

Prestige Affects Cultural Learning in Chimpanzees

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How strong is the evidence?

Posted by **dsperber** on **25 May 2010 at 20:51 GMT**

Boyd, Richerson, Henrich and their collaborators have argued that there is a bias in human cultural transmission causing people to preferentially copy prestigious individuals. This article seems to strengthen the hypothesis by showing that such is also the case among chimpanzees. While the study is ingenious and interesting, I don't see it as having quite that force, for the following reasons.

First – and this is a conceptual clarification rather than an empirical point – the idea of prestige goes together with that of reputation, i.e. of a socially shared opinion. Talking of prestige among the chimpanzees, who don't gossip about one another, extends the idea of prestige to that of being seen as superior. This then supports the prestige bias thesis only if, in that thesis, prestige does not mean more than this. In other words what it supports is at best a diluted thesis where the ordinary or the sociological notion of prestige plays no role at all.

Second, here is how the prestigious individuals were chosen in the experiments:

“In each of the two groups, we selected a pair of female models (model A and model B) ... In order to test the influence of social dynamics on learning, we maximized the potential social differences between the models, such that model A was older, held one of the highest social ranks in her group (based on daily observations) and had successfully introduced novel behaviors on several previous occasions. In contrast, model B was younger, held one of the lowest social ranks and had no previous experience in introducing novel behaviors.”

In other terms there is no separation of the age factor and the social rank factor. Moreover, the third criterion “had successfully introduced novel behaviors on several previous occasions” brings in an risk of circularity: We don't know what factor had helped an individual chimp to introduce novel behaviors in the past (imagine, for the sake of argument that it was something about her smell), and so it can be that same factor that explains her doing it successfully again.

Third, the authors themselves indicate an alternative interpretation of their results:

“Observers appear to have paid selective attention to model A over model B... Attention also appears to play a central role in the selective transmission of behavior.”

If observers pay more attention to model A than to model B, then surely, everything else being equal, they are more likely to imitate A than to imitate B. They may be paying more attention to A than to B because there is more to learn from A than from B, but also because it is riskier to ignore what A does than to ignore what B does, or because A is bigger and noisier, or whatever.

The thesis that “prestige affects cultural learning in chimpanzees” even in its appropriately weakened version, is not properly evidenced by these experiments.

No competing interests declared.

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Response to: How strong is the evidence?

vhorne replied to **dsperber** on **13 Jun 2010 at 16:28 GMT**

We agree that prestige is intimately linked with reputation, and that in our study we investigate evidence for prestige in the sense of superior influence over the behavior of others as a result of greater relative age, experience and/or social rank. Unfortunately, it appears that there are no controlled experimental studies for prestige effects in humans. Comparison between chimpanzees and humans is therefore hampered by the lack of comparative empirical work. We are open to the notion that human prestige can be separated from rank, age, or previous experience, but thus far the data are lacking, and we predict that the separation will be harder than it may seem.

With regard to the argument that we have investigated a “diluted” version of prestige in which “the ordinary or the sociological notion of prestige plays no role at all” we are unaware of evidence that prestige is commonly defined by the collective spoken language of a group, rather than the collective behavior of a group towards a particular individual. For example, the Oxford English Dictionary defines prestige as: “impressive or overawing influence, glamour... influence or reputation derived from achievements, associations, or character, or (esp.) from past success; a person's standing in the estimation of others.” This is a good fit to our chimpanzee study. To suggest prestige and reputation must rely on gossip between people seems to restrict the notion unnecessarily.

We agree that age, rank and experience were linked together in our study, within the concept of prestige. But isn't this also the case in human society? We know of no systematic studies of human prestige in which these variables have been counterbalanced and controlled, precisely because they tend to co-vary in natural situations. Individuals who have gained prestige within a community often exhibit a combination of all three - age, experience and

social status. Clearly, further studies are needed to tease apart the relative role of each variable in comparison to the power of the combined effect, both in humans and chimpanzees.

When we proposed that observers selectively attend to the behavior of the more prestigious model, we did not intend this as a competing explanation, but as one of the underlying causes of their subsequent decision to copy model A. It is their decision to pay more attention to model A than model B that is the core of our thesis. More influential individuals are attended to more by others. This must surely also hold true for humans, as it is difficult to imagine that prestigious humans affect others without any changes in attention.

The commenter points out that there are alternative explanations, such as model A performed more loudly than B, or had a particular olfactory signature. While this point is well taken, we refer to the strength of our two group, counter balanced methodology in interpreting the results. Two different chimpanzees with 'model A characteristics' were preferentially copied by two separate groups of observers. Additionally, in each group, model A used a different receptacle to counterbalance any preference by observers for the color, shape, or sound made by the apparatus used by model A. The models themselves made no special or unusual noises during the study, and the reliance on olfaction by chimpanzees is similar to that of humans. We are not aware of human studies on prestige controlling for noise or olfaction, so we fail to see why this should be a requirement for chimpanzee studies without good theoretical grounding. The power of our study is that two separate sets of observers chose to copy two different model A chimpanzees in each of their respective groups. The predictions regarding these models were based on shared social characteristics, and the predictions were statistically supported. Unfortunately, quantitative studies like ours seem not to exist for human cultural transmission. More pointedly, if we measure the current evidence for prestige in our own species by the same standards as proposed, we would probably consider the evidence rather weak.

- V. Horner, D. Proctor, K. Bonnie, A. Whiten & F.B.M. de Waal

No competing interests declared.

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RE: Response to: How strong is the evidence?

ericmjohnson replied to vhorner on 15 Jun 2010 at 07:39 GMT

Sperber's insistence that prestige is limited to populations who "gossip about one another" (i.e. humans) doesn't take into account the amount of information that can be conveyed non-verbally. Chimpanzees are highly social and utilize grooming in much the same way humans use conversation. Dunbar (1995) measured the number of individuals in the average human clique and estimated that human gossip was almost three times as efficient a bonding mechanism as chimpanzee grooming (<http://j.mp/bn1WOh>). However, in a reanalysis of Dunbar's data Nakamura (2000) estimated that:

"Chimpanzees can obtain about the same efficiency as humans in terms of quantity of social interactions because their grooming is often mutual and polyadic." (<http://j.mp/c4TkRC>)

In other words, the amount of social bonding that Sperber insists can only occur through gossip can be achieved nearly as well through nonverbal grooming behavior.

Second, it's difficult to understand how Sperber could object that Horner et al. didn't separate the variables of age and rank considering that research on prestige in human societies doesn't separate these variables either. Both are intertwined in the anthropological literature and contribute to the prestige of an individual within a given society.

For example, Stearman (1989) wrote of the Yuquí foragers in eastern Bolivia:

"The Yuquí concept of leadership and prestige . . . consists of (1) being saya [upper caste], (2) being a good hunter and therefore provider of meat to the band, (3) having senior status based on age (but only relative to the ages of the rest of the band), and (4) possessing a certain charisma in terms of an aggressive personality and ability to deal with peers." (<http://j.mp/d1M050>)

Begler (1978), in her analysis of egalitarian societies, likewise found age and rank to be intertwined:

"Age almost invariably provides the basis for a system of ranked statuses, whether appearing as formal age-grades, or simply being recognized informally as stages through which a person passes in the course of his/her life." (<http://j.mp/9enH4k>)

Finally, Henrich and Gil-White (2001) also linked age and rank together in their definition:

"Age is a proxy for skill/knowledge/success; the longer someone has lived, the more and better skills/knowledge he/she has likely accumulated. Simply living longer is a complex "skill" with acquirable components. Deference toward elders allows proximity and thereby promotes the acquisition of useful information. This reasoning predicts a general correlation between age and prestige, and also that elderly individuals will maintain their status well past their prime." (<http://j.mp/ackVeb>)

If Sperber accepts this multifaceted understanding of prestige for human societies it would be hypocritical to object when the same standard is applied to nonhumans.

Furthermore, the suggestion that there was "circularity" in the study because Model A in both groups had previously been observed introducing novel behaviors can't be taken as a serious objection. The purpose of the experiment was to determine whether novel behaviors were adopted because of prestige or because they were more effective. In the experiment both high-ranking Model A and low-ranking Model B received a food reward after placing a token in their respective containers, so the only reason to follow one versus the other was the social prestige of the model. What Sperber calls introducing bias was actually the variable the researchers were interested in studying.

Furthermore, his suggestion that "something about her smell" may have influenced the decision to follow Model A versus Model B is a flawed critique given recent literature on the subject. Matsui et al. (2010) have shown that there are no significant differences in the number of functional

olfactory receptor genes between marmosets, macaques, and the hominoids (<http://j.mp/8XAVHb>). Humans and chimpanzees have nearly identical numbers of intact or truncated OR genes (396 in humans compared to 399 in chimps) and both species use olfaction in largely the same way as the authors pointed out above. Furthermore, as Horner et al. reported, there were no threat displays by the chimpanzee models nor unusual vocalizations during the experiment that would have influenced other members of the group. The researchers further controlled for the color and appearance of the containers by using two separate groups and reversing the container used in each. In both groups it was the prestigious chimp alone who influenced others to follow her lead.

No competing interests declared.

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Rejoinder to Eric Johnson

dsperber replied to ericjohnson on 23 Jun 2010 at 08:19 GMT

Eric Johnson takes exception to my comment that: "Talking of prestige among the chimpanzees, who don't gossip about one another, extends the idea of prestige to that of being seen as superior." I maintain that, in its ordinary sense (which I am not particularly defending as the 'right' one), 'prestige' is a high positive reputation achieved through communication about the prestigious individual and resulting in a public opinion. I agree that much information can be communicated non-verbally, but the issue is whether chimpanzees actually communicate about their evaluation of third parties. Johnson suggests that they might do so through grooming, but his argument is oddly constructed. Conversation may indeed have, as argued by Robin Dunbar, a social bonding function similar to that of grooming, but it does not follow that grooming has the communication function of conversation. In particular, unlike gossip, grooming is not an exchange of information about third parties. (Incidentally, I have no idea why Johnson attributes to me the absurd view that: "social bonding ... can only occur through gossip." I never thought or said anything of the sort.)

Regarding Johnson's defense of the article against my reservations, see my rejoinder to the authors. Johnson has posted a longer and more personal version of his comment at his blog, "Primate Diaries" at <http://scienceblogs.com/p...> to which I respond there. His post on his blog has elicited further comments, in particular one by Frans de Waal to which I respond at www.cognitionandculture.n...

No competing interests declared.

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RE: Rejoinder to the authors

dsperber replied to vhorner on 21 Jun 2010 at 20:18 GMT

I am grateful to the authors for responding to my comments, but I am afraid that, possibly because I was not explicit enough, they have missed the main tenor of my reservations and, it seems to me, have not answered them satisfactorily. Since these are important issues for our understanding of animal and human culture, let's pursue the matter. To begin with, I should make it clear that I agree with the authors (if I have understood what they mean in the opening paragraph of their reply) that experimental evidence for the interesting hypothesis that there exists a 'prestige bias' among humans is far from compelling. More generally, I believe that 'prestige' is, in the social sciences, a rather vague concept of limited theoretical use. I am skeptical of the idea that it could be made more theoretically useful by importing it in the field of primate studies, where I maintain that it takes on a broader meaning, before re-exporting it into the social sciences.

The authors of the article summarize their findings as follows: "[W]hen given opportunities to watch alternative solutions to a foraging problem performed by two different models of their own species, chimpanzees preferentially copy the method shown by the older, higher-ranking individual with a prior track-record of success." What was properly controlled and established is indeed that the difference in copying behavior between the two methods is due to the differences between the two models. This is quite interesting in itself but raises a number of questions.

The contrast between the two models A and B was maximized along three dimensions: age, social rank, and previous successful introduction of novel behaviors. The possibility that one or several of these traits might be less or not at all causally relevant was not investigated, let alone controlled for. As the authors note, "Further experiments are required to tease apart the relative contributions of these aspects of prestige in chimpanzees." Moreover, together or individually, these traits might be correlated to some other trait that might be more causally relevant than any one of them. The authors themselves mention the role of attention. A possibility is that model A is the object of greater ongoing attention because of its rank, and that what causes its being more copied is this greater attention. In other terms, one possible interpretation that should be controlled for is that individuals that are paid more attention to are more often copied whatever the reasons for this attention. To control for this possibility, two individuals that are equally good candidates for being model A (or for being model B) should be differentiated by some added feature that would attract attention to only one of them. This feature could be for instance an artifact such as a salient bodily ornament or a bell, but people working with chimpanzees will no doubt have more appropriate ideas. Would the individual attracting more attention be more often copied? If so, further controls would be needed to ascertain how much of the difference in copying observed in the initial experiment can be explained as a result not of rank, age, and previous success per se, but as a result of attention. I have no strong reason to assume that an attention-based interpretation is correct, but it is not implausible and should be controlled for. (And by the way, I did not mean to suggest –it didn't even occur to me – that, in the experiment, it could be that model A had performed more loudly than B, or had had a particular olfactory signature; what I meant in evoking such apparently extraneous factors was that, in principle, it might be that any difference sufficient to grab attention would produce similar results).

The authors interpret their results "as evidence that the preferred model in each of the two groups tested enjoyed a significant degree of prestige

in terms of whose example other chimpanzees chose to follow." A problem with this interpretation has to do with what the authors mean by 'prestige'. Were they to equate prestige with a greater likelihood of having one's example followed, the claim would be tautological. What the article under discussion shows is that, among chimpanzees, some individuals are more influential, and this is not trivial. But to say that they are more influential because they are more prestigious (with prestige so understood) would be a tautology and not a genuine causal claim. If by 'prestige,' the authors mean a combination of age, rank, and previous success at introducing novel behaviors, then, as I have just argued, their evidence is not strong enough to show that prestige is the cause of greater influence. Moreover, if this is what they mean by 'prestige', they are using the term in a non-conventional way, and in particular in a way different from that used in the influential hypothesis of Boyd, Richerson and Henrich according to which there is a bias in favor of imitating prestigious individuals.

I had assumed, maybe wrongly, that this study was offered as an extension of the idea of a 'prestige bias' to the case of non-human primates. The authors, mentioning Henrich and Gil-White 2001, write: "The role of prestige is sometimes presented as uniquely human despite the absence of comparative research on this important topic." They do not address the argument that Henrich and Gil-White give for surmising that indeed prestige might be uniquely human. Henrich and Gil-White suggest that while, in non-human primate, there is a just dominance hierarchy based in particular on the power to coerce, in human there is both dominance based on the power to coerce and prestige based on the capacity to persuade and the two are quite distinct. This is not to deny that there is a correlation between dominance and prestige, but the correlation is interesting precisely because the two are not identical. Some people, artists, scientists or sport stars for instance, who are not of high social rank and who may be quite young, may nevertheless be quite prestigious. Do the authors really want to cast doubt on what I take to be a plain observation with massive historical, anthropological and sociological evidence in its favor on the ground that we lack experimental evidence directly supporting it? I agree that experimental evidence would be welcome too, but experimental evidence is not the only kind of evidence (as not just social scientists but also astronomers and ethologists will tell you).

In my initial comment, I made in passing the conceptual point that, in the social sciences, prestige is linked to reputation, and implies not just – and not necessarily - that many people have a good opinion of the prestigious individual but also and more importantly that many people communicate, through gossip for instance, such an opinion and communicate it not so much as their own private view but as a piece of common knowledge. Prestige is a cultural phenomenon: it is a positive opinion that is openly propagated as common knowledge. One of the consequences of this public character of prestige is that it is possible to have a dissociation between public and private opinions. An individual may be prestigious, and yet most people's private opinions may not match this prestige. Conversely, an individual may be highly regarded by many people in private and yet have a poor reputation. These situations occur for instance in situations of censorship, or when people come to individually doubt the merits of a prestigious person but are reluctant to speak against what they take to be public opinion. This aspect of prestige is obviously relevant to its social dynamic. I am not aware of any evidence showing a similar dissociation of public and private opinion among non-human primates. Taking together the possible dissociation between dominance and prestige discussed by Henrich and Gil-White and the possible dissociation between public and private opinion I have just mentioned, both of which are characteristic of prestige among humans, I would maintain that the authors use the term 'prestige' in a broader sense than what is usually implied in talking of prestige among humans. They are of course entitled to do so, but this detracts from the relevance of their study to the 'prestige bias' hypothesis.

No competing interests declared.

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