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A Book Review Made out of Whole Cloth

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Abstract

A good book review provides documentation for its evaluations, especially when they are either very positive or very negative. A good review is also faithful to what the author has written and bases criticisms or praise on accurate paraphrasing or quotes from the book. This review by Thompson fails on both accounts. Critical comments are not documented and the review is based on what Thompson imagines Read to have written, not what Read actually wrote.

Melissa Thompson's (2012) review of my book, *How Culture Makes Us Human: Primate Social Evolution and the Formation of Human Societies* (Lexington Press, 2012), is made out of whole cloth. She begins, polemically, by contrasting the "sophisticated" approach of biocultural anthropologists who consider, the "interactions of Darwinian evolution and cultural processes" with those supposedly "hailing the fundamental dominance of one mode of change" (p. 434). With these words, she constructs the image of a book that promotes an outmoded and simplistic opposition between biology and culture. In reality, I connect the biology of Darwinian evolution with the cultural richness of human societies through exploring the transformation from "a beginning point in which social organization and structure are explicable by reference to biological evolution driven by natural selection and biological kin selection" to "an ending point of cultural kin-based, hunter gatherer societies ... [where] we find cultural idea systems ... constituted as a 'complex whole' provid[ing] the framework within which individuals act and interact" (pp. 183-184).

These cultural idea systems, though integral to the fabric of human societies, do not exist for Thompson. Instead, she reduces culture -- despite an initial acknowledgement that it is "a critical feature of our species ... and is fundamentally different in both form and degree from social processes occurring in other species" -- to an "efficient mode of transmission within and across generations," sharing with biology the role of "shaping particular traits," and only existing to "support and enhance biological predispositions" (p. 435). This thin model of culture has already been found to be wanting. As two eminent philosophers of biology have expressed it:

These thin models for culture coexist with population genetics models because each is thought to describe a distinct transmission process, separated by a process of development through which genotypes make phenotypes. In both, it is supposed that we don't need to take seriously 'thick' or rich details of either development or culture to gain a deep understanding of these evolutionary processes. But by the same token, they have no purchase on the 'rich' details—or even (more troubling) on the very *existence* of rich details. And in failing to do the latter, we argue that they must fall crucially short of an adequate account of the nature and transmission of culture. (Wimsatt and Griesemer 2007:237, emphasis in the original).

Thompson fails to deal with the fact that human social systems arose through an "evolutionary trajectory going from systems of social organization based on face-to-face interaction to the relational basis for interaction that characterizes human societies" (p. 10). The importance of this transition lies in its implication that, as a consequence, "evolution of human societies becomes evolution in the system of organization for a society, and evolution becomes constrained and directed by the logic and consequences of what has been constructed, not by change in individual traits per se" (p. 31).

Thompson views the evolutionary trajectory leading to human societies in only one way, namely as an extension of non-human primate behaviors, with competition and selection the means to account for "group-beneficial behaviors." For her, the evolutionary pathway is simply the consequence of genetic transmission subject to Darwinian fitness selection acting on biologi-

cally based behaviors, and augmented by so-called culture transmission in the form of phenotypic transmission of acquired or learned, behaviors subject to selection by factors that structure who imitates whom. Cultural diversity arises, she says, in analogy with biological evolution, through “cultural evolution and social innovation” acting on “common biological foundations” (p. 435). She contrasts this with my supposed claim that if “patterns of primate social evolution underlie human behavior, the cultural practices must form a precise fit to biology” (p. 435). Since cultural practices do not form a precise fit to biology, it follows, according to her argument, that I am claiming human behavior is disconnected from its biological roots in the non-human primates. Nowhere does she provide evidence for this remarkable claim for the simple reason that it is her invention.

As I state in the Preface: “The challenge, when coming forward to human societies as we know them, has been to *connect our Darwinian beginnings* to the current complexity of human social systems ...,” and “This transition *involved more than just the elaboration of pre-adaptations and/or cognitive and behavioral capacities that were already present in an ancestral primate species*, but centered especially on the evolution of cultural systems” (pp. 10, 15, emphasis added). “More than” does not mean “disconnected from.” As for “patterns of primate social evolution” that I allegedly discount, I state that the pattern of non-human primate social evolution towards more individualistic behaviors carries forward to humans (see book Figure 4.5, which also shows continuity for genotypically and phenotypically transmitted behaviors as one goes from the non-human primates to *Homo sapiens*). I then observe that this has critical consequences; increased individuation, unless mitigated in some manner, introduces social complexity into social systems that is difficult to accommodate through means such as face-to-face interaction or biologically based behaviors introduced through, for example, biological kin selection. Similarly, I discuss how the evolutionary trend in the non-human primates from ostensive to performative forms of social organization carries forward to *Homo sapiens*, though by reverting to a more ostensive form of social organization among hunter-gatherers due to the presence of cultural systems that structure relations among societal members.

The “pattern of primate social evolution” for more individualized behavior led to limited means for mitigation of social complexity arising from increased individuation of behavior, namely intensification of face-to-face interaction and reducing the size of social units. I argue that going beyond this limited mitigation required a qualitative change in the basis for social organization. The qualitative change is from face-to-face interaction to the relational-based systems of social organization that we see in hunter-gatherer (and subsequent) societies that was ushered in by the introduction of kinship systems based on culturally defined, and linguistically marked, kinship relations.

Thompson attributes to me the assertion that “kin schema ... are structurally independent of genetic relationships” (p. 435), but what I wrote is the opposite: “*This does not mean that kinship systems are cultural constructs that determine kin relations in a manner independent of biological relations*, but only that kinship terminologies are not constructed according to the logic of biological relations,” (p. 24, emphasis added). That kinship systems are not constructed according the logic of biological relations has been well-documented (Read 1984, 2001, 2007, 2010; Read and Behrens 1990; Bennardo and Read 2007; Leaf and Read 2012, among other references).

Under her view of culture equated with the mode of transmission and her assumption that human social behavior is just an extension of the evolutionary trends for social behavior already present among the non-human primates, we are left without any means to account for the evolutionary development of relation-based systems of social organization. While some of the non-human primates may have the cognitive capacity to categorize on the basis of relations represented by behaviors such as female parenting, they lack the cognitive capacity to conceptualize the critical concept of a relation of a relation upon which a kinship system of relations depends, let alone the cognitive capacity to work out forms of organization and structure for social systems based on such a system of relations.

Trivially, relation-based systems of social organization were “selected for” in some sense, else they would not characterize human societies. Equally trivially, relation-based systems of social organization had to arise through an evolutionary process at least initially grounded in the biological facts of non-human primate societies. What we want to know, though, is how, in an evolutionary sense, cultural kinship-based systems of social organization came about and made it possible to develop coherent and well-integrated social systems that could also incorporate individualistic behavior. We also want to know how relation-based systems of social organization were able to transcend the limited means for incorporating individualistic behavior found in non-human primate forms of social organization, and what implications this had for the formation of new forms of social organization during the evolution leading to human forms of social organization. None of this negates the fact that, as a species, we carry forward with us a biopsychological heritage from our primate ancestry, thus making our behaviors today a complex mix of both behaviors arising out of that ancestry and behaviors coupled with the cultural systems that frame how we act socially.

Thompson wonders why I do not extend my observation that “kin recognition need not be perfect for [biological] kin selection to operate” to “fictive kin relationships in humans” (p. 345). First, “fictive kin” is her term and not mine. It derives from an outdated and incorrect assumption that kinship, as it is recognized in human societies, is biological, hence any use of kin terms for non-biological relations (other than affinal relations determined through marriage) is “fictive.” Fictive for whom? The culture bearers who use the terms in that manner or the analyst who presumes kinship as it is recognized in human societies is biological kinship? For the culture bearers, it is not fictive kinship. It is kinship. In our society, an adopted child recognizes the persons adopting her or him as mother and father, not as “fictive mother” and “fictive father.” Second, when biological kin recognition by individuals is imperfect, there must still be factors biasing social interactions towards one’s close biological kin in order for the conditions under which biological kin selection might operate to be satisfied. Third, with relation-based systems of social organization, the trend away from conditions amenable to biological kin selection is accentuated and biological kinship becomes even less a possible determinant of the range of social behaviors present in a society. With classificatory kinship terminology systems, argued by many to represent the earliest forms of cultural kinship terminology systems, the horizontal degree of biological kin distance is not recognized, contrary to what is required for biological kin selection to be effective. Fourth, and going back to her concern that non-human primate behavior trends underlie human behavior, primatologists report, as I discuss, that biological kin selection has decreasing

importance as we move forward from the Old World monkeys to the chimpanzees due to the introduction of more individualistic behaviors. Selection for more individualistic behaviors acts against biological kin selection: “Selection for individuality, it should be noted, is antithetical to biological kin selection, as the latter introduces ... homogeneity of behaviors across biologically kin related individuals, whereas the introduction of individuality does the reverse” (p. 116).

The consequences stemming from this evolutionary trend towards individuality are critical: “Increased individualization of behavior substantially increases the complexity of a social unit, and the combination of both increased complexity and individualization appears to reach a limit regarding processes such as biological kin selection and face-to-face interaction for introducing the traits upon which integration of complex social environments depends” (p. 138). Thompson may wish to claim that challenges to social coherence and integration due to increased complexity arising from an increase in individuation were resolved among the non-human primates through genotypic and phenotypic transmission, but she provides no evidence to show that this is possible. The trend we see in social organization going from the Old World monkeys to the chimpanzees strongly suggests that it is difficult, evolutionarily speaking, to introduce biologically based behaviors that ameliorate the increased complexity of social systems introduced by increased individualization of behaviors, while maintaining social coherency and integration.

Thompson says I overplay “the pervasiveness of [biological] kinship in dictating primate social interactions” and at the same time I under-represent “the capacity of primates to understand higher-order relationships” (p. 435), also without documentation or examples. Here and elsewhere Thompson refers to primates collectively, as if cognitive and behavioral capacities are shared equally among all primate species, whereas this is obviously not the case. What do primatologists say about biological kin selection in relationship to the major primate divisions? According to Strier, it plays an important role among the Old World primates: “some of the strongest evidence of [biological] kin selection in action has been found among female Old World cercopithecine monkeys, such as baboons, macaques, and vervet monkeys” (2000: 127, quoted on p. 40). In contrast, Langergraber, Mitani and Vigilant comment, regarding chimpanzees: “[biological] kinship plays a limited role in structuring the intrasexual relationships” (2009: 840, quoted on p. 104). As for Thompson’s higher-order relationships, are these well-understood by the prosimians, and do Old World monkeys have the same capacity for understanding higher-order relationships as do the chimpanzees? In both cases, the answer is “no,” and this difference in cognitive capacities is subsumed in my discussion of the evolutionary trend for the ostensive/performative contrast introduced into the primate literature by Shirley Strum and Bruno Latour (1987). It is with performative social systems where we begin to see higher-order relationships come into play, though it needs to be remembered that humans are the only animals that can “reason about higher-order relations in a structurally systematic and inferentially productive fashion” (Penn, Holyoak, Povinelli 2008:128, quoted on p. 148). I leave it to the reader to decide whether I overplay biological kinship regarding the Old World monkeys and underplay higher-order representations with the chimpanzees.

I am also said to undervalue the biological kin biased resource flows in human societies -- again without evidence. It is not clear to me in what way I have supposedly erred. From her comment, I suspect that Thompson would rather I discussed hunter-gatherer social organization

using assumptions such as optimal foraging theory, rather than focusing on the cultural kinship basis of their social organization. Trivially, there are biological kin biased resource flows in human societies. We share food in a family context and families are largely, though not exclusively, made up of biologically related individuals. Material goods inherited by children from parents are likely to be distributed along biological lines, and so on. However, ethnographic evidence for hunter-gatherers shows that not all food resource flows are determined by the degree of biological relatedness, and cultural rules regarding the distribution of hunted food resources typically incorporate non-biologically related individuals. The choice of Netsilik sealing partners, for example, emphasized distant [biological] male kin, or even unrelated males, as the sharing of concern was not that of sharing among family members, but among families without close kinship connections. Conversely, the Netsilik did not have the equivalent of sealing partners for sharing food within the family, for here the participants can rely, without cultural rules, on the biological proclivity of adult individuals to engage in parenting behaviors with their offspring.

Supposedly, I conclude that behaviors such as altruism and reciprocity cannot occur “unless individuals have a formal cultural representation of their relationship” and when it does occur, I am said to claim that culture is acting “against individual biological motivations” (p. 435). Again, no examples of my sins are provided. Concerning altruistic behaviors, what I actually wrote is that kin (in a cultural sense), “are expected to act altruistically to each other, *not because of the evolution of altruism as a biological trait*, but through culturally expressed concepts of kinship that define for individuals what it means for them to be kin to one another” (pp. 199-200, emphasis added). In other words, we, as a species, have either extended proclivities toward biologically based altruism by cultural means or introduced altruistic behavior when otherwise it is absent through behaviors associated with cultural kinship relations. Thus kin, as culture-bearers, understand that they are expected to act in a “prescriptively altruistic” manner regardless of their actual biological relatedness and even if biological altruism by way of Hamilton’s rule does not apply. Rather than the alleged replacement of biology by culture that Thompson attributes to me, what I actually wrote is quite different: the altruism introduced through cultural kinship is not restricted in the way altruism introduced with respect to biological kin is restricted by the conditions of Hamilton’s rule.

Concerning my comment about marriage legitimizing the offspring of a woman and Thompson’s assumption that marriage is simply a cultural practice institutionalizing pair-bonding, we need only consider that the extensive, ethnographic comparative accounts of marriage document the simplicity of her claim. Though we may consider, in our society, pair-bonding due to two individuals “falling in love” to be the ideal prerequisite to marriage, this is hardly a universal pattern. Marriage without prior pair-bonding is more often the case, though marriage may lead to pair-bonding. What does appear to be universal is the transformation made by marriage in the status of a biologically maturing female, in particular, concerning reproduction. As Bronislaw Malinowski stated almost a century ago: “The woman has to be married before she is allowed legitimately to conceive ... The principle of legitimacy works at times in indirect ways, but on the whole the law which demands marriage as the preliminary to family seems to be universal” (1930: 24-25). The Nayar of India are a classic example of Malinowski’s statement. Nayar marriage is not about pair-bonding but instead is about legitimacy (Gough 1961). In traditional times, if a Nayar woman had a child without yet being married, she and her

child could be put to death. The first marriage of a woman took place before she reached puberty and was consummated by a *tali* (necklet)-tying ceremony, not by sexual intercourse. The man who performed the *tali*-tying act would usually leave after the ceremony and she would have no further contact with him, yet children by other men through a *sanbandham* marriage must refer to the *tali*-tying husband as *appan* ('father') if they know him and must perform death rituals for him as their father upon his death. In effect, the first marriage enacted through the *tali*-tying ceremony legitimized a girl as an adult who could now conceive and have children recognized as legitimate members of her matrilineage. Without the *tali*-tying ceremony, she could only bear illegitimate children. There is no pair-bonding in the *tali*-tying marriage.

Thompson's comment that I supposedly attempt to reject "the hypothesis that ecology is a predictor of human social organization" (p. 345) by assuming "acreage rather than resource density [was] the most pertinent feature of the environment" is absurd. Of course human societies make adaptations to their ecology that relate to aspects of their social organization. I discuss, for example, how the often dismissed ecological argument made by Julian Steward and Elman Service for patrilineality and patrilocality as the likely form of social organization in hunter-gatherer societies is, in fact, supported by data (see book Tables 2-3) once we take risk into account: "Under cold conditions with a short growing season and high risk, group-level selection will favor forms of social organization with sons residing with fathers. In low risk, warm environments, material constraints are weaker and the residence patterns should be more varied." (p. 198).

As for the claim that I consider acreage to be more pertinent than resource density, consider what I actually wrote: "We obtain further corroboration that the population size of simple hunter-gatherer societies is constrained by the internal dynamics of social organization rather than by external ecological and environmental conditions through the former predicting that the area for a hunter-gatherer society should be independent of population size" (pp. 176-177). The "internal dynamics" refers to the maximal population size that can be maintained when societal members must be cultural kin to one another. The form of my argument quoted above is the standard one of corroborating a model by testing whether a prediction from the model is verified by data. Other predictions can be made and tested; for example, another prediction stemming from population size being determined by internal dynamics and not by external factors such as resource density is that population size and complexity of tools should not be correlated. In hunter-gatherer societies, complexity of tools varies with risk (Read 2006, 2008 and references therein), taking into account both the likelihood and consequences of failure of a hunting episode. As predicted, there is no correlation between population size and complexity of tools (Collard et al. 2005; Read 2006, 2008), thus providing additional corroboration of the hypothesis that the population size of hunter gatherer societies is constrained by internal dynamics.

Thompson concludes with the claim that I am saying, in effect: "cultural processes are simultaneously transcendent and arbitrary with respect to the needs of individuals" (p. 435). Yet what I wrote is the exact opposite: "Ancestral hunter-gatherer societies developed cultural means for the expression and continuity over generations of societal practices *from whose functionality individuals and families benefit*" (p. 15, emphasis added), and I gave as an example: "The system of sealing partners had group level functionality *from which individuals received fitness benefits*" (p. 193, emphasis added).

Perhaps what she means is that, in her view, knowing the “needs of individuals” suffices to account for the cultural forms and practices that supposedly satisfy those needs. If so, she is going back to a long-discredited functional argument that by knowing the needs of individuals we can account for the form of cultural phenomena. Cultural phenomena have functionality that meets the needs of individuals, but to assume that the need determines the particular form of the cultural phenomena is not valid since different cultural forms may meet the same need. A cultural kinship system makes it possible for the social field among hunter-gatherers to include several different residence groups, but the particular kinship terminology that is the basis of the kinship system can vary widely from one hunter-gatherer group to the other, even with comparable ecological and environmental conditions as I discuss using Australian and African hunter-gatherer groups as examples (see book Figures 3.2, 3.4). The proximal reason for the variation in the terminology structures relates to differences in the generative logic of a kinship terminology (Read 2007), much like languages differ due to having different grammars, but the reason for the variation in the generative logic does not yet have an obvious answer and may be historically contingent; regardless, a kinship system is anything but “transcendent and arbitrary with respect to the needs of individuals.” 8

A good book review should have a fair and accurate summary of the arguments of the author. The reader of a review depends on the accuracy of the reviewer’s summary. Evaluations are, of course, the province of the reviewer, but the reviewer has the responsibility to provide evidence for the evaluations, whether positive or negative. In my (hardly unbiased) view, Melissa Thompson’s review fails on both accounts and thereby does a disservice not only to me, as author, but, more importantly, to the readers of her review. It is a review made of whole cloth.

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