# How Conformism Creates Ethnicity Creates Conformism (and Why this Matters to Lots of Things)

In this essay I will explore the important connection between conformism as an adaptive psychological strategy, and the emergence of the phenomenon of ethnicity. My argument will be that it makes sense that nature made us conformists. And once humans acquired this adaptive strategy, I will argue further, the development of ethnic organization was inevitable. Understanding the adaptive origins of conformism, as we shall see, is perhaps the most useful way to shed light on what ethnicity *is*—at least when examined from the functional point of view, which is to say from the point of view of the adaptive problems that ethnicity solves. I shall begin with a few words about our final destination.

## *Ethnicity and ethnocentrism*

Ethnicity is a phenomenon that rightly occupies much attention in lay and scholarly circles alike, because it is relevant to almost everything that humans do. What is it? From the descriptive point of view, ethnicity is normative culture. That is to say, an ethnie is a collection of human beings who more or less agree on how a human life should be lived: which foods should be avoided, which eaten, and how the latter should be prepared; what sorts of behaviors are funny, shameful, offensive (and which aren't); by what specific ritual displays should politeness be expressed in a million different contexts; what forms of dress and cosmetic enhancement are appropriate for members of either sex; etc. Ethnicity is a collection of 'oughts' and 'ought nots' that get passed down more or less as a package along with the associated social label inherited from one's parents; "I am an X."

In some academic circles, the question "Which ethnie has figured out the right way to live?" will immediately be met with the following retort:

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"Why, the premise is absurd! Why should there be one best way to live a human life?" Perhaps. But this cosmopolitan multiculturalist complaint belongs to a clear minority. To the same question, most human beings all around the world have a ready answer, and it is always the same; "*My* ethnie lives life the way a human should." Consequently, members of ethnie A can easily amuse, offend, or shock members of ethnie B merely in the act of *conforming* to the 'oughts' and 'ought nots' that As feel obligated to pass down from one generation to the next.

Such haughty or offended reactions are usually labeled 'ethnocentrism', or, depending on their intensity and negativity, 'prejudice' and 'racism'. Many academics consider ethnocentrism a "bad" thing in any of its forms. But is it? *Yes*, it is a bad thing, very much so. The values of science require that we root out from our observational methods any source of consistent, distorting bias; and believing that *cultural difference* implies *error* makes it well-nigh impossible for the social scientist to make much progress in the study of cultural variation. Even more important, by my lights at least, is that so long as we are not cosmopolitan and therefore tolerant and compassionate with respect to the ways of our neighbors, we are still moral failures.

That said, however, might not ethnocentrism be "good" from an evolutionary point of view? That's a perfectly separate question. However, it is hard for people to see this because they easily forget that the famous terms 'good' and 'bad' are utterly dependent on the speaker's transitory frame of reference for their meaning. From the point of view of an evolutionary analysis, 'good' is whatever promotes demographic success and 'bad' is whatever hinders it. These descriptors may sometimes overlap with those corresponding to a moral frame of reference (such as modern humanitarianism), but they need not and they often won't. To pick the most overused (because useful) example, the moral frame of the Catholic hierarchy will celebrate a celibate and chaste priest as "good" (officially, at least), and yet from the *organic*, *evolutionary* point of view the genes inside a celibate and chaste priest find themselves in a dead-end and cannot make it to the next generation. So in the Catholic case something that is morally "good" is "bad" from the point of view of natural selection.

Although the term 'adaptive' is more proper when speaking of evolutionary processes, and I will insist on it, the question "Is ethnocentrism

adaptive?" does not remove the semantic problem because the first connotation of 'adaptive' *is* 'good'. Here's the demonstration: the phrase 'it is good to be well adapted' raises no eyebrows whereas the phrase 'it is bad to be well adapted' will require an explanation and a special context. That 'adaptive' should have the connotation 'good' is unfortunate, in a sense, because—except for those trained in evolutionary theory—it does not specify 'good' *for what*, and since it doesn't, the temptation is to distend 'good' in all its possible directions: pleasant, desirable, efficient, *moral*. . . . The semantic trap has therefore the power to ensnare our minds and prevent the growth of scientific understanding. So I make myself clear: although I will defend below the argument that the psychological strategies responsible for ethnocentrism are adaptive and therefore "good" for genetic evolutionary success, we must remember that this judgment is entirely neutral with respect to morality. The question here is merely whether ethnocentrism is common because the psychological mechanisms that underlie it have been selected for in the human species; we are certainly free to make moral choices that contradict nature's own.

To suggest otherwise—that people are compelled to reify as moral everything that is natural—is to commit what philosophers call the "naturalistic fallacy." The definitive refutation of this fallacy is empirical: despite the fact that humans all share one and the same nature, there is a staggering variety of human clusters, called 'ethnies', each organizing human life around radically different sets of 'oughts' and 'ought nots', and humans are quick to condemn each other from the perspective of their own parochial cultural frames.

I have pointed out this land-mine the better to walk us carefully around it, because social science requires a causal analysis of the origins of a phenomenon, and we must therefore not mix it up with a moral evaluation of the same. But the road to scientific understanding is here booby-trapped in other ways, and something else that lies in ambush is the temptation to pathologize the phenomenon of parochial prejudices. In other words, those of us who consider prejudice unfortunate and best transcended gravitate quite naturally to placing it in some category of "sickness." However, if such behaviors were selected for, then they are functional, in which case conformism is adaptive, and so are its downstream consequences: ethnicity and ethnocentrism. This is the argument I will defend

below, and, if it is reasonable, then treating such phenomena as somehow "pathological"—synonymous with "malfunctioning"—just because they offend us will be grossly misleading.

Consider a parallel. The sexual attraction that a grown man feels for a fifteen-year-old female is perfectly natural, the product of a *well-functioning* adult, male human brain. Saying this out loud is obviously quite different from endorsing their sexual union because, once again, there is no requirement that everything natural be considered moral or legal. But if a society wishes to segregate the sexual behavior of "adults" and "children" with minimal harm to both and to bystanders, then it will be wise to understand the psychology of normal sexual attraction rather than pretend the things it wants to prohibit are pathological merely because they are prohibited. Similarly, recognizing the adaptive nature of conformism and its ethnocentrist consequences hardly demands that we agree with the invidious judgments that people make about "others," much less that we apologize for them.

The parallel is useful for making another point. Even if it is perfectly normal for a grown man to find adolescent women attractive, it is also quite obvious that most grown men who live in societies where this is considered beyond the pale have no trouble restraining their urges. In like fashion, if ethnocentrism is natural, nothing stops us from condemning it, and we can expect that growing condemnation will make it increasingly rare, just as sexual relations between grown men and adolescent women are less common where frowned upon.

In my view, if we wish to transcend ethnic strife, then we will be wise to understand the role that perfectly normal human psychological functions play in producing it. Such an understanding is all but impossible without a functional/historical—i.e., evolutionary—perspective that investigates *why* normal human psychological behavior is what it is, both when we like it, and also when we don't.

*My approach: Dual-Inheritance Theory*

'Meme' is a convenient gloss, fashionable these days, for an idea, behavior, belief, etc.—anything that can be transmitted through social learning. Cultural populations, in this view, are distributions of memes that evolve by non-genetic processes of Darwinian selection, as some

memes get more copied than others. The result, colloquially, is "history," and so a proper science of history will have to be based on the laws that govern the transmission of memes from one generation to the next through various processes of social learning. A small but growing field of scholars is concerned with specifying the laws of cultural transmission so that a mature and explicative science of history can be put together, and I count myself among them. Despite being a small field, there is more than one school of thought here. My own is called 'Dual-Inheritance Theory' and it was hatched by Robert Boyd and Peter Richerson, with *Culture and the Evolutionary Process*, in 1985.

Now, what might be an example of a cultural-transmission law? One of Boyd and Richerson's original proposals was that humans have a conformist bias for the acquisition of information. In other words, the claim is that if a meme enjoys a plurality in some local environment, people will find this meme "tastier," so to speak, merely because it is more common. If this candidate law applies, knowing this will be maximally useful if we can also say with some precision how the human brain decides what the "local" environment is. Why? Because a meme may have a plurality in my neighborhood and yet at the same time it may be rare in my city. I will have a few things to say, towards the end, about how the mind solves such "sampling" problems, but for our present purposes we will just assume that humans have a parsing mechanism that tells them what the relevant "local" environment is for any particular meme (so as to determine if it has a plurality there), and leave it at that.

I will defend here that it is indeed a law of psychology that humans are conformists. Then, with that for premise, I will lay out a relatively straightforward argument for the emergence and function of ethnicity, and, by extension, ethnocentrism. However, before I begin I want to distinguish analytically between two types of conformism, because one kind will be more important to us than the other: *information-gathering* and *norm* conformism.

### *Two analytically separable types of conformism*

In information-gathering conformism, we acquire the locally most common memes because we are betting (not always consciously) that such memes will be appropriately useful for dealing with the current environment, broadly construed. For example, if you are a hunter-gatherer

and there are three competing memes for how best to find a particular kind of prey, you will prefer the meme with the most adherents, betting that this strategy will give you the best results on average. A simple modern example is finding the exit in an unfamiliar building—the best first guess will be to follow the flow of the greatest number of people. Thus, we are here trying to gather the best information about our local environment.

In norm conformism, on the other hand, we acquire common memes that have no necessary relation to the (physical) environment itself, but which facilitate interaction with other human beings. If most of the people around you speak English, then it makes sense to learn English, because you need to communicate with others for practically everything you would like to get done. If most people around you think the proper greeting is a handshake, as opposed to a bow, then it makes sense to shake hands, to avoid offense or puzzlement. The point here is that we are trying to *coordinate* with relevant nearby humans as best we can.

It is the second kind of conformism that I will focus on most strongly, because it is most relevant to the phenomenon of ethnicity; but I will say first a few words about the likely evolutionary story that explains the emergence of conformism in any of its forms. The story leading up to the emergence of conformism begins with the evolution of imitation.

### *True imitation*

What makes humans different from non-humans? This is an old question, and all sorts of answers, religious and secular, have been advanced. But recently, the study of primates—and non-human behavioral ecology more generally—has made popular the argument that in fact there is no quantum leap from non-humans to humans. According to this argument the differences are all a matter of degree. Some of us, however, remain old-fashioned. I believe there *is* a quantum leap of sorts. On the one hand, experiments have shown that human infants only minutes out of the womb can already imitate, which shows just how important this ability is for what it means to be human (Meltzoff 2002:23–24). On the other hand, our closest relatives cannot seem to pull it off (see below).

It is true that a few not-closely-related species learn by imitation, but the case for their relevance to humans is not strong. For example, songbirds

learn song by imitation, but they hardly learn anything else this way, whereas humans are generalists. The same point can be made for cetaceans, which apparently exhibit vocal imitation (Janik and Slater 1997). Moreover, humans are descended neither from songbirds nor cetaceans. The imitative abilities of Grey parrots appear to be somewhat more impressive (Pepperberg 2000), but this lab-induced behavior may have little relevance to parrots in the wild—and, once again, we are not descended from parrots. It is therefore quite significant (1) that imitation in any form appears to be quite rare, (2) that generalized imitative abilities appear to be unique to humans, and (3) that our closest relatives cannot imitate at all. So I agree with Kinsbourne (2002:311) that, "imitation [is] a prime suspect for being a precursor of much that is uniquely human in human cognition, including its enrichment by sociocultural influences." In other words, it may be that imitation is the quantum leap—the change that underlies everything else that *we* can do and non-humans cannot.

I hasten to define 'imitation'. The term is loosely used to cover many different forms of social learning, but as scientists in animal and human behavior have studied the various ways in which information can make it from one individual to another, finer distinctions have emerged, and with them a new vocabulary. The new taxonomy of terms includes 'stimulus enhancement', 'local enhancement', 'goal emulation', 'true imitation', and so forth. The term 'true imitation' is often defined from the point of view of what is methodologically required to demonstrate that it has taken place, so as to rule out confounding it with other social-learning phenomena in any particular experiment. This is of course necessary and useful, but for our purposes a conceptual definition will be best: 'true imitation' is the ability of an individual to acquire the details of another's *technique* by means of observation. The following example will illustrate the difference between humans and non-humans and also the usefulness of this definition.

Michael Tomasello and colleagues carried out a series of experiments that, by comparing humans to common chimpanzees (*Pan troglodytes*) and orangutans (*Pongo pygmaeus*), vividly illustrate the cognitive/behavioral uniqueness of humans (Nagel et al. 1993; Call and Tomasselo 1994). At the far end of a table, beyond the reach of a caged ape, they placed (say) a piece of candy. The experimenter then demonstrated for the ape the

use of a rake-like tool that could be used to drag the candy to where the ape could take it with its hand. Like a rake, this tool has 'teeth', but opposite the teeth it sports a spatulated edge. The easy way to get the candy is by putting the *edge* against the table (with the teeth face up and away from the table's surface). If one instead puts the teeth against the table the candy keeps slipping between them. When the experimenter demonstrated for the apes either way of doing it, they were oblivious to the technique employed. For example, starting out with the tool in the "teeth down" position, the experimenter would demonstrate by picking it up and turning it around so that the spatulated edge would be against the table's surface, then effortlessly dragging the candy to himself. The tool was then returned to its original "teeth down" position and a new candy was placed beyond arms' reach. The apes had no trouble understanding that the tool was causally connected to the end-goal of getting the candy. But as for technique, they paid no attention: they did not turn the tool around as demonstrated, but tried repeatedly (and awkwardly, and ineffectively) to get the candy with the tool in its "teeth down" position. Eventually they got better through trial and error, discovering on their own the usefulness of the spatulated edge, so the point is not that apes are stupid but that they can't imitate. Apes, in other words, cannot *ape*—the colloquial verb is a misnomer. Human children (2-year-olds), by contrast, copied exactly what the demonstrator did, with no hesitation. In fact, the children insisted on a verbatim reproduction of the demonstration even when the less efficient technique had been shown!

What does this show? That human infants are *obsessed with technique*, sometimes even to the detriment of the goal sought, while apes by contrast take zero notice of technique, notwithstanding their ability to extract from a demonstration the causal connection between a tool and the satisfaction of a goal. Unlike apes, therefore, humans learn *directly* by observation, without an intermediate stage of trial and error (provided the motor coordination involved is not so refined that it requires rehearsal). *Human see, human do*, in other words, but since this does not apply to monkeys, we have found another colloquial misnomer (the reputation non-human primates have for being good imitators has really not been fairly earned).

It is true that some apes have shown moderate imitative abilities, but this invariably appears to require that they be raised and trained by humans—it doesn't happen in the wild. Tomasello and Call sum up what we know about this (1997;288–89) as follows:

> The issue is that all of the strong evidence for imitative skills comes from apes who have been raised and trained by humans in all kinds of instrumental and social skills. These studies likely do reveal ape capacities for imitative learning, but such capacities have not been found in apes who have not had extensive contact with humans. . . . Of the 13 anecdotal observations that Whiten and Ham (1992) report as possible instances of ape imitation, 11 are imitations of human behavior by human-raised apes (usually with human artifacts). And, indeed, the two studies that have compared apes with different amounts of human experience . . . have found dramatic differences (Hayes and Hayes 1952; Tomasello, Savage-Rumbaugh, and Kruger 1993).

On this evidence, imitation is a good candidate for a "quantum leap" separating humans from our nearest cousins. Of course, one could say here, what about language, prestige hierarchies, religion, large-scale cooperation between genetic strangers, etc.? Don't non-humans lack these as well? They do, but if the argument can be defended that most of the things that make us uniquely human evolved *because* a capacity for imitation first emerged in the human lineage, then it is entirely fair to say that imitation—or "*true* imitation," in any case—is the qualitative difference that, over time, has made one particular kind of ape *human*. Boyd and Richerson (1985) were the first to defend this argument explicitly, and they produced specific models to demonstrate that, given capacities for direct and more-or-less exact observational social learning, conformism followed, which in turn made group selection leading to the uniquely human phenomenon of cooperation among relatively large groups of genetic strangers possible (for recent work see Soltis, Boyd, and Richerson 1995; Henrich and Boyd 1998, 2001; Gintis *et al.* 2003). Arguments for the evolutionarily coercive impact of imitation have also been advanced for the emergence of prestige hierarchies (Henrich and Gil-White 2001), which are unique to humans, and for language *via* prestige hierarchies (Gil-White 2005b *in prep.*). Since more complex human phenomena, such as religion, certainly depend on conformism, cooperation among genetic strangers, prestige processes, and linguistic interaction, they may be considered downstream effects of true imitation. If this body of work has merit, then, to find that non-human primates cannot truly imitate is to find the *key* qualitative difference that makes us uniquely human.

It is easy to defend that true imitation is adaptive. This particular form of information transfer saves individuals the trouble of refiguring out solutions which their predecessors have already found through costly in-

dividual-learning (trial-and-error). Explaining why other species lack anything so useful is by contrast anything but obvious, so I shall avoid that problem here (for a theoretical approach to this problem, see Boyd and Richerson 1996). Instead, I take imitating humans as my point of departure and proceed to ask: how would natural selection refine this capacity, once it was in place?

Boyd and Richerson (1985) reasoned that "emergent phenomena"—*patterns*—would result at the population level merely from the fact that everybody was an imitator. Because an ability to notice population-level patterns can promote inferences with adaptive value, natural selection will favor individuals with mutant genes coding for a psychology that perceives and adaptively responds to such patterns. As a result, mutant genes of this sort will spread (assuming the costs of the new psychological equipment are paid for by the benefits, of course). Over time, as improvement is piled upon improvement, we will get the evolution of a well-integrated battery of psychological biases jointly designed to squeeze as much reproductive benefit as possible from noticeable population-level patterns. As you may have already guessed, another word for these patterns is 'culture'.

In the jargon of evolutionary theorists, true imitation is a *preadaptation* because, once it evolves, it makes the evolution of refinements to it possible. But it is also a *selection pressure* because individuals who fail to adapt to the emerging patterns unleashed by true imitation will be disfavored. As a result of both effects, conformism is an obligatory outcome when you have a group-living species that uses "true imitation," but in order to get there we must first climb the intermediate steps, and I turn to these below.

*Leap 'n' crawl (guided variation)*

Since social learners will also be individual learners (because individual learning predated social transmission), Boyd and Richerson (1985) first considered the question of how individuals could combine direct social learning (e.g., imitation) and individual learning in the most adaptive way. Their models demonstrated that optimal "naïve individuals" should first rely on socially transmitted information (i.e., the knowledge of their cultural "parents,") and only *then* refine the received wisdom through individual learning. Their reasoning is as follows:

Individual learning is a trial-and-error process in which the learner retains a "better" variant when more or less accidentally hit upon. This implies the ability to rank different memes according to how well they accomplish a particular goal; that is, the ranking ability is what gives the learner a criterion to establish that a given variant is "better"—that a given trial, in order words, is not an error, but something to be retained and repeated. But trial-and-error, depending on the number of trials, can take time, and it can also lead to costly errors. If a child can learn by observation and copy directly her cultural parent's meme, she can save herself such costs.

So imitative social learning is adaptive because it saves us a lot of time, sweat, and risks that otherwise would have to be expended reinventing all sorts of wheels. However, there is hardly any reason to throw away trial-and-error learning entirely. If the copied behavior can be made even better, it would be silly not to. So, after copying the behavior of a cultural parent, a learner should attempt to improve it another notch, if possible, through individual learning.

In other words, first you *leap*—and you do so in two different senses at once—and then you crawl.

In the first place your leap is a leap of *faith*—because you acquire information socially without subjecting it to the scrutiny of individual experimentation, in fact "trusting" that the earlier evaluations of others will be sufficient (recall that children in the experiments mentioned above copied the technique exactly, whether it was good or bad for attaining the goal: they simply trusted). Second, it is a leap of *knowledge* because you will often acquire information that simply *cannot* be acquired on your own without lengthy experimentation, if it can be acquired this way at all. This will be the case whenever the meme (or functionally related collection of memes) has accumulated complexity over many generations of refinements.

Having leaped, now you *crawl*. That is, the meme once acquired, you now sweat a bit (or a lot) through individual learning in order to achieve small adaptive refinements to what you've socially inherited. This combination of individual and social learning will make the cultural "children" of each generation, on average, more skilled than the cultural "parents," pushing the population gradually in the direction of the optimal meme (and this is why Boyd and Richerson call it *guided variation*, for what

moves in the direction of the optimal meme is the population average plus the cloud of variation around it).[1]

We must be careful to say exactly what we mean. The resting place is the optimal meme *with respect to those psychological adaptations that determine how memes get ranked*—i.e., with respect to "utilities" such as pleasure, pain, fatigue, interest, etc., which may be aroused or not by the performance of any particular meme. If humans are still living in an environment in all essential respects similar to the one in which those meme-ranking utilities evolved, then the "tastiest" memes will tend to be those improving people's chances of leaving descendants (i.e., their "fitness") the most. But if humans have recently invaded new ecological zones and also created wholly artificial environments in which to live (as they have), then memes that satisfy their utilities optimally will sometimes be doing so at the expense of their biological "fitness."

Before we move on, notice that to make this all work we must have an environment that is not too unstable. Just as hand-me-down clothes will not likely be worn if the fashion has become very different, so the energy savings of received traditions are a false benefit if the environment has changed so much that they are no longer useful. Should the environment fluctuate wildly every generation, for example, it is obvious that the meme arrived at by the cultural parents will not likely be useful to their offspring, abolishing the benefit of social learning.

However, the environment cannot be too stable if cultural transmission of the human sort is to emerge. Why? Because if the environment is perfectly stable, then a reliance on social learning can easily have higher costs than just evolving genes coding for innate knowledge structures specific to that environment. The reason for this is that anything learned is subject to errors, and it also forces the learner to incur a "start-up" cost because, during the time you are learning, you pay the cost of not having yet the adaptive behavior.

Boyd and Richerson's argument for the evolution of the cultural capacity in humans is precisely this sort of argument: our species was faced, for a good many generations, with a series of climactic fluctuations that were rather violent, but which occurred slowly enough relative to the passing of human generations that the best solution for these ancestors was the particular combination of direct social learning and individual learning that Boyd and Richerson call *guided variation* (what I call 'leap

'n' crawl'). Since the Pleistocene, the era in which our lineage emerged into its modern humanity, was subject to precisely these kinds of fluctuations, their case looks good (Richerson and Boyd 2000a, 200b; Boyd, Richerson, and Bettinger 2001).

Once "leap 'n' crawl" evolves, the stage is set for the next psychological adaptation. Together with social life in aggregates bigger than the nuclear family, guided variation creates the opportunity—and pressure—for *conformist transmission* to evolve.

*Joining the herd—conformist transmission*

If every cultural "child" could learn from only one cultural "parent," then ordinary "leap 'n' crawl" would be the best it could do. But in a group-living species, there is opportunity for greater refinement thanks to the "emergent phenomena"—or population-level patterns—that social transmission sets in motion. The pattern of interest here is that the best memes available in a local population will tend also to be the *most common* ones. Why should that be?

Individuals can copy each other, so anybody who spots a better meme in someone else can adopt it. Therefore, most members of a local population are likely to end up with the same, locally best meme. At any one time, then, the most common meme in the local population will likely be one of the best available. The variation in the population will then result mostly from (1) copying errors (whether from observational or implementational limitations of the erring actors); (2) recent innovations from a few individuals; and (3) learning lags. Given that "leap 'n' crawl" yields an emergent pattern where common memes tend to be relatively adaptive ones, naïve individuals with a bias to start out copying, from the previous generation, the most common memes for each domain of thought and activity, will be taking advantage of this pattern, and will be favored. Hence, conformism—a preference for high-frequency memes—will evolve.

Conformist transmission thus improves naked guided variation by further specifying the nature of the "leap." A conformist cultural "child" will not only begin by privileging "parental" information over individual effort, but will actually discriminate amongst the various offerings exhibited by the various cultural "parents" in the previous generation, favoring whatever meme is most common. Henrich and Boyd (1998) have shown through simulation that over a broad range of environmental con-

ditions, direct social learning in group-living animals leads robustly to the evolution of a strong bias in favor of conformist transmission as opposed to straight imitation of single cultural parents.

But conformism cannot emerge unless you first get true imitation, and here's why. Take, for example, termite fishing, a tool-using behavior that some chimpanzees in the wild have been observed to engage in (Goodall 1986). The tool here is a reed, which is introduced in the opening of a termite mound, and which comes out full of enraged termites that bite the intruding instrument, thereby making themselves available for the chimpanzee to eat. It appears clear that social interaction is involved in the acquisition of this foraging behavior, but since chimpanzees are incapable of true imitation, what a learner gets from a model cannot be the details of his or her specific technique. Rather, as in the experiments mentioned above (Nagell *et al.* 1993; Call and Tomasello 1994), what a learner obtains is the information that the tool (a reed) is important for the satisfaction of the goal (eating termites). Precisely because the specific technique is not something a chimpanzee can acquire, there is neither opportunity nor motive for chimpanzees to attend to the relative frequencies of different kinds of techniques in order to prefer the most common one. And this quite despite the fact that individual learning through trial and error *will* make the more successful techniques common.

We have been discussing conformist transmission for information-gathering purposes—i.e., for solutions to environmental problems. But there is another selection pressure, mentioned above, and not discussed in Boyd and Richerson (1985) or Henrich and Boyd (1998), which favors conformism: the need to maximize the number of successful interactions with other individuals (Gil-White 2001a, 2005a *under review*; see also McElreath, Boyd, and Richerson 2003, which discusses this selection pressure but not in the context of conformism).

The stuff of social life can be thought of as a series of "games" that individuals play with each other. Each interaction or "game" has necessary costs and potential benefits to any given player, and the net payoff to each player is a function of the game strategies simultaneously selected by all players. A now rather large literature, called "game theory," has risen to the task of examining what the equilibrial solution (or "best strategy") is for any specifiable game. Some of these games are called "games of coordination," because in such games the players involved all obtain a

greater net payoff when they successfully match their strategies. What matters most here is not the specific memes involved, but that, whatever these memes are, everybody have the same ones.

So, for example, suppose that you and I are playing the "greeting game." This is a low-stakes game where, if we greet successfully, we both get a payoff which is equal to the time saved not making a second or third attempt to greet each other. If your "strategy" is to bow deeply and mine to extend my hand, my open palm will collide with your head, we will have an awkward moment, exchange excuses, and give it another go. Suppose that this time I choose to bow and you extend your hand, we have another collision, exchange excuses again, cough a little bit and smile, and bravely give it one more try. Etc. We are failing to *coordinate*, and our inability to choose the same strategy results in a cost paid in the currency of wasted time—because whatever else you and I were going to do, we cannot get started until we greet. Notice that *how* we greet is almost entirely irrelevant, but our behaviors do have to match.

Unlike greetings, some coordination games can involve very high stakes. For example, if the game is "driving" and you choose the strategy "on the left" whereas I choose the strategy "on the right" it will not be a happy occasion if we are driving in opposite directions, for here the costs can be considerable, as for example when Americans travel to Britain and have trouble remembering that they are supposed to switch to the "on the left" strategy. Polite excuses and smiles will not typically be forthcoming after a collision, in this case, as the costs are much higher.

Now, of course, in the greeting game the costs and benefits at stake are low only if we restrict ourselves to the game itself. But should we take a longer view, the game doesn't look quite so trivial. If there are a myriad cooperative games for mutual benefit that you and I could play, but there is a rule saying "only those who greet successfully may play," then the long-term costs of failure to greet are quite significant. Naturally, except in the case of secret societies, no such extreme rule applies. If you and I failed to greet a few times, but really wanted to succeed, sooner or later we would achieve some way of deciding that we had both acknowledged each other and that the greeting part of our interaction was over. And if we still wanted to launch ourselves together into some cooperative endeavor—say, for example, that you are going to lend me money to start a store—we could still do it.

However, a bad experience getting the interaction started (an awkward greeting) might give us reservations. Why? Because difficulty matching our behaviors in the greeting game may reasonably be interpreted as a harbinger of things to come. Perhaps we will also have different expectations when we play the cooperative creditor/borrower game? One has to wonder. Just imagine, if you and I are also poorly matched in terms of our ideas concerning how to honor a contract, how to settle a contractual dispute, under what conditions we may renegotiate, the statue of limitations on debt obligations, convertibility of the debt into other currencies, etc., this will lead to costly problems for one or both of us. Hence, to the extent that particular ideas about such things are correlated with particular ideas about how to greet, what happens at the time of greeting is hardly trivial, but pregnant with relevant information. This sort of thing appears to be what "ethnicity" is all about.

Another case to consider is when, despite you and I having similar ideas about contracts and their enforcement, we may have different signaling systems for the steps to be taken in carrying out the relevant interactions, so that when I am saying/doing one thing you interpret it as quite another. For example, if, when I say "I will meet you at 12:00 to discuss the terms of the contract," what I really mean is "I will meet you at 12:30" (because I grew up in an impunctual culture), but you understand my statement literally (because you grew up in a punctual culture), then you will be gone by the time I arrive and we will both have wasted time and energy. Our negotiations could not even begin due to this miscoordination, and you may interpret my apparent default on our agreement to meet as indicative of an inability to meet other, more important obligations. Not insignificantly, we may now both be upset.

Thus, even when people want to be nice and cooperative, they may fail to achieve mutually beneficial interactions if they are poorly coordinated. Instead, misunderstandings, lost opportunities, and even conflict may result. Not because anybody was trying to take advantage of anybody else, but simply because the two parties that attempted joint action had different models of behavior. Given these potential problems, *the adaptive path for an individual in any given local population is to adopt the norms that enjoy a local plurality.* In other words, norm-conformists maximize their chances of entering into profitably reciprocal interactions because

they tend to acquire interactional memes that are already held by a greater number of locally relevant others.

*Evidence for conformism*

Is the human brain designed *for* conformism? Attempts to answer this question begin with the late Solomon Asch (1956) in a series of experiments that became justly famous.

In these experiments, the subject is presented with a mind-numbingly simple task: compare the lengths of some obviously dissimilar lines. Already in the room are six other people who appear to be fellow subjects, and no evidence is ever given to the contrary. The experimenter takes a card with printed lines and places it on an easel. One line is labeled 'S' and the others are labeled 'A', 'B', and 'C'. The task is to say which of the three lines—A, B, or C—is equal in length to line S. Any idiot can solve this problem because one line is clearly of identical length to S and the others are not even close. However, unknown to the subject, this is not really what the experiment is about, and his "fellow subjects" are in reality confederates of the experimenter, playing entirely scripted roles.

The phony subjects answer aloud, and in order, with the real subject answering last because things were arranged so that he would be seated last. There are several such trials, one after the other, each with a different card sporting a target line and three comparison lines, and each equally easy to do. The confederates behave normally at first, picking the line that to any sentient mortal is obviously of the same length as S. But then, in some trials, they mischievously and unanimously pick an incorrect line. Thus, if in one of the deliberately weird trials the correct answer is line B, the real subject will hear his colleagues answer, "A," "A," "A," "A," "A," "A," in a maddening repetition that will make our hapless protagonist wonder if this is not suddenly "Candid Camera" or, more ominously, "The Twilight Zone." Then it is the subject's turn to answer. Imagine that you are that subject: How do you feel?

> Your eyes give you a clear answer to the experimenter's simple question: 'B' is obviously right. But all of those people said "A." There is something wrong here; either you can't see, or you misunderstood the instructions, or something else is going on. Maybe *they* got the instructions wrong: maybe *they* can't see. But how likely is that? After all, they all gave the *same* wrong

> answer. And what a fool you'll be if you answer "B" and "A" *is* the right answer. They'll probably laugh at you. . . . Will you go along, or let on that you are a fool?—Sabini (1992:22–23)

It turns out that about a third of the subjects went along with the confederates in most trials where these latter were unanimously giving a wrong answer. As for the rest, one quarter remained independent in *all* such trails, and the rest conformed to the absurd majority opinion on some trials but not on others. Is this sufficient evidence of conformism? Sabini (1992:23) considers the result somewhat equivocal.

> Were subjects in general independent, or did they conform? There is no simple answer to this question; this is no fairy tale. On the one hand, most of the subjects' responses were independent (two-thirds of them); on the other hand, most subjects (three quarters) conformed at least once out of the twelve critical trials. If you expect people to conform, then you should be surprised at the rates of independence. But if you expect people to be independent (which Asch did), then you should be surprised at the degree of conformity (Friend, Rafferty, and Bramel 1990).

A scientist's surprise (or lack thereof) at the result must indeed be a function of her prior expectations. But whether the experiment yields sufficient evidence of a conformist bias is a different matter, one that depends on how much conforming behavior in the experiment will count as evidence for a systematic "taste" to make our behavior match that of the majority. This assessment must naturally be placed in the context of the task, and so the observation here is that we have *moderate* levels of conformity to an answer that subjects can clearly see is *obviously wrong*. In other words, this is very much like what happens in *The Emperor's New Clothes*, the fairytale that Sabini apparently asks his readers not to compare this to.

In that fairytale, you may recall, everybody can see that the emperor is quite naked, but since others declare that he wears the most excellent clothes, and since it has been announced that only fools are incapable of seeing the emperor's most wonderful attire, people conform and declare the naked emperor well-clad. When the right answer is obvious, but you nevertheless conform to produce the wrong answer, you have generated an *extreme behavior*—and it is precisely because Asch considered such behaviors much too extreme to be likely that he was in fact not expecting to see any conformism in his own experiment. But what follows from this? In less ambiguous contexts, where the right answer is not obvious,

the same bias that produced extreme behaviors in a moderate amount of cases for Asch will likely produce run-of-the-mill conformism in just about everybody. After all, it is precisely in ambiguous situations that we would expect humans adaptively to prefer social learning, because the usefulness of social learning increases as the difficulty of acquiring information through individual trial and error rises. *And ambiguous situations abound.* Thus, Asch's result can legitimately be considered good evidence for a general conformist bias in humans.

Jacobs and Campbell (1961) examined conformism in a task where the correct answer is highly ambiguous. They formed micro-"societies" of two, three, and four individuals in which only one or two were real subjects, the remainder being confederates. Real and fake subjects were placed together in a darkened room and shown a fixed spot of light, then asked to estimate the distance that the light had traveled. In the experiment the light did not, in reality, travel at all—it was fixed. However, it is well known that due to a consistent optical illusion, people think the light moves about four inches: it's called the *autokinetic effect* (the effect had been used before to study dyadic rather than conformist social influence [Sherif 1935]). The confederates gave their estimate first, and they had been instructed to give estimates (16 inches!) much higher than the usual estimates. Then the real subjects would give it a try.

In Jacobs and Campbell's experiment this constituted the first "generation." For the second "generation," one of the fake subjects was removed and replaced with a real one, and all participants then proceeded to make estimates again. This procedure was repeated until the micro-"society" was composed exclusively of real subjects. From then on, in each "generation," a real subject would be removed and replaced with another real subject, for a total of eleven "generations."

What did they find? When there is only one confederate (fake subject) and two real subjects, the wildly high estimate of the lonely confederate (16 inches) nevertheless has *some* influence, as in the first generation the real subjects give estimates higher than four inches, though always below nine. When there are *two* confederates—a 2/3 majority—and only one real subject, the latter is quite strongly influenced and in the first generation gives a very similar estimate, about 14 inches.[2]

This experiment also suggests that conformism is stronger with ambiguous information. The results are strictly speaking not directly comparable to the Asch experiments because in Asch's study the task was

qualitative (which line), and here it is quantitative (how many inches). And yet the differences in conformism are so sharp that certain inferences are not unreasonable. With Jacobs and Campbell we have a much weaker confederate majority than what Asch used (2/3 as opposed to Asch's 6/7), but Jacobs and Campbell nevertheless produced estimates by real subjects that were fully 88% as large as those of confederates, and they got the overwhelming majority of real subjects to conform this way, whereas Asch, by contrast, got only a minority to conform. It would seem that the key difference explaining why conformism was so dramatic in the Jacobs and Campbell experiment is indeed that the information here is quite ambiguous. If these researchers had provided subjects with a task as painfully obvious as that in Asch's experiments, their real subjects presumably would have likewise been tempted to doubt their own sanity or that of others when hearing the estimates of their fake compatriots, in turn producing estimates that presumably would have been less influenced by the majority.

The Jacobs and Campbell study provides evidence for something else: guided variation. The mean estimate of the subjects slowly decreased as the stubborn confederates of the experimenter were steadily replaced with real subjects, generation after generation, until it became a constant at around four inches. This suggests the application of a default conformist principle which is subject to revision as the subject acquires individual familiarity: conformist leap 'n' crawl.

I bring your attention again to the two analytically distinct types of conformism. In the Asch experiment, you, the subject, can easily see what the correct answer is. If you conform, it's not because the crowd influenced your idea of the actual state of affairs in the world—you know what that is, because you can see without any trouble which line is of the same length as the target. The question here is therefore not to obtain information about your environment, as you have already acquired that information by yourself. Thus, if you conform, the reason must be that you do not want your behavior to stick out as different from everybody else's. *You don't want to look like a deviant.* This desire to match your behavior to that of others means that the Asch experiments are giving us evidence for *norm* conformism. In the Jacobs and Campbell experiment, on the other hand, you are estimating something uncertain, and so conformism here is not a question of not looking like a deviant, but of trying to be accurate.

The issue of ambiguity, however, is relevant to both forms of conformism. When I am not sure what the relevant norm is, the behavior of a nearby crowd is more likely to influence me, just as I am more likely to adopt a description of the world that most others endorse when it is difficult for me to make an individually independent verification. Finally, though we may analytically and even experimentally distinguish these two forms of conformism, that does not mean we cannot experience them both simultaneously in a given situation, where we may feel both a desire not to stick out, and also a desire to improve our understanding of the state of affairs.

Since we are interested here primarily in norm conformism, because it is this that underlies ethnicity, I turn next to the phenomenon of *pluralistic ignorance*.

### *Evidence of norm conformism in ecologically realistic situations: pluralistic ignorance*

Although in most ordinary situations the dominant bias is for people to overestimate their own similarity to the mental states of others (the false-consensus effect; e.g., Marks and Miller, 1987), in certain special circumstances the opposite happens: you assume that you alone are distinct in your particular belief, capacity, or state of mind, thinking that only *you* are conforming to a widespread meme, whereas everybody else really has a private desire for the meme itself. The term 'pluralistic ignorance' was coined by Katz and Allport (1928) to describe the puzzling (and sometimes infuriating) phenomenon of widespread public conformity to social norms that have no widespread private support (Miller and McFarland 1991). In other words, we have here a psychological mechanism for the perpetuation of a *status quo* that nobody agrees with, resulting from the widespread and incorrect private assumption that everybody *does* agree with it (Krech, Crutchfield, and Ballachey 1962; Kuran 1995). This is the phenomenon colloquially termed a 'silent majority'.

Consider a familiar scenario. You are a college student at a party and everybody is drinking quite heavily. You naturally do the same, but not because you enjoy drinking heavily or because you want to on this particular day. Rather, you drink because you don't want to appear as the odd person out who refuses to join in the fun: a "party pooper." However, even though you yourself understand well your own motives for drinking as

heavily as everybody else, you assume that the heavy drinking of others is a result of their *intrinsic*—rather than socially-influenced—preference for doing so.

That's an interesting move. Why are you doing that? Consider: the only motivations you have access to are your own. And your behavior is identical to that of others around you. So wouldn't the natural thing be for you to guess that, if they behave like you do, their mental states are also like yours? But you don't: you impute different motives to them!

Now, suppose that most other drinkers reason just like you, thinking that they alone have a socially influenced motive for outputting the same behavior they see in everybody else. In that case what is taking place is called *pluralistic ignorance*: everybody is conforming to a norm that each presumes results from people's private desires, when in fact no such private desire exists. So everybody overdrinks because everybody feels a peer pressure that isn't there. . . .

As Miller and McFarland (1991) put it,

> To unlock the mystery of pluralistic ignorance we must explain why individuals—who realize that their behavior is a façade and an inaccurate reflection of their real feelings—do not assume that this is probably true of others as well.

Moreover, as they point out, false uniqueness effects are not hard to understand when they paint the self in a favorable light (e.g., more intelligent, more "hip". . . . ). The puzzle is to understand why individuals would infer in the behavior of others a causal impetus different from their own when they have to pay the price of feeling deviant as a result.

(Note that pluralistic ignorance does not apply to situations where people miscalculate the majority opinion because of exposure to a small and therefore biased sample. Pluralistic ignorance results when you draw opposite conclusions about the internal states of self and others despite the fact that others are in fact reliably observed to behave similarly to you.)

Using our drinking example above, if pluralistic ignorance is really at work in this type of situation, then we predict that after convincing all members of the party that most others do not really wish to drink that much, the level of drinking should go down. This is not just a thought experiment. Some years ago,

> . . . students at Northern Illinois University were asked to guess how many of their peers drink heavily, the results were surprising. Officials found those

> surveyed guessed high—far too high. Those overestimates about binging have led to a new weapon in the battle to curb college drinking: Telling kids the truth. Heavy drinking dropped on the De Kalb, Ill., campus after administrators went public with the survey results, which showed that not nearly as many people favored "binge" drinking (having five or more drinks while "partying") as students thought.—Clayton (1997)

The social contexts that can lead to pluralistic ignorance are varied; but what Miller and McFarland (1991) have found in a review of the literature is that these all seem to have in common the fear of appearing deviant, or, more precisely, an aversion to the embarrassment experienced when others observe one's deviance. It is worth looking at the experiments that inform their intuitions.

It has been found that students in class will interpret the behavior of others—namely, not asking questions for clarification—as indication that *others* understand even when they themselves still feel confused, and even though they themselves are not raising their hands either. This can hardly be a rationalization to boost self-esteem, since it makes the individual lower her appraisal of her own comprehension abilities. So why behave in this fashion? Since pluralistic ignorance is observed across a wide variety of situations, all of which have in common that they include the danger of embarrassment, Miller and McFarland (1991) hypothesized that it is fear of embarrassment which leads people to the kinds of inferences characteristic of pluralistic ignorance. Thus, anxiety over one's own possible deviance has the effect of restricting one's behavior to what everybody else is doing so as to avoid the risk of being exposed and thereby embarrassed. They tested this hypothesis and found it supported.

Subjects were given a purposefully obtuse passage concerning theories about the self-concept (whatever that is), and told that this would prepare them for a group discussion examining people's folk theories of "the self." The passage in question would be virtually impossible to understand for people unfamiliar with this area of theory (and those of us who read social-science journals know that such passages are not hard to find). In the experimental condition, subjects were all placed in the same room, and they were instructed to consult with the experimenter—who would be close by in an adjacent room—but only if they had "any really serious problems in understanding the passage." The qualification "really serious" was meant to ensure that requesting help would be embarrassing. There were two control conditions. In the first, no social comparison was possible

because subjects were alone, and in the second, subjects were together in the room but the experimenter told them no questions could be asked.

In a post-manipulation questionnaire, it was found that only those subjects who participated in the condition with an audience and with the possibility of embarrassment (experimental condition) rated themselves as understanding the passage less well than their peers. And yet, they really had no information about the comprehension of others unless it was the fact that those others had not requested help *either*. In the other two conditions, by contrast, subjects reasoned that they had understood the passage as well as everybody else. This demonstrates that it is the immediate fear of embarrassment that produces the inferences labeled 'pluralistic ignorance'.

I find this convincing. However, psychological explanations ideally should not stop at explaining one mental phenomenon in terms of another. Ultimately, we want a functional account. Not only because a functional account is more satisfying, but also because it gives us a better sense of which proximate psychological mechanisms are features of adaptive design and which are side effects. In the experimental condition, the prose that subjects were asked to read is extremely difficult to follow, and from this fact alone the subject could reason that others were having trouble reading it too. But since this eminently reasonable hypothesis does not occur to the experimental subjects, I suspect that they reason *backwards* from their embarrassment. That is, they consider asking a question, but they anticipate embarrassment if they do. Now they feel like answering the following: Why do I feel embarrassed? And they provide themselves with an answer: I don't want to look stupid. If this interpretation is reasonable, the evaluation of others as understanding better—is a *post-facto rationalization*, there merely to explain the embarrassment.

But if the embarrassment is prior, then we must pay attention to what produces the embarrassment in the first place. My guess is that the main psychological process here is *apprehension about attracting public attention to oneself.*

In other words, merely "sticking out" (engaging in behavior that is noticeably at variance with that of others, who then notice it) is uncomfortable and embarrassing. This embarrassment will increase, of course, if the scrutiny of others is perceived to carry a negative evaluation; but sticking out is enough to produce it, and the negative evaluation of others

may then be imagined—even if there is no evidence of it—merely to make the embarrassment seem rational. Thus, subjects feel agony at the thought of becoming highly noticeable by getting up to ask a question in a quiet room if there is little or no precedent for this (i.e., nobody, or just one or two people, have done it so far). It seems silly to say that you are afraid merely to stand up and have others notice that you have done so all by yourself, so you tell yourself that what really bothers you is that everybody thinks you are stupid. Of course, this is incredibly egocentric: others in the room usually do not care a whit about you, as they are also worrying about their own selves (it is amazing how often this thought does not occur to us).

But if this hypothesis—that merely sticking out is enough to produce embarrassment—is on the right track, then noticeably idiosyncratic behavior that attracts public attention should be embarrassing even when the above rationalizations about the mental states of others (and which get dubbed "pluralistic ignorance") have no place. Thus, notice that you will probably feel embarrassed when you are asked to go up to the podium to receive an award, or an applause. And the larger the crowd, the bigger your discomfort. (You will also feel pleasure, of course; the discomfort comes from being in the spotlight, and the pleasure from the fact that you are being positively regarded.) To support such introspective exercises there is also some experimental evidence. For example, Hashimoto and Shimizu (1988) found that in cultures as different as the Iranian and the Japanese, merely being stared at was one of the causes that led to embarrassment in both.

The literature on "pluralistic ignorance" has therefore probably missed the most important psychological process to be explained by focusing on rationalizations that are specific to particular contexts. Nevertheless, I believe this literature is correct when it emphasizes that we are looking at a mechanism for norm conformism. Fear of the spotlight appears to be precisely this: a crude mechanism to make us match our behaviors to those of others. If you do what everybody else is doing, you will not attract attention, so if you fear attracting attention, you will do as others do. If doing as others do is adaptive, as we argued earlier, then a fear of sticking out will be adaptive, because it will cause us to conform.

And there may be profound effects here. When doing what others do involves expressing an opinion in public (say, a political belief), then the

more you say it the more you may come to believe it, even if you initially expressed yourself merely to avoid sticking out and in contradiction to your initial private beliefs.

> Pluralistic ignorance can facilitate social change by inducing private acceptance of a mythical position and not simply by inducing public conformity to it. One implication of this fact is that much pluralistic ignorance may go undetected because it so quickly leads to revisions in private attitudes. . . . Pluralistic ignorance, thus, may shorten the road between [merely overt] compliance and internalization (Kelman, 1958).—Miller and McFarland (1991)

But even if it turns out that the effect of public conformity on private belief is weak, the trans-generational effect is bound to be strong, because members of the next generation can only see private behaviors when they are expressed, and if what is being expressed is a falsified preference, they may acquire such preferences as their own private beliefs. Thus, the transition from publicly falsified preferences to internal beliefs may happen relatively easily from one generation to the next, even under the extreme assumption that it has zero effects within generations.

We should not jump to the conclusion that such things are always undesirable, as they can cut both ways. For example, if you like tolerance, but hate hypocrisy, how will you pass judgment on "political correctness"? Political correctness is morally inconsistent, because it is itself a form of intolerance, and the pressure it exerts causes a lot of people with prejudiced ideas to falsify their preferences, generating hypocrisy. But the next generation, as a result, gets to see fewer expressions of public prejudice and grows up with a lower threshold of tolerance for bigotry. This in particular can be celebrated even by those of us who find that "political correctness" is too extreme and too sweeping, and therefore in some ways genuinely costly. Of course, there is an argument that too much political correctness may create a backlash, so that it is possible, depending on which model truly describes the dynamics here, that political correctness, for having gone too far, will have a net adverse effect on tolerance in the long run. The point I want to make is merely that passing judgment on silent majorities as "good" or "bad" for us cannot be made in the abstract, but must be done on a case-by-case basis, given that some may be bad and others not, or they may be good in some ways but not in others.

Economist Timur Kuran (1995) has produced a very useful examination that merges economics, politics, and psychology, and which explores in depth the consequences for mass political action of pluralistic ignorance and related processes. My own quarry here is with a related topic: the aversion to sticking out as powerful evidence that humans have been subjected to the selection pressures set loose by the imperative of norm-coordination. It is these selection pressures, I will argue, that gave rise to the phenomenon of ethnicity.

### *Discrimination, membership criteria, and the maintenance of ethnic boundaries*

'Discrimination' is a loaded term, but I will treat it dispassionately here for reasons that by now will be familiar. The term has two meanings. One is "the showing of partiality or prejudice in treatment"; another is "the act of distinguishing differences and honoring preferences." The latter meaning clearly does not connote disapproval, much less hatred. If I sort buttons of different colors into piles I will be discriminating among them, but my opinions of the buttons will be quite dispassionate, I assure you. In similar fashion, one can be perfectly cosmopolitan and tolerant about members of another ethnie and still prefer coethnics because doing business with them is, after all, so much easier. Parents can insist that their children marry within the ethnie merely because they realize that marriage is already difficult enough without adding the problem of mismatched norms. And perhaps an even better reason is that marrying off one's children, in every simple society, is the beginning of a long-term alliance with the in-laws, and alliance partners are best selected with ease of coordination in mind. This would all be "to discriminate" in the sense of acting on one's preferences, but none of it requires passing negative judgment on ethnic others.

That said, I rush to add that the two meanings here considered—to distinguish and prefer, on the one hand, and to express prejudice, on the other—are certainly not denoted with the same word, 'discrimination', *by accident*; and I will argue further below that prejudice has quite a lot to do with our sensitivity to the costs of norm differences. For the moment, however, let's consider the effect that discrimination—purely in terms of

choosing certain interaction and marriage partners over others—will have on the flow of memes.

The transmission vehicle that most memes have employed, for most of history, to jump from one place to another have been human heads. For this reason, even benign discrimination will cause fewer exchanges of heads between societies, and this will restrict the meme-flow between them. Given that parents famously or infamously play the part of an "ethnic endogamy police," they contribute to isolating one ethnie from cultural developments in another, which in turn reinforces a trend towards separate cultural/evolutionary histories. This applies even when the ethnies in question live in the same environment and side by side.

But interactional costs and mechanisms to avoid them do not only act to keep the "meme pools" of different ethnies relatively isolated and diverging from each other—they also generate, within the ethnic boundary, opposite forces for *ethnic homogeneity*. It is against your interests, after all, to deviate from the common memes in your local population. You need your local relevant others to improve your chances for survival and reproduction, and having more potential partners with whom you can interact smoothly—because you are well coordinated with them—requires that you adopt the norms of the local majority. This is the argument for why natural selection favored a conformist psychology. Beyond this evolved tendency, experimenting with alternative behaviors will often present an individual with rewards and punishments that will further reinforce a tendency to conform.

But all of this does not quite explain why norm differences should remain stable over time. One obvious way to explain the stability of norm differences between neighboring ethnies is by arguing that the relationship between physical proximity and frequency of contact is not straightforward, but instead interacts with social identity. The larger the norm differences between any two communities, the less their respective members will be likely to interact, even when they live at the physical boundary; the less they interact, the larger the norm differences become—mutual circular causation. This is bound to work as a partial answer, but I don't think it is the whole story. In addition, it appears that our psychology calculates the relative frequency of a meme by putting for the most part only *coethnics* in the sample of relevant observations, treating interactions with ethnic "others" as irrelevant for this purpose.

The result of both effects is a discontinuous boundary, such that relative proximity to ethnic others at the edges seems not to produce any smoothing of differences in many domains governed by interactional norms (see Gil-White 2005a *under review*). In my own fieldsite in Western Mongolia, for example, it is quite noticeable how differently the two local ethnies—Torguuds and Kazakhs—behave, despite the fact that they live side by side and are often interdigitated (*ibid.*).

The second effect in particular—where social identity marks off coethnics as relevant and ethnic others as irrelevant to the conformist psychology—has an interesting consequence. As legendary anthropologist Frederick Barth (1969) originally observed, in a paper that has deservedly become a classic, the ethnic boundary is as much the cause of norm differences as the norm differences are causes of ethnic boundaries. Awareness of differences can produce discrimination in our choice of interaction partners, but so can the development of different social "identities" make us include those whose identity matches our own as relevant and others as irrelevant, and this parsing, coupled with conformism to plural memes within the sample of relevant actors, will then facilitate the emergence of stable cultural differences. The point: it is not always necessarily the case that a norm boundary emerges first and social identities are then produced to label the differences—the social identities can come first.

Precisely because perceived normative differences and contrasting social identities restrict meme-flow across the social boundary, and because the imperative to find well-matched mates is strong, ethnies tend to reproduce *vertically*. That is to say, the cultural traditions are typically being transmitted to biological descendants. This gives the sets of memes associated with an ethnie a certain amount of inter-generational stability or "gravity," and for this reason Barth spoke of ethnies as the cultural analogue of species. Just as species have relatively stable boundaries that are vertically preserved over time, so do ethnies.

Anthropologists have paid less attention to these arguments in Barth's classic paper than to others, which has led him to complain about it:

> . . . the issue of cultural content versus boundary, as it was formulated [by Barth 1969], unintentionally served to mislead. Yes, it is a question of analyzing boundary processes. . . . But locating the bases of such boundary processes is not a question of pacing the limits of a group and observing its

> markers and the shedding of members . . . central and culturally valued institutions and activities in an ethnic group may be deeply involved in its boundary maintenance by setting internal processes of convergence into motion; and we need to pay special attention to the factors governing "individuals' commitments to the kind of personhood implied by specific ethnic identities" (Haaland 1991:158).—Barth (1994:17–18)

Barth is bemoaning the fact that fans of his 1969 paper should have come away with the idea that to understand ethnic boundaries one merely needs to keep track of who joins and who leaves—i.e., "pacing the limits of a group, and observing its markers and the shedding of members."

But if Barth does not endorse this approach, why did his readers make him out as having done so? No doubt because Barth, for starters, derived his insights about ethnic boundaries from observing how members of one ethnie in Pakistan decided to leave it and attach themselves to another. Barth attributed this to the rational economic calculations of the participants and built a general model of ethnic processes on the idea that these are voluntary associations of *interest*—sort of like "clubs."

When anthropologists and many others saw this argument, they fastened onto it like a starved dog to a bone. If it was true that people rationally and freely choose their ethnic membership, they reasoned, then ethnies cannot be biological units with unalterable essences, and this pulls the rug from under the feet of racists, who always argue for biological interpretations of cultural differences. Here the worthy goal of combating racism has had lamentable consequences because, as is often the case in social science, empirical rigor has deferred to ideology. Ethnies are certainly not biological units—cultural anthropologists are right about that. But they are right entirely by coincidence, because they are wrong about the reason, as people do *not* get to choose their ethnic identities (something that is rather obvious).

The reason ethnies are not biological units, but merely cultural ones, are three: (1) some intermarriage almost always occurs, (2) intermarriage is certainly not the only way to get genes across an ethnic boundary (just use your non-Victorian imagination), and (3) as population geneticists long ago showed, quite modest amounts of gene-flow between neighboring populations will make them *panmictic*—that is, a single population from the genetic point of view—so all you need is a little bit of intermar-

riage and inter-ethnic infidelity and the biological unity of any ethnic population will be thoroughly undermined. Since you always have that, ethnies cannot be biologically meaningful units.

My fellow cultural anthropologists—Alas!—have, by and large, not made their case this way. Rather, they seized on Barth's argument about supposed "ethnic switching" and in so doing they painted themselves into a rhetorical corner. Now they think that if one should oppose Barth's ethnic-switching argument, and argue instead that people *cannot* choose their ethnic membership, one must be arguing for ethnies as biological "races." But that hardly follows.

The reason most cultural anthropologists have trouble seeing this is that they have made themselves victims of the following absurd mapping: that "identity choice"/"no identity choice" directly maps to "cultural"/"biological." But that is nonsense. Just go ahead and try to marry your mother; society will not hear of it, and therefore the marriage license will not be issued (not unless you lie, successfully, about your biological ancestry). So you cannot choose the social identity "mother's husband," but not because it is biologically impossible. Similarly, if your parents are ethnically X, you can shout all you want that you are ethnically Y, but nobody will recognize it (not unless you lie, successfully, about ancestry). The reason is not that there is really something in the blood of an X that makes him X but that *people think there is* (Gil-White 1999, 2001a). A social identity cannot be "worn" unless others let you:

> . . . individuals may be able to make just one, or more than one claim [to membership], and find groups more or less willing to recognize their claim or claims. This constraint is sometimes forgotten. The individual can make any claim he or she wants to, but, to have any effect, a claim must be recognized.—Heather 1996.

Many cultural anthropologists, as if to make sure that reason will not succeed, add that ethnies are not *real* because they are "constructed." Why this argument? This is also a consequence of the laudable anthropological desire to combat racism. But once again the strategy relies on a perfectly absurd mapping: 'natural'/'cultural' is mapped to 'real'/'constructed'. So, in order to deny social categories a biological reality, anthropologists declare that they have *no* reality, as they are supposedly "constructed" fictions with no substance whatever. It should be obvious, however, that what is

constructed is not thereby rendered fictional. (Imagine telling an engineer that her bridge is not real because it had to be constructed.)

In terms of achieving the intended result of combating racism, these strategies of cultural anthropologists have been spectacularly bad because laypeople are hardly fooled. They can immediately recognize that denying the reality of social categories is absurd, and the transparency of such nonsense has made them suspicious that the academic effort to declare social categories fictional is a form of political correctness (which it certainly is). The result is that laypeople end up convinced that their own *completely erroneous* intuitions about the biological substance of certain social categories must then be correct, or else why would so many politically correct academics deny it with strident arguments, the absurdity of which follows by inspection? Since this is the opposite of what anthropologists wanted, it follows that they have shot themselves in the foot.

The only way to set things right is, of course, to talk straight. The set of things that are "real" is not coextensive with the set of things that are in some narrow sense "biological," and therefore to say that ethnies are drenched with substance in no way requires that this substance be *racial.* This is the point that was lost when the frenzy over Barth's ethnic-switching claims drowned out his reflections on the normative substance of ethnic phenomena. As we saw above, Barth argued that

> central and culturally valued institutions and activities . . . [are] deeply involved in [ethnic] boundary maintenance by setting internal processes of convergence into motion . . . [as are] . . . the factors governing individuals' commitments to the kind of personhood implied by specific ethnic identities.

Barth's whole argument had been that people switched from one ethnie to another because they were seeking a new "kind of personhood"—a new set of "oughts" and "ought nots," and a new network of relationships—more to their economic advantage. This is what everybody missed, and yet Barth was very clear on this point: "if they say they are A . . . they are willing to be treated and let their own behavior be interpreted and judged as A's and not as B's" (1969:15). The whole point of being an ethnic A versus a B—what this *meant* for Barth, in other words—was that one voluntarily *conformed* either to A or to B "rules of the game," as he called them. There is hardly anything fictional or superficial about that. On the contrary, such matters are central to almost everything we do in our

everyday lives. It turns out that Barth was wrong that ethnic *identity* could be *freely chosen in the first generation*; but his argument that the underlying substance of ethnicity is tied to the pressures to conform to locally dominant memes gets it exactly right.

A final point about ethnic boundaries.

When I correct Barth on ethnic switching I do not mean to say that there is no voluntary movement of personnel between ethnies; there *is*. Barth's fieldsite was a bit peculiar in this way because, more than in other places, people could make choices about which ethnie to associate and reside with, and this naturally made a great impression on Barth. However, my claim is that people cannot make recognized switches of ethnic *identity* in the first generation, and in fact none of the individuals whom Barth collected data for had done this (Gil-White 1999).

Over the long stretch of historical time, ethnies are always forming and reforming, accepting lineages and shedding them. But that's the key word: ethnies shed *lineages*—not "members." In the first generation, if a man who was born of German parents says, "I am now a Swede," nobody will recognize the claim, neither Germans, nor Swedes, nor "bystanders." And this is precisely why he would thus remain a German and not become a Swede: having a social label is a matter of getting it recognized by those around us. Nothing more, but also nothing less. For example, before the age of birth certificates, my name cannot be 'Francisco' unless people agree to call me that (and the meaning of a birth certificate is itself entirely dependent on a social agreement anyway—like a marriage license).

It is crucial not to make here the common and deplorable confusion between citizenship and nationality (see Connor 1972; Gil-White 2005c *in press*). If our German migrates to Sweden and gets a Swedish passport, he would become a citizen of Sweden, but not an ethnic Swede. Notice, however, the difference: if he in addition married a Swede, then his children certainly *would* be considered ethnically Swedish (or at least half-Swedish). A few generations down the line, one German ancestor, even if at all remembered, will make no difference: his descendants will be counted as totally Swedish, just like everybody else around them. In this manner, the Swedish ethnie gains a lineage through this immigrant, because the man's descendants are all Swedes. However, *he*—the immigrant—never becomes ethnically Swedish (he is always "the German ancestor"). So, although migration allows ethnies to gain and

shed lineages, the same is not true for *members*. (The example I provide works even though the two ethnies chosen are considered "related"; I could have chosen "easier" examples, such as a German moving to Ireland).

This is hardly an exclusively European phenomenon, by the way. One of the first things I did when I became interested in ethnicity was to look at the examples from various parts of the world where ethnographers had claimed to have observed people choosing to switch their ethnic identities. What I found, without any exceptions, was that such claims were contradicted by the data of the very ethnographers making them, and this included Frederick Barth and his mentor Edmund Leach. They did have examples of ethnies absorbing foreign *lineages*, but nobody had a single example of people making recognized switches of identity in the first generation (see Gil-White 1999).[3] My attempts to confirm that this followed from people having a folk belief that ethnic identities are transmitted biologically were quite successful (Gil-White 2001a, 2001b, 2002).

Now, when I explain the above findings to laypeople, it never needs any belaboring because for them it is trivially obvious that the necessary and sufficient condition for wearing an ethnic label is biological descent. What needs belaboring, to them, is why it was ever necessary for me to argue my case. The explanation I am always forced to give (and to repeat here once again) is that—against all reason and empirical evidence—many cultural anthropologists, and certainly those who boast that they are "postmodernists," have passionately argued that ethnies are voluntary associations and, to boot, fictional categories with zero substance, incorrectly believing that their false claims were indispensable to fighting racism. This anthropological error (but let us spread the blame also to other social scientists and philosophers who have cheered it) is at once scientific and ethical—that is, a form of malpractice.

I shall explain exactly what I mean.

It just so happens that *there are no human races*. The human species is spectacularly uniform compared to, for example, common chimpanzees (where biologists *can*, by contrast to the case of humans, identify three different races; Boyd & Silk 2003, 390–91). The reason is simple. As noted above, very little gene flow between populations will make them *panmicitic* (i.e., one population, from the genetic point of view). The amount of gene flow between human populations is not a little; it is a lot. This is

confirmed by the worldwide assay of human genetic variation carried out by Luca Cavalli-Sforza and his colleagues, which demonstrates that the common illusion that certain social categories are biological races is just that—an illusion (Cavalli-Sforza, Menozzi, *et al.* 1994); see also Brown & Armelagos 2001; Boyd & Silk 2003: 456–64). In fact, it turns out that our eyes often get the relative genetic similarity of different populations entirely wrong. For example, our eyes will effortlessly group Australians and New Guinea Islanders with Africans, but in fact they are closer to the Chinese. Our eyes group North Asians with South Asians, but the genetic data shows North Asians closer to Europeans. Mind you, these examples cannot legitimately be used to promote the inference that the real "yellow" race includes Australians and that the real "white" race includes North Asians, because no cut of the human species qualifies as a "race" the way biologists use this term. We are just too uniform.

Now, if categories that humans create—based on what they incorrectly think are discontinuously varying and mutually correlated differences in skin color and the like—are *not* biologically meaningful, then neither can neighboring ethnies be such, as here people on either side of an ethnic boundary are often not even physically distinguishable, and the average genetic differences between them are in fact vanishingly small. This is not to deny that certain individual genes can be more common in some ethnic populations than in others. What it denies, rather, is that the aggregate genetic differences between ethnies raise their biological meaningfulness anywhere near the status of a "race" as this term is applied by biologists in the case of other species.

It is these genetic data that are relevant to the question of whether human populations are biologically meaningful. By contrast, whether the social grammar of ethnicity allows people to declare their choice of ethnic membership was never the relevant question, and hence answering it one way or the other was always powerless to make a statement about whether or not ethnies were "races."

So notice what cultural anthropologists have done. They have set up a debate that could not answer the question, and where the only way they could win their ideological point was to assert something that any sentient mortal can see is absurd by inspection—that ethnies are supposedly voluntary associations. Thus, cultural anthropologists have actually been

assisting *the racists* by presenting an opposition so laughably weak that to be associated with it is embarrassing. Even worse: many cultural anthropologists have vilified the profession of biology, the very profession that has given us the data that demonstrates that there are no human races.

Why the vilification of biologists? The obvious reason is that pseudo-scientific racists—eugenicists masquerading as biologists, or as "intelligence-testing" psychologists who talk nonsense about heredity—have put forward scientific frauds in order to promote the idea that humans can supposedly be divided into biological races, and that this supposedly matters (Gil-White 2004). But the way to combat a scientific fraud is not to produce a fresh one. Perhaps my fellow cultural anthropologists should take the Hippocratic Oath to "First, do no harm." If they mean to oppose racism, they should not make absurd arguments that will make the racists look good. If they don't have better arguments, then they should do no harm, and they should let somebody else fight racism—at the very least they should not vilify the people who bring the demonstration that truly undermines the racists, because this makes the job of explaining to ordinary people that there are no human races more difficult than it needs to be.

All that said, one may nevertheless notice that the view of ethnicity which I am defending is not without its discomforts. If ordinary people really do believe that ethnic identities require biological descent, aren't we then matter-of-fact racists? That depends on how we define 'racist'. My dictionary informs me that such a person does not merely divide humans into putatively biological categories, but *also* considers these categories to be of greater or lesser moral worth. So if people make biological descent the criterion of membership in their ethnies, this does not yet make them racists unless they, in addition, think that some ethnies are morally superior to others.

Now, of course, one can argue that such rules of membership nevertheless *do* make people susceptible to racist ideologies; in other words, to think this way makes us good "raw material" for racist appeals. This argument has force, and if anthropologists really mean to oppose racism they would do well to consider it seriously. After all, we cannot effectively oppose what we do not understand; and so we should not—merely because racism offends us—avert our gaze from the possibility that human-evolved psychology makes racism likely. Or, rather, *precisely*

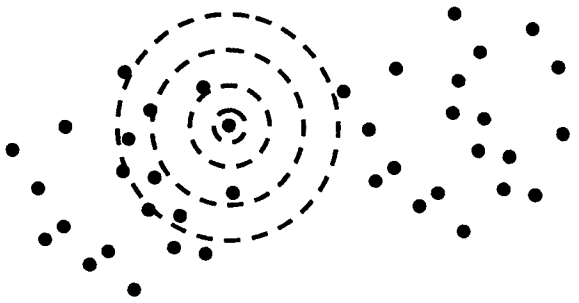
because racism offends us, we should seek to grasp the inner workings of this psychology in order the better to transcend it.

*Conformism, and the evolution of ethnicity*

The above discussion concerning the stability of ethnic boundaries does not explain why the boundaries first arose, nor why people should have an intuition that membership in an ethnie requires biological descent. I have treated the question of why our ethnic psychology is primordialist and essentialist elsewhere (Gil-White 2001a, 2001b, 2002). My topic here is conformism, so I shall focus below on the role that I believe norm-conformism played in producing certain emergent phenomena at the meta-populational level, which phenomena we now call 'ethnicity'.

It may appear at first that if humans are conformists we should be predicting perennial stasis, but this follows only if we assume that there are no social learning biases that can override conformism under certain conditions. Such biases, however, *do* exist. Henrich and Gil-White (2001), developing certain ideas in Boyd & Richerson (1985: ch. 8), have argued that the most important of these is *prestige-bias*. Without belaboring it here, this is the preference for individuals who appear to be more successful than average in valued domains of behavior. When such individuals innovate new memes, we will tend to copy them despite the fact that, by definition, such memes will initially be rare. In the simplest model, we may imagine local populations as having considerable conservative gravity, sticking through conformism to whatever memes enjoy a plurality in the previous generation, but getting nudged every now and then to new equilibria by prestigious individuals. This simple model allows us to give an equally simple and broad outline of the processes that led to the emergence of ethnicity.

Ancestral hominids could not have been distributed uniformly across the physical landscape. The nucleation of resources and the presence of geographical barriers would have caused uneven rather than smooth dispersal. In addition, kinship and reciprocal ties, and the need to maintain them, would have favored nucleated units. The assumption of a clumpy pattern of human residence (villages, hunting bands, etc.) is therefore a safe one, and it has an important implication: at the dawn of conformist transmission, even those living at the edge of a residential clump are mostly sampling memes from *within* that clump, as shown in the following figure.



Put another way, even should I live at the edge of one of these ancestral residential communities, the clumping will ensure that I am still closer physically to members of my local group than to those of any other. Thus, my conformism will not be much different form that of other members of my residential group who live closer to its physical center: we are all mostly sampling from the same set of people.

The consequence is that different communities will end up stabilizing different sets of memes. Why? Because even though mutations in cultural transmission are not always random (because people often *do* sit down to plan and solve specific problems) they nevertheless tend to be idiosyncratic (because there are quite often many different imaginable solutions to the same problem). If cultural mutations (new memes) that can swim against the conservative force of conformism will typically be those which emerge first in the minds of prestigious individuals, then it matters that individuals have unique and unrepeatable lives, the vagaries of which make certain specific mutations more probable than others. In each local group, then, as the locally prestigious people, with their unique life histories, introduce certain idiosyncratic memes (at least in *some* domains), each group ends up stabilizing memes that are particular to *it*.

If you doubt this it will suffice to reflect briefly on your own family. Every family develops nuances of interaction that are normative and also particular to it, and yours is no exception (e.g., can the children pass gas and curse in front of the parents?; how sacred is the post-prandial conversation?; are there certain jokes whose grammar cannot be understood outside of the family?; etc.). If this sort of variation can emerge for units as small as a single family, it can certainly emerge for units such as villages or bands.

The fact of emergent cultural differences between local, residential groups will then set in motion processes at the next level of complexity—that is to say, at the level of units made up of several residential groups, each acting in contrast to other such units. However, in order to consider these higher-level population processes, we first need a handle on what is plausibly the initial engine for linking several residential groups into a higher form of organization: the need to avoid *inbreeding depression*. The imperative to avoid this is not unique to humans, but flows rather from the properties of Mendelian genetics. In order to appreciate it properly, I will very briefly review what a mutant recessive gene is, and why getting them in pairs is not a good thing.

In a *diploid* species such as ours, every genetic locus contains two genes, and the resulting phenotypic effect that any particular locus is responsible for will be some combination of the action of both. In the case of so-called *co-dominance*, for instance, each gene at a locus will contribute 50% of the phenotypic result. A simple example would be a locus coding for, say, surface coloration in a moth, where one of the two genes coded for "black" pigment whereas the other coded for 'white')—here co-dominance will result in a gray colored body. But other things are possible. Instead of co-dominance, imagine that one of the two genes (say, the one coding for 'black' pigment) contributes 100% of the effect on the phenotype, and the other one (coding for 'white') zero. In that case the first gene is called *dominant* and the second *recessive*, and the resulting surface coloration is black. Recessive genes, therefore, do not "express themselves" unless you get them in pairs—in the example, unless both the genes at the pigment locus are copies of the recessive allele coding for "white," in which single case the surface coloration *will* be white. A well-designed organism will have evolved mating strategies that lower the likelihood of this sort of thing happening in its offspring because recessive genes result from mutations, and most mutations have adaptively negative effects. The reason why is easy to understand: a genetic mutation occurs when the DNA molecule makes a mistake while trying to replicate itself precisely; although mistakes can occasionally produce improvements, there are more ways of spoiling the adaptive functionality of a complex machine (such as a biological organism, or your car) than of improving it when making random—i.e., accidental—changes to it.

Now, it turns out that preferring non-relatives as mating partners is an excellent way of lowering the chances that your offspring will have paired recessives at a locus. Or put another way, mating with your close relatives dramatically *increases* the chances that your offspring will have paired recessives, in turn making "incest avoidance" adaptive. To see why, imagine that a father has a bad recessive at a given locus, in which case there is a relatively good chance that his son and his daughter both get it. Should these siblings mate, there is a relatively good chance that they will have at least some children who inherit both recessives at that locus, and hence turn out defective. If, instead of mating with each other, they should mate with unrelated individuals, the chances of getting two recessives at that locus are much smaller. For this reason, in species after species, natural selection has favored mechanisms to avoid breeding with close relatives.

In humans, the most important incest-avoidance mechanism is known as the Westermarck effect, named after its first theorist (Boyd and Silk 2003:501–03). This is how it works: if you were in close and intimate proximity to someone while growing up, then you'd rather not have sex with this person. Of course, the mechanism is so crude that it will naturally produce sexual aversion to non-relatives so long as they are more or less like siblings in terms of how often and how intimately you interacted with them while growing up. But despite its crudeness, the mechanism works just fine on average.

The Westermarck effect is not experienced by the individual human as something having the force of a rational argument, but rather as a more-or-less automatic aversion. Of course, on top of this aversion arguments *can* be built, and society after society has provided its own, because cultural transmission can often radically embellish and flavor even those domains where humans come equipped with innate preferences. For this reason, incest taboos from place to place (beyond the normative avoidance of nuclear family members as mates) are often quite different—so much so, in fact, that describing them and theorizing about them has been one of the staples of anthropological work ever since the field got going. At the dawn of culture, however, before all such embellishments, the simplest inbreeding avoidance mechanism was likely just to exchange wives between neighboring residential communities, given that the first level of organization beyond the family would have been the local resi-

dential group, and given that inbreeding within small residential groups will quickly turn them into large families with the result that maladapted individuals with two recessives at various loci become relatively common—this result being known as "inbreeding depression." Thus, it is a safe assumption that wife-exchange between local residential groups was the most relied-upon cultural mechanism of incest avoidance at the dawn of culture.

Now, imagine that you are an individual in one of these ancestral communities, and you are looking for a mate for one of your children. This is a big deal, because the common interest in the grandchildren is the glue that will bind your family to that of your in-laws, generating goodwill for all sorts of exchanges and mutual aid. In other words, when you marry your children off you are buying an insurance policy, and this is of vast importance to those living in a subsistence-level economy because luck is a fickle mistress, and so individual families sooner or later must fall on hard times.

It has always been obvious to ethnographers—coming as they do from societies with welfare institutions and sophisticated mechanisms of market insurance—that people in subsistence-level economies think about kinship ties with a completely different level of intensity. For example, in my own fieldsite, working among the nomadic Torguud, whenever one person was introducing me to someone else and there was some link of kinship through descent or marriage, however remote, great care was taken to explain this to me, and to trace the exact lineage path or marriage connections, as well as to make sure they could figure out the proper kinship term (not always easy) for the relationship in question. It always seemed to me that this was not so much to inform me as it was an opportunity for them to remind each other of their common bonds, which speaks to the centrality of such ties. Extrapolating this feature of modern subsistence economies to human societies at the dawn of culture is reasonable because our ancestors indeed had subsistence economies, and it is the dangers of living in such an economy that makes kinship ties—for the insurance they provide—so important.

What this means is that when picking a mate for your children, under these conditions, you will care deeply about the likelihood that you will get along with the family to whom your grandchildren will join you.

Naturally, quite important here is the question of those interactional memes that potential in-laws adhere to. Since conformism will act to stabilize different memes in different residential groups, parents in our imagined ancestral environment will have a strong incentive to pay attention to such differences, and to go shopping for sons- or daughters-in-law in residential groups whose interactional norms are not too far away from their own. Such individual choices will then tend to reinforce the pre-existing cultural similarity between residential communities that led to the initial marriage choices, for by sending human heads to and fro preferentially between these communities, the groups in question will be seeding each other with their respective memes. As a result, the residential groups so involved will become more similar to each other culturally, which in turn will strengthen even further the pattern of preferential wife-exchange in self-reinforcing circular causation. What emerges over time, then, is a meta-community of residential groups that is relatively uniform in its norms, and with a developed tradition for exchanging wives with each other, until this tradition becomes a *rule*. In this way, the *norm boundary* eventually comes to gel with the *endogamy boundary*—in other words, the boundary where norms change rather sharply will also be the boundary beyond which people for the most part do not exchange wives.

As the normative and endogamy boundaries became one, sooner or later people discovered the usefulness of *naming* such boundaries in order handily to distinguish in speech those who are "us" (i.e., those into whom I may marry, because they share my norms) from "others." It is also useful to broadcast which labeled category I belong to (for example, with a distinctive hat, dress, scarification, body paint, etc.). This way, costly interactions that would otherwise result from alien "others" mistakenly assuming I am one of theirs—costly because problems of coordination would result in inadvertent offense, time wasted, and generally suboptimal interactions—can now be avoided before they start (see McElreath, Boyd, and Richerson 2003). Such markings also act as a reminder for those who occasionally must engage with people beyond their norm boundary, minimizing the probability of careless offense and its attendant costs. As a result of all these processes, sharp discontinuities in interactional memes came to coincide with carefully *marked and named* boundaries beyond which people tended not to outmarry. And all of this together is what we now call 'ethnicity'.

*Is ethnocentrism adaptive?*

The interactional preference for those in our own ethnie, especially when that interaction is marriage, as per the above analysis, is obviously adaptive. But this is not yet ethnocentrism. It may seem at first that no invidious judgments about other people's norms need, in principle, result from such rational and self-interested preferences. And yet they do.

When I asked Torguud herders whether they would allow their children to marry a Kazakh (members of a neighboring ethnie), the most common answer was "*Kazakh bolokhgüi*" ("Kazakhs not allowed"). When I inquired as to why, I always got the same answer: "*Buruu nomtoi*" ("Wrong book"). In this expression, 'book' is a euphemism for 'grammar'—as in 'cultural grammar'. It really speaks volumes, doesn't it? One does not marry Kazakhs *because* they have the "wrong" norms. The endogamy boundary and the norm boundary are one. Another way of putting this is that *the boundary most relevant to the conformist psychology must perforce be the ethnic boundary* (fuller discussion in Gil-White 2001a and 2005a *under review*). Strongly reinforcing this interpretation is the fact that no objection was ever raised to the idea that a Torguud child would marry into a different Mongol tribe—so the point was to keep marriage *within the Mongol ethnie*.

All right, but what need is there for Torguuds to say that Kazakh norms are wrong? Can't they just say they are different? In principle, sure they could (and some individual Torguuds indeed do); but with Torguuds as with everybody else there is a tendency to moralize the differences. The reason is once again adaptive though it is also, certainly, unfortunate.

In order to choose interactional partners with matching norms, a human needs a motivational system designed to achieve this particular result. If you could design a human from scratch, you could give it any motivational system you wanted to, obeying any particular rules you chose to give it. Because I myself adhere to the values of modern humanitarianism, if I were given that job, I would design human beings so that they could make adaptive choices without negatively moralizing the things they dispreferred. Such humans would then make intelligent interactional discriminations, preferring those whose norms were more like their own, without ever casting aspersions on those whom they chose not to interact with—living and letting live, in other words. But I was not

given this job—nobody was. Human beings were not designed by some intelligent engineer but by a blind and stupid force: natural selection.

Natural selection is what we retrospectively recognize took place after some units of design managed to leave more descendants than others, making *that* kind of design more common than its competitors. It is a lawful process that can be understood, and Darwinian scientists are beginning to get a real grip on it, but for all its lawfulness it still has not one iota of consciousness. The designs that emerge out of this process are not at all like what an engineer would do from scratch—especially not what an engineer equipped with the ideals of modern humanitarianism would prefer. Rather, this blind and stupid process will make what it can with what is there.

So what was there? Here is my guess. Long before the emergence of mature ethnic boundaries, a human needed to figure out, within his local residential group, whom to interact with and whom to avoid. Some people are riskier than others, imposing greater costs and providing fewer benefits when we interact with them. The costlier ones tend to be less honest and reliable, and we may designate them with the technical term 'cheater'. Their main characteristic is that they tend to break the local norms. A well-adapted human needs a perceptual and motivational system that will help him or her avoid "getting screwed" by cheaters, and so natural selection equipped us with such emotions. People whom we observe to obey the local norms get positive values in the emotional utilities that help us decide whether to interact with them, and those whom we observe to break our norms get negative values. When the negative value is high, we say that we "mistrust" such a person. These emotions help us make proper interactional discriminations, because we feel inclined to avoid those whom we mistrust. In psychological jargon, the felt emotion of trust or mistrust is a *response*, produced by a *stimulus*, which is the perception of relative norm-conformity. This stimulus lies on a continuum, and so does the response.

Now, how does a human equipped with this emotional system perform in a world that comes to be characterized by ethnic boundaries? Well, when these emerge, any outgroup ethnic will become a stimulus to the trust system, because any outgroup ethnic will be failing to conform to the perceiver's own local norms. It is of course unfair, from a moral perspective, to say that an outgroup ethnic is *violating* my norms—entirely

unfair. But my trust system has been activated, because the stimulus it responds to is the perception of non-conformity to my local norms.

What will natural selection do with this result? *Keep it.* Why? Because it is adaptive for me to prefer co-ethnics over outgroup ethnics, and if I mistrust outgroup ethnics, the adaptive outcome follows—at the price of an injustice, it is true, but we can't get upset at natural selection because it had no choice but to use what was there, as it cannot make choices in the first place (it is, after all, a blind, unconscious process). The result is that human psychology is designed so that when we perceive outgroup ethnics conforming to *their* local norms, we are automatically tempted to treat this as a violation of our *own* local norms, and we take offense accordingly. Outgroup ethnics, then, are not merely different, but they do everything "wrong" (they have the "wrong book"), so we tend to avoid them, which is adaptive on average.

Remember that by 'adaptive' I mean simply 'conducive to passing on the genes as a result of giving a higher rate of easily matched—and therefore relatively low-cost—interactions'. But passing on the genes does not have to be our highest moral goal, and it clearly isn't: the most cursory look at the distribution of income around the world will demonstrate that those best equipped to support the largest number of children—Europeans—are producing the smallest families. So, given that there is no necessary relationship between natural imperatives and our own moral ones, it is perfectly reasonable for us to deplore the morally negative consequences of ethnocentrism, despite the fact that ethnocentrism is adaptive.

And we can overcome these negative consequences. This is obvious from the fact that some societies have learned to be more tolerant of ethnic differences than others. Any society that wishes to improve even further will benefit from a scientific understanding of the causes of ethnocentrism. It is not pathological, but perfectly natural, and it grows out of the adaptive pressure to conform to our local norms, so as to get along better with our locally relevant others. We can go on doing that, and also live and let live, but the best way to get there is to learn what our brains will be *trying* to make us do.

## *Conclusion*

Norm-conformism is an adaptive strategy that maximizes the number of potential interactants in the conformist's local population. It makes

sense to lament and oppose specific outcomes of particular conformist processes, such as some silent majorities, and ethnic prejudice. But to treat "conformism" and its consequences as a generalized evil in the abstract would spill a narrowly applicable moral evaluation into domains where not only does morality not apply, but where even a non-moral interpretation of the negative judgment "bad" will also not fit, given that norm-conformism does a lot of useful work helping humans navigate their social world. As always, it is best to put our moral goals in charge of conduct directed towards our fellow human beings. If we turn them instead into axiomatic priors of a scientific analysis, we saddle our attempt to understand human perception and behavior with epistemological baggage that makes it harder to understand why people do the things they do. Such ignorance can lead us to hurt people when we meant to help, and it follows directly that this is ethically undesirable. Therefore, if we have a compassion-based obligation to, first, do no harm, then we have a moral imperative to be honest about what causes human behavior, even if we would prefer to have been designed differently. Wishful thinking will not heal a troubled world, but an improved understanding of it just may.

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## NOTES

1. Technically, 'leap 'n' crawl' involves social learning as your default, initial strategy *most of the time*. That is, the equilibrial strategy gives initial preference to social learning with high probability, but there is always a small probability that individual learning will be used even in the beginning (see Henrich and Boyd 1998).

2. Notice, real subjects are not, in this case, influenced to add eight inches to their estimates, but about 10. In other words, although the fake subjects are two thirds of the population, and their proportional representation has therefore been doubled, their influence does *not* double but goes beyond that. What this means is that the influence of a majority is more than just the sum of individual influences, and it is precisely such a non-linear effect of numbers that demonstrates a conformist bias, for it shows that our psychology is impressed by the sheer fact of a majority, and not merely additively affected by, say, an increase in exposure to the meme due to its numerical superiority (Boyd and Richerson 1985: 206).

3. This does not include cases of "passing." This term refers to switches of identity that do happen in the first generation because those who hear a person's claim to be X don't know who this person's parents really are/were. But the fact that successful passing requires keeping biological ancestry secret only reinforces the point defended here.

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