 3). Thus, this person was coded as a soft rather than hard primordialist.

One thing these studies cannot tell us is whether a tendency to label people on the basis of descent has any significance beyond the labeling process itself. All it shows is that for people to think of you as a Mongol you need Mongol blood—but this could be nothing more than a simple label. The cognitive perspective adds little to our understanding of ethnic group behavior unless we can show that the process of ethnic categorization affects people’s expectations, rather than merely indexes people’s biological ascent. This is what Study 3 undertook to investigate.

V. Study 3

The object here is to see whether people associate ethnic membership with the possession of an ‘essence’ particular to the group. The implications of this study for the literature on essentialism are explored in Gil-White (2001a). Due to space considerations, I avoid here a review of that literature (interested readers should see Medin & Ortony 1989, Medin & Atran 1999; Atran 1997, 1998; Gelman & Medin 1993; Hirschfeld 1996; Gil-White, 2001a).

Briefly put, however, essentialist thinking posits an ineffable and ambiguous something or other—the ‘essence’—which resides ‘inside’ the thing (whether the thing is a natural substance, element, or a living kind). The essence is believed *causally* responsible for the thing being what it is. One of the most interesting consequences of essentialist thinking is that appearance does not drive categorization. Because the essence is causally responsible for the reality of the thing, and because it is carried inside, the exterior appearance is a *typical consequence* of the essence but does not define it. Only the essence defines the thing. This is why it is possible for humans to accept without too much effort that a dolphin is not a fish, or that fool’s gold is not gold, and that there are experts who know why. Essentialism of both living kinds and natural substances would explain our readiness to discount appearance and rearrange our categories on the basis of some not readily apparent underlying ‘nature’. The fact that my respondents in the studies above consider the biological child of ‘A’ parents an ‘A’—despite the fact that such a child would *appear* in terms of language, customs, traditions, dressing, and cultural reflexes as a ‘B’—suggests that they may be essentializing the categories. If the essence

is taken to be transmitted biologically, this could explain why ethnic ascription is a matter of descent: that is, it takes a Mongol essence to be a Mongol, and you can only acquire this essence through biological descent—thus, only those descended from Mongols can be Mongols, and they will be Mongols regardless of how they appear and behave.³

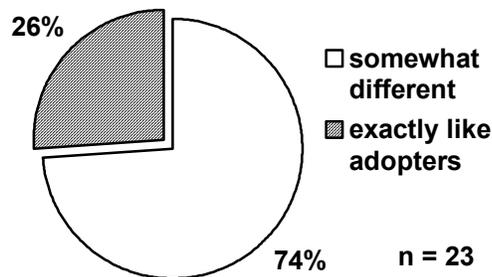
If this interpretation is correct, there should be an *expectation* of behavioral implications contingent on the possession of a particular essence. In other words, if a person is believed to have a Mongol essence, it should follow that the essence makes this person a *Mongol* in a deep, intrinsic way (i.e. not just a matter of labeling), with attendant expectations for this person’s behavior. Thus, although my respondents accept the premise of question 3—namely, that the adopted child acquires the language, traditions, and customs of the group that rears him/her—there should be resistance, if they are essentialists, to the idea that such a child is exactly like the adopters. If they are essentialists, they should deny that the child can become *exactly* like the adopting group, even when the child is unaware of its biological ancestry, and even when there is no cultural contact with the group of his biological progenitors.

A sample of ‘hard’ primordialists from Study 2 were asked an additional question. First, the details of question 3 were reviewed, and then I asked the following: “This adopted child, will he become *exactly* like the Kazakh, or will he be somewhat different?” (Recall that in Study 2 the adopters are Kazakh.) If they answered that the child would be somewhat different (a common locution was “No, he can’t/will not become *exactly* like the Kazakhs”), then I followed up with the question, “How will the child be different?” Almost invariably, the response to this was that the child’s features would be like those of a Mongol, revealing that subjects had understood the question as it was intended, in terms of similarity to Kazakhs *as a group*, and not to the two adoptive parents in particular. Then I followed up with the question: “And how will the child *behave*? Will the child behave *exactly* like the Kazakhs or will he behave somewhat differently?” Below are their responses to this final question.

Figure 1. Proportion of essentialists vs. non-essentialists among primordialists.

³ For a dissenting view from this biological interpretation of essentialism see Gelman & Hirschfeld 1999. For a supporting view, see Atran 1998.

Question: Will the child behave exactly like the Kazakhs [adopting ethnief] or somewhat different?



Some of the hard primordialists were also queried as to how many generations of male descendants marrying Kazakh women and bringing their children up within the Kazakh ethnief would be necessary before they were willing to call the descendant 'a Kazakh'. In order to do this, each new generation was drawn as linked to the preceding one, followed by the question: "This male child also grows here among Kazakhs, just as before, and marries a Kazakh woman. What is his ethnicity." If they answered 'Mongol', the male child was joined by a drawing of his wife, and his own male descendant, and the question was re-asked. The cycle was repeated until they answered 'Kazakh' or else stopped me by declaring the exercise futile since no descendant in the male line would ever be a Kazakh.

A medical emergency cut my stay by three weeks and there was no time to complete a satisfactorily large sample for this last question. Both 'essentialists' (as defined by their answers in Figure 1) and non-essentialists were asked, and my sample sizes are $n=8$ for essentialists and $n=6$ for non-essentialists. The answers ranged from "two generations" to "never", and the essentialists gave larger figures. Despite these very small n 's, one observation appears significant: fully one half of the essentialists responded that the child would *never* become a Kazakh, and this was the modal response. In contrast, not *one* of the non-essentialists gave such an extreme answer. If we ignore the distributions and treat this as binomial data about the likelihood of finding the answer 'never' (equivalent to a 'success') among essentialists and non-essentialists alike, even these small samples yield a relatively clear result ['essentialist' mean = .5 ($n=8$), 'non-essentialist' mean = 0 ($n=6$); Pearson chi-sq. = 4.2, $p = .07$]. This suggests that essentialism is connected to an intuition of 'intrinsic' nature and therefore inalterability.

VI. Study 4

This Study was elaborated to address the following question: are these ethnies or ‘races’? *Bona fide* human races from the biological point of view do not exist (Boyd & Silk 2000, Brown & Armelagos 2001), so the question I pose is not really ontological but epistemic. My respondents clearly think of Mongols and Kazakhs as phenotypic groups, as evidenced by their response to the first question in Study 3, that a child of Mongols adopted by Kazakh would have a Mongol physical appearance. Since it has been shown that humans typically essentialize what they think are ‘races’ (as shown by Hirschfeld 1996), then it could be argued that the physical differences between Mongols and Kazakhs are duping them into thinking that they are looking at races rather than ethnies. If this is going on, it would explain my results. From this perspective one could argue that I have not shown a thing about ethnic cognition. All I have done is provide further evidence for how people process what they think are races. However, I think the issue may be more subtle. I believe it *is*, indeed, true that because my respondents believe these categories—Mongol and Kazakh—to be phenotypic, they are, in fact, thinking of them in racist terms. But the question for me is: does the environment of their experience provide sufficient justification for this belief? Given the facts of phenotypic distribution in the area, should my respondents—on empirical grounds alone—expect the adopted child of Mongols into the Kazakh community to look different from the adopters?

Suppose that the empirical facts of the phenotypic distributions do not support this expectation. Then it may be that the intuition of phenotypic differences *follows* from essentializing the ethnic categories. This could certainly happen if we intuitively process ethnies as ‘species’, because species cognition includes the expectation of category-based morphological uniqueness (which may be an even stronger bias than the general category accentuation effect; Hogg & Abrams 1988:19). Of course, phenotypic differences may cause essentialism too (and Hirschfeld’s 1996 data certainly suggest this very point), but if in the absence of such differences ethnic groups are biologized—with associated essentialism—we should ask ourselves why. This latter question is pursued in Gil-White (2001a). Here we are merely concerned with the empirical question of the actual phenotypic differences between Mongols and Kazakhs in Bulgan Sum, and thus with the question of whether the obvious racism of my respondents is based more on actual or imagined physical differences.

I chose this site because the apparent phenotypic overlap of the local groups suggested that most individuals in either ethnie fell within the range of variation of the opposite one. Both Torguuds and Kazakhs in Bulgan Sum are characterized by astonishing phenotypic variation, including even some blondes in both groups. It is hard to provide a description of a ‘prototypical’ Mongol or Kazakh using the populations of Bulgan Sum as my guide. Most of the Torguuds of Bulgan Sum (and certainly the Bangyakhlan clan, with which I worked) arrived in the 1940s from Xinjiang, just across the border with China, which lies smack against Bulgan Sum. That region is called Jungaria, and Torguuds returned to it at the end of the 1700’s after having migrated to the

banks of the Volga and spent two-hundred years herding there. During that time they raided and waged war against Cossaks, Kazakhs, Russians, Nogay and others, sometimes taking individual slaves, sometimes enslaving whole groups and forcing them to migrate with them (Khodarkovsky 1992). In the process, much genetic mixing took place, resulting in a rich variety of phenotypic attributes that even Torguuds themselves recognize is rather wild. The Kazakhs of Bulgan Sum are also recent arrivals from Jungaria. Bessac (1965) reported that the Kazakhs in that area (which is richly multi-ethnic) were in the habit of stealing children from Tibetan and Mongol communities (and perhaps others as well) and raised them as their own. In addition, Kazakhs were as prone as the Torguuds to raid and enslave their neighbors. Kazakhs further West have been in contact with Russians for a long time, as well as engaging in considerable migrations throughout this region of Central Asia. The end-result of these processes is that the two groups are now such that they appeared to my untutored eyes as having considerable phenotypic overlap and extraordinary within-group variation.

A. Pre-test

In 1998 I conducted a pre-test to see whether a more rigorous one would be worthwhile. A sample of 8 Torguud and 8 Kazakh individuals was deliberately chosen as a best-case sample to confound judges who tried to distinguish them perceptually into Mongol and Kazakh categories. If this sample created no uncertainty, there would be little reason to investigate further. Each photo is a close-up of the face without hats or any identifying markers of ethnicity, taken against a portion of wall with no decorations which therefore gave no clues. Every photo is of a relatively young adult, to avoid older Kazakh males who groom their facial hair in ethnically particular ways.

The judges were a group of 19 students from a mathematics class at the National University in the capital city (in Bulgan Sum finding a comparable sample of judges who had never met at least some of the individuals in the photos would have been very difficult). The students guessed one by one, and the order of the photographs was randomized for each.

Their performance overall was 33% correct guesses. This is worse than chance (which has an expectation of 50% correct guesses), and indeed the per-photograph analysis confirmed that not only had I created uncertainty in the judges but in fact had managed to systematically fool them—with respect to *some* photos—into thinking it was a picture of a member of the opposite ethnies. This encouraged me to do a more thorough study. The following test was done in the spring of 2000.

B. Methods

Using a digital camera, I took photos of 10 Torguud and 10 Kazakh individuals, with the following demographic breakdown. Kazakhs: 6 adults, 3 male and 3 female; 4 adolescents, all male, and no children. Mongols: 2 adults, both female; 5 adolescents, 4

male and 1 female; and 3 children, 1 male and 2 female. The two sets of photographs are somewhat unbalanced because the procedure for collecting individuals was more aimed at avoiding my biases than at achieving demographically balanced sets.

This procedure was as follows. At the inter-ethnic children's spring festival, held outdoors, I announced to anybody who would listen and care that I was taking photographs for my work and that whoever had his/her picture taken would be able to keep it (they are being sent by mail). However, only those for whom the roll of a dice revealed the numbers 1, 2, or 3 had their picture taken. This avoided my biases in inclusion decisions. Biases in recruitment were also avoided because most of the recruiting to get people to roll the dice was done by other people, who eagerly brought friends and acquaintances to try out. When I noticed that I was getting too many people in one category, I did attempt to balance by telling my recruiters "Now I need adult women" or "Now I need children" etc. But this part of the procedure was *ad hoc* rather than systematic and I thus failed to achieve perfectly balanced sets. Male adolescents are both greatly represented and well balanced because they were disproportionately eager to participate. As before, middle-aged adults were avoided because of the ethnically particular grooming of Kazakh facial hair. The pictures include heads and shoulders, without hats or other distinguishing markers, against the backdrop of the outdoors, which gives no clue as to the ethnic membership.

The test again involved residents of Ulaanbaatar making judgments as to the ethnicity of each person in the set. The sample size is $n = 28$, 14 of them female. There is one teenager and an 80-year-old person. The rest are in their 20's, 30's and 40's. In order to recruit judges, I sat at a table inside the electronics store of a friend, and had an assistant recruit potential participants. Each willing participant was sat at a table in front of my laptop and I then had them look at the pictures, one by one. Each picture filled the 13" screen almost to its entirety. When they gave the answer for a picture, I moved to the next one. Sometimes people expressed a desire to change their minds immediately after the photograph had disappeared from view, and I allowed them to look at it again. The order of the pictures was the same for all participants and it alternated a Kazakh photo with a Mongol photo. Participants were not told of any order or alternation in the photographs, though this could be inferred if they were good at identifying Mongols and Kazakhs. This part of the design may thus create a bias to help participants make the identifications.

C. Results and discussion

Table 6 shows the photographs in descending order of the accuracy with which they were identified. K and M stand for "Kazakh" and "Mongol", and the numbers identify the ordered position of each photograph in the sequence. The mean = 75% correct guesses summarizes overall performance for the combined sample of Torguud and Kazakh photos. Sex and age of the judges does not appear to have an effect on performance. Spearman's rank correlation coefficient for age is .3, but appears not to be a

real effect ($p=.2$). Sex likewise does not appear to affect judgments [‘male’ mean = .76, ‘female’ mean = .74; T-test: $t=.48$, $p=.63$). In addition, respondents (all of them Mongols) did not guess better with photos of one group than with the other [$\Pr(“K”|K)^4 = .74$, $\Pr(“M”|M) = .76$; Paired T-test: $t = .342$, $p = .735$].

Table 6. Guesses of $n=28$ participants.

<i>Photo</i>	<i>K6</i>	<i>M1</i>	<i>M5</i>	<i>K7</i>	<i>K10</i>	<i>K4</i>	<i>M2</i>	<i>K3</i>	<i>K5</i>	<i>M3</i>	<i>M7</i>	<i>M10</i>	<i>M4</i>	<i>M8</i>	<i>M6</i>	<i>K9</i>	<i>K8</i>	<i>K2</i>	<i>K1</i>	<i>M9</i>
% correct	100	96	93	93	93	89	86	86	86	82	82	82	79	75	64	57	46	50	43	21

If Mongols and Kazakhs in Bulgan Sum actually constitute one rather than two phenotypic distributions, participants guess at random and we expect 50% correct guesses. If these are completely different distributions for all the diagnostic traits (e.g. the task is comparable to differentiating Kalenjin from Swedes), then we expect 100% correct guesses. The obtained average suggests the overlap is exactly between these two extremes.

But it is best to *see* this overlap. We cannot infer the overlap in two n -dimensional distributions where each dimension is some measurable phenotypic trait. We can only infer the overlap between two probability density functions along what one may call a ‘gestalt’ trait that has ‘looking like a Mongol’ on one end, and ‘looking like a Kazakh’ on the other, and which is presumably expressed in a respondent’s psychology (as a result of the combined effect of perceiving the values of multiple diagnostic traits). If such a gestalt ‘trait’ is indeed a psychological reality, and if it in turn underlies people’s guesses that the stimulus picture is of either a Kazakh or a Mongol, then inferring the overlap between Kazakhs and Mongols along this trait is meaningful. This inference is exactly equivalent to the problem of establishing signal strength in signal detection theory (Moore 1989:131-136). In order to estimate this overlap, we assume that the distributions are approximately normal, and have approximately equal variances. Both Kazakhs and Torguuds have a history of much wandering and much mixing, and both struck me as having extraordinary variation, so the assumption of roughly equal variances is probably sound. Since most phenotypic traits usually approximate normal distributions, a gestalt distribution resulting from them may also be approximated with a normal.

My data estimate the following probabilities: $A = \Pr(“K”|K) = .74$, and $B = \Pr(“K”|M) = .24$. With these, the estimated distance between the means of the two

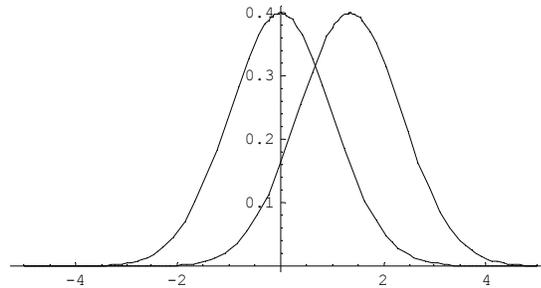
⁴ This is the probability that a photo was voted “Kazakh” when it was indeed Kazakh.

density functions, expressed in standard deviations, is a sum of Z-scores: $d = Z_{(1-A)} + Z_B$. This gives:

$$d = .64 + .71 = 1.35 \text{ SD}$$

The following figure illustrates the overlap between two normal distributions with equal variance, where the difference in the means is 1.35 SD.

Figure 2. Two normal distributions. Left distribution is $N(0,1)$, right-hand distribution is $N(1.35, 1)$.



Given this degree of overlap, the fact that my Torguud respondents in Bulgan Sum unanimously expected a Mongol child adopted by Kazakhs to appear different seems to exaggerate the actual differences, and therefore suggests that categorization into two different ethnies biases people's expectations of phenotypic differences, and also that 'racial' phenotypic differences may not be necessary for essentialism.

However, I confess that this result is less than decisive. I have no rigorous data on the degree of confidence with which judges guessed, but I did notice a great deal of hesitancy, even for photos that were on average correctly identified. If I were to do it again I would collect, after each guess, a likert scale self-report of the judge's confidence. This would improve the estimate of the percentage of members from one group who would look anomalous if they were surreptitiously introduced into it as children.

But raising this issue points out another methodological shortcoming. Even the Torguud intuitions on this point could have been gathered with greater subtlety. It is true that my Torguud respondents unanimously said the appearance of the adopted child would differ from the adopting community, but I could have then prompted them to specify whether 'always', 'most of the time', 'half of the time', or 'sometimes'. This would yield more informative data that would admit of a truly useful comparison of their intuitions against the measured overlap. I will attempt to gather such data at the next opportunity.

The more methods are used to investigate a question, the better. Validity for a construct results from testing it with different instruments and finding that the results

converge across such disparate tests (Campbell & Fiske 1959). Thus, let me point out two more ways to test whether ‘racial’ differences are necessary for essentialism.

Anecdotal data suggests that Torguuds essentialize intra-Mongol ethnic boundaries (e.g. between Torguud and Uryanxai Mongols). If true, this will be a better refutation of the idea that ‘racial’ differences alone drive essentialism, for the phenotypic overlap here is much greater still than between Torguuds and Kazakhs. I will test this in a future fieldtrip.

Second, since Hirschfeld tested for essentialism by presenting children with the equivalent of the Kalenjin/Swede contrast alluded to above—where members of the two groups differ on all their diagnostic traits and therefore an identification test would yield 100% accuracy—this presents the possibility of a different test. One could repeat Hirschfeld’s experiments with a phenotypic contrast like that found in Bulgan Sum. Find two populations for which guesses will yield an overlap in the gestalt ‘trait’ distributions as above, then see whether children will essentialize to the extent found in Hirschfeld (1996). If they don’t, this is evidence against the idea that the ‘racial’ (as opposed to the *cultural*) phenotypic differences in Bulgan Sum are responsible for the essentialism.

One final point. It is possible that participants in this study were using as cues subtle differences in dress style. To examine this possibility, I repeated the test with a new sample ($n = 27$; 15 males and 12 females, ages 17 to 60), but cut the photographs so that only the face was apparent. The methods were all as before and the sample of judges demographically very similar to the first.

This hypothesis was not supported. The paired-samples T- test (pairing the ‘head and shoulders’ version of each photograph with its ‘head only’ version) actually yields a small and nonsignificant increase in accuracy of 3.3% [$t = -1.4, p = .17$]. Closer analysis revealed that most of the change was carried by only one photograph which in its new position was susceptible to a ‘run’ effect (because it was now preceded by several photos reliably guessed to be Kazakh). I had changed it from the first to the last photograph to start respondents off with a Mongol rather than a Kazakh photo, just in case there was a strong priming effect of the first photograph. That respondents were susceptible to such ‘run’ effects suggests of course that randomizing the order for every participant is better, but it reassures me that the small increase in accuracy is spurious. (Order- randomization for each participant was not done because this is more time consuming with a computer sequence than with a stack of printed photographs, which can be shuffled, and my time constraints were severe. In more favorable circumstances, one should randomize.) In sum, I conclude that my respondents were relying solely on phenotypic attributes and were not using clothing as a cue.

VII. Summary

The studies reported here sought to investigate whether the rules for ethnic membership held by ethnic actors in the locality of Bulgan Sum are mostly ‘blood’-based or enculturation-based. For the most part, they are ‘blood’-based. It was also found that this model of membership carries with it the intuition of a shared essence for members of an ethnies, which essence is taken to causally underlie ethno-typical thought and behavior. Finally, an empirical test of the phenotypic distance between the two contrast groups suggests, though not conclusively, that the essentialism may not be primarily driven by the physical differences between the two groups.

The methods used seek, to the degree possible, an approximation of those methods which psychologists have perfected for lab-based research. There are several drawbacks to this sort of an effort. The implementation of such methods in the field can be difficult and time-consuming, follow-up studies cannot be as thorough as one might desire, and there is very little room for error in terms of experimental design, because it is prohibitively expensive to redo an experiment. Despite these shortcomings, and despite the fact that the quality of the data thus obtained cannot have the same rigor as that obtained in a lab experiment, this form of investigation, in combination with ethnographic research, is a powerful tool that leads to greater confidence in what is being described ethnographically. The flipside is good too: a comparison of cross-cultural results obtained in this manner with results from lab experiments helps establish the cross-cultural validity of claims that, if based solely on lab-based research, would languish in a perennially uncertain status. Marrying anthropology and psychology in the theoretical and also methodological sense is a worthy goal to strive for in future work.

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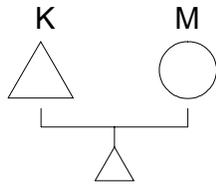
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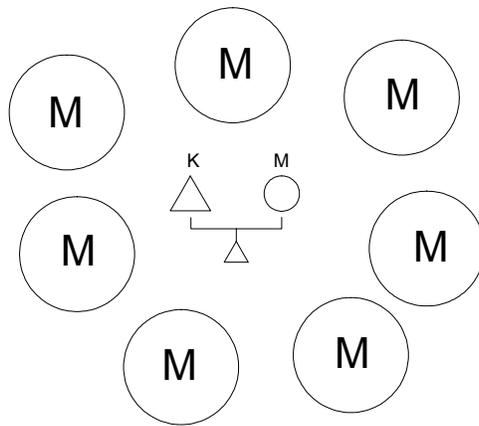
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Appendix (diagrams used)

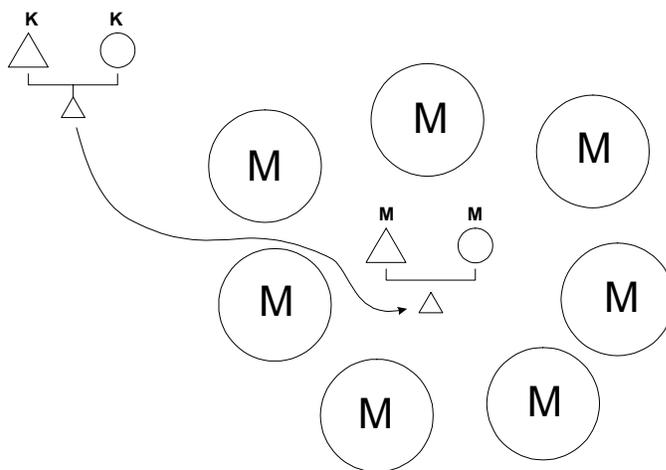
Study 1, Question 1



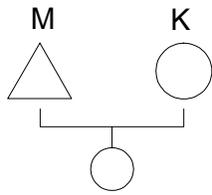
Study 1, Question 2



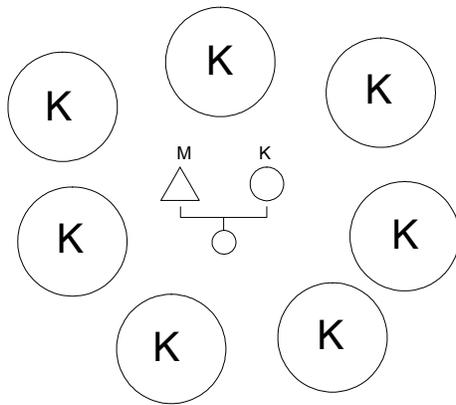
Study 1, Question 3



Study 2, Question 1



Study 2, Question 2



Study 2, Question 3

