

The Thread: Commentary on Shaw, Scheel, and Gardner's review of Michael Tomasello's book, *A Natural History of Human Thinking*.

Julian Jaynes once said that some intellectuals, when faced with a large theory, "search for that loose thread which, when pulled, will unravel all the rest." (Jaynes, p. 447) And then he commented "And rightly so," as he should have. It appears as though Shaw, Scheel, and Gardner, (2017, Hereafter, S, S, & G) in their review of Michael Tomasello's book, *A Natural History of Human Thinking*, are looking for just such a thread. But rightly so? This commentary will suggest that S, S, & G have gone a tug too far.

The title of their review, *Tomasello Turns Back the Clock*, prefigures a search for a thread belonging to a much larger tapestry than just one book. The tapestry they are seeking to unravel is made clear in the second paragraph where the authors deliver one "tell" and one telling indictment. The "tell" (in poker, an unintended telegraphed clue to your hand) is their statement about Tomasello that, "His claims of species-wide deficit in chimpanzee cognition depends on studies that are methodologically flawed." (p. 125). The "tell" is the use of the word "deficit." And the telling statement is: "Entire programs of research, cited frequently in Tomasello's book, confound species membership, developmental histories, and testing conditions." (p. 125). The bulk of S, S, & G's commentary is less about the book and more about Tomasello's relationship to the "confounds" of these research programs.

The reason the word "deficit" is informative is that neither it, or anything like it, appears in *A Natural History of Human Thinking*. For chimpanzees, as a species, to have a cognitive deficit of this sort requires that they once were, or are now, as cognitively competent as human beings but that either evolution or certain circumstances today have drained it out of them. I can't speak for Tomasello, but my reading of his work does not support the claim that he believes Chimpanzees have a cognitive deficit. They are what they are. And, without a doubt, the research programs that study them have their flaws. But, with equal certainty, they are not so fumbling as to fail to notice a small human being trapped in a chimpanzee's body.

So is this just a lack of attention to detail on the part of S, S, & G? Perhaps. Certainly it is a tone setter for what I see as an overly zealous attempt to diminish a large body of work. But, more interestingly, I think it betrays a kind of chauvinism that they are implying inflicts Tomasello work. While forcefully stating that Tomasello fails to acknowledge the competencies found in cross-fostered chimpanzees, they simultaneously ignore that most of these competencies are not observed in the wild, in captive field environments, in caged animals, and, if found at all, only with considerable effort under experimental conditions. Chimpanzees have been around for millions of years. Yet, on their own, they have never developed the behaviors that S, S, & G seem to say are within them. Did they really have to wait for cross-

fostering humans to help them discover their inner “human?” To torture one of Noam Chomsky’s more famous quips, this does seem like believing that there is an island way off somewhere sporting a species of flight-capable, yet completely grounded birds, who are just waiting for humans to come along and teach them to fly.

These comments are, in no way, meant to disparage the expertise or valuable work that Shaw, Scheel, and Gardner have demonstrated over the years. But biases can do funny things. And sometimes harmful things. And, as I see it, *A Natural History of Human Thinking* is a quite serious, competent, and important contribution to our understanding of who and what we are. So this commentary is meant to balance what I see as a harmfully misleading review. But I get ahead of myself.

The following is the opening paragraph of the book review:

In the first two chapters, Tomasello outlines his hypothesis, isolating and describing what he considers to be critical components of human thinking. He presupposes that a common ancestor must have closely resembled extant nonhuman great apes in the ability to think. Tomasello cites research findings from primarily captive chimpanzees as evidence for what this common ancestor did or did not do. By book’s end, Tomasello has proposed a two-stage evolutionary explanation of human thinking that started with the common ancestor using cognitive skills only somewhat socially and only for competitive advantage. Tomasello speculates that changes in ecological conditions led to a need for collaboration to obtain sufficient food and that over time increased group sizes led to conventionalized communication and culture. His evolutionary narrative ends with the speculation that the accumulation of different cognitive skills resulting from different cultures has allowed humans to conquer all sorts of previously uninhabitable places. (Shaw et al., p. 125)

This is a fairly accurate rendering of the arc of Tomasello’s book. Unfortunately the book review ends there. Okay that’s an exaggeration, but only slightly. The three quotations of Tomasello in the roughly 2,658 remaining words are overshadowed by seven quotations from other sources. And all of this embedded in a single-minded press to invalidate Tomasello and his colleagues’ use of chimpanzee/human comparisons derived from their particular experimental designs.

There are six chapters in Tomasello’s book, *The Shared Intentionality Hypothesis; Individual Intentionality; Joint Intentionality; Collective Intentionality; Human Thinking as Cooperation*; and a *Conclusion*. Further, there are numerous subsections important to Tomasello’s thesis: *Second person thinking*, and *Perspectivity: The View from Here and There*, for example, in chapter 3. Also *Agent-Neutral Thinking*, and *Objectivity: The View from Nowhere*, in chapter 4. And *Sociality and Thinking*, and *The Role of Ontogeny* in chapter 5. All of these topics are important to Tomasello’s thesis. And their contribution is not diminished by

the singularity of S, S, & G's criticism. As valid as some of their criticisms are this, book requires a broader evaluation than the one crafted by Shaw, Scheel, and Gardner.

Shaw, Scheel, and Gardner segment their review into three parts; *Developmental Histories, Testing Conditions, and Absence of Evidence*.
Developmental.

S, S, & G begin building their case by citing examples of profound retardation in institutionalized children, Dennis (1960), for example, and Hunt et al, (1976), some of whom could neither sit alone nor creep at age one. Also, more recent cases, where Bakermans-Kranenburg et al., reiterated retarding effects of institutions on children's physical and cognitive development. These examples are meant to prime the reader's intuition about some of the problems of caged-raised chimpanzees. In particular, the uncertainty about biographical details such as how much time was spent in cages, whether they were traumatized by things like the death of a mother, and the number of hours per day in which they interacted with a human adult. With this lead-in the authors call attention to what Tomasello refers to as human-raised chimpanzees. They want the reader to see this as very different from cross-fostered chimpanzees who had been raised in the home like a human child. They point out that,

"Tomasello uses "human-raised" in reference to chimpanzees who had been raised from birth by humans and lived in an animal park open to the public. For the first 12 months of life they were raised within a human family. They were subsequently introduced into a group of other human-raised chimpanzees, but all still interacted directly (i.e., not through caging) with humans for at least 2 hours per day in various training and enrichment activities." (Shaw et al. p. 127)

They sum-up this description by noting that, "Indeed, human parents who raised children under Tomasello's "human-raised" circumstances would be considered unfit (Kennedy, 2011)." (Shaw et al. p. 127).

Testing Conditions.

Here S, S, & G make the point that valid group comparisons require comparable testing conditions. Citing Boesch (2007), they say that the vast majority of experimental procedures violate this central experimental paradigm. There is also a photograph showing a chimpanzee being tested alone in a dark cage while the observer-experimenter sits outside the cage with a minimum of social interaction. And they note how different this is from how human children are tested in a room with friendly adults and a sociable experimenter presenting the materials.

These testing conditions are flawed also, they comment, because without proper controls the parents, the tester, and the data collectors could influence the subject's responses.

Absence of evidence

In this section S, S, & G remind us that absence of evidence is not the same thing as evidence of absence. They cite Tomasello's discussion of joint attentional activities like giving and taking objects, rolling a ball back and forth, building block towers together, jointly putting toys away, and 'reading' books together. (Tomasello, 2014. p. 44). And they note that Tomasello claims these behaviors illustrate a uniquely human capacity for joint attention because he and his colleagues were unable to elicit such behavior in caged chimpanzees. And their point is that just because he couldn't find these behaviors in caged chimpanzees, it does not mean that they are not there. And then they protest that he completely overlooked research programs involving cross-fostered chimpanzees and thus failed to avail himself of evidence for just these sorts of behaviors. Further, they direct the reader's attention to a 1973 film showing Washoe, a cross-fostered chimpanzee raised by the Gardners, doing things like jointly turning the pages of a book. S, S, & G also include several still shots of Washoe playing a game of catch with Roger Fouts. (Shaw et al. p. 128)

So, lets go back to the beginning, back to the title itself, *Tomasello Turns Back the Clock*. Where is Tomasello turning the clock back to? Pretty much the dark ages of modern psychology. First the 1950s, citing Riesen and Kinder (1952); back to the days when "Authoritative psychologists used to teach that only the most basic postural and locomotor functions develop during the first year of human infancy, and these develop almost independently of the nurturing environment." (Shaw et al. p. 125) And then the 1920s when Köhler wrote, "There has arisen among animal psychologists a distinct negativistic tendency, according to which it is considered particularly exact to establish *non*-performance, *non*-human behavior, mechanically-limited actions and stupidity in [nonhuman] animals." "Tomasello's book would have fit nicely in the negativistic zeitgeist among comparative psychologists in the 1920s." (Shaw et al., p. 129) This last quote, occurring in their summary paragraph, leaves the distinct impression that, buyer beware, Tomasello's book is going to be a particularly unpleasant read. The first quote of Riesen and Kinder, appears right after their two introductory paragraphs, and sets the stage for what S, S, & G have come to talk about; Tomasello's complicity in a decades-long methodologically flawed research agenda. And here, it gets puzzlingly interesting.

To support their case S, S, & G draw upon the work of the psychologist Timothy Racine. In particular, citing three passages from a chapter called *Conceptual and methodological issues in the investigation of primate intersubjectivity*, in a 2008 book titled, *Enacting intersubjectivity: A cognitive and social perspective on the study of interactions*. After a philosophical discussion of the epistemological assumptions underwriting species comparisons, Racine and his co-authors turn their attention to three methodological failings of these comparisons. The first is a failure to control for rearing history, the second is a failure to control training regimens, and the third is confounding experimental manipulations across levels of independent histories. There seems to be little doubt that Racine is the go-to guy to help S, S, & G make their case. And, indeed, he is. Except for one puzzlingly interesting detail; he has nothing but good things to say about Michael Tomasello. Below I will reproduce the Racine citations and then comment on them in light of other things that Racine has to say.

S, S, & G's first citation of Racine is in the Developmental Histories section where they mention the rigorous experimental regime required in the cross-fostering of chimpanzees. After stating that this has rarely been attempted they evoke Racine's support with:

"only two formal cross-fostering experiments have raised apes in a human culture from near-birth. The first was the study by Hayes and Hayes of a single chimpanzee subject, Viki. The second was by Gardner and Gardner; in this experiment, four chimpanzees were cross-fostered from neonates: Moja, Tatu, Dar, & Pili (Pili died at less than two years of age, so there is a limited behavioural record for him). Thus, to our knowledge, only four chimpanzee subjects in the history of science (excluding Pili) could have served, in principle, as an experimentally valid comparison to human children. (Racine p. 73)" (Shaw et al. p. 126)

While it is to their credit that S, S, & G bring up this statistical issue, it certainly does not work in their favor. A statistical N-of-four is not exactly the strongest starting place.

Citing Racine again in that same section:

"There seems to be a very widespread misconception that any ape raised by humans is therefore cross-fostered by humans to the same degree, regardless of the diversity of circumstances in which these apes have lost their biological mothers, the wide variations in the social, emotional, and physical environments in which they were raised from birth, and the extraordinarily large differences among captive apes in their relative familiarity with humans (Racine, p. 73)." (Shaw et al. p. 127).

The next sentence in Racine's paper extends this support of S, S, & G with: "We hope that it is now obvious that virtually all direct comparisons of the cognition of apes and humans are invalid from an experimental point of view." (Racine, p. 73). This dovetails with S, S, & G's earlier citations from Ghodssi, and Akiyama about possible developmental retardation, physical and cognitive, due to institutionalized child rearing. But then Racine's comments take an unexpected turn: The very next sentence is: "This is not to argue that the whole enterprise of comparative cognition is meaningless ..." (Racine, p. 73). Hold onto that thought for a moment.

The third citation of Racine is:

"If one compares individuals from two separate groups with radically different rearing histories and finds a significant difference in a dependent variable between those two groups, then one cannot rationally conclude that you have uncovered a group, but not a rearing history, difference; it is entirely unclear, in these kinds of research designs, whether differences between apes and humans are attributable to species differences (i.e., different evolutionary histories), rearing history differences, or some interaction between these evolutionary and developmental factors. Whenever a researcher concludes, [as Tomasello did,] from research designs like this that they have identified a species difference in social cognition, this conclusion can only illustrate the interpretive bias of the researcher. (Racine, p. 73)." (Shaw et al. pp. 127 - 128).

For now, I want to note that the bracketed reference to Tomasello in the above citation does not occur in the original Racine paper.

Racine Redux.

Racine, Leavens, Susswein, and Wereha's 2008 paper sets out to examine the conceptual and methodological issues in primate intersubjectivity. Particularly, they examine the assumption that mental states are causal precursors to acts defined as declarative and imperative pointing. They also review methodological techniques used in cross-species comparisons to study these issues. I will only look at their methodological comments since that is what S, S, & G draw upon.

The backdrop against which Racine et al. examine these methodological issues is the juxtaposition of the camps of two well-known researchers, Michael Tomasello and Daniel Povinelli. Each of these researchers, along with their colleagues, produced a target article in the journal *Behavioral and Brain Sciences* in 2005 and 2008, respectively. Both articles argue that human social cognition is unlike that of any other species in nature. What is particularly

interesting, Racine points out, is that this is about the only thing the two research groups agree on. “In this chapter,” Racine et al. say, “we conceptually and methodologically analyze the arguments in support of these conclusions.” (Racine. p. 66). The points made in the Racine paper are in agreement with those made by S, S, & G. And if the reader is new to these observations, by the end of the paper she should have a new understanding of the difficulties associated with cross-species comparisons. And if the reader only knew Tomasello’s book by the review of S, S, & G., she would acquire yet another new, and surprising, understanding. That is, for all the heavy citations by the reviewers, in no way does the Racine paper support their harsh criticism of *A Natural History of Human Cognition*.

Some of the evidence for the difference between S, S, & G’s Racine and real Racine can be seen in a few of the casual, en passant, lines in the Racine paper. For example, on page 73, shortly after the second quote by S, S, & G, we see, as mentioned earlier, “This is not to argue that the whole enterprise of comparative cognition is meaningless.” (Racine, p. 73). And, later on, we find, “Again, in the face of the extraordinary difficulty of adequately controlling for rearing history factors in ape-human comparisons, we believe that these kinds of experiments are worthwhile.” (Racine, p. 74). And, on the other side of the coin, Racine casts a dimmer light on the kind of “experiments” that S, S, & G approve of. “we cannot, for example, consign human children to be raised by chimpanzees. Quasi-experimental designs can, however, be achieved by cross-fostering apes with human caregivers.” (Racine, p. 72). With their forceful broad-sides against Tomasello’s methodologies, the reviewers would, likely, be a bit uncomfortable with their most called-upon source referring to their preferred research program as “quasi-experimental.” But there is more.

In section 5.3.2 of *Conceptual and Methodological Misconceptions of Behavior*, Racine et al. deliver on their promised evaluation of Povinelli and Tomasello. For example, in the opening paragraph of that section we have, “Given that this is such a commonplace practice in comparative psychology, it is perhaps not very surprising that rearing histories are ignored in much of contemporary empirical research – albeit with some notable exceptions.” (Racine. p. 74). What notable exceptions are Racine et al. referring to? They direct the reader to reference 33 which is, *A Nonverbal False Belief Task: the Performance of Children and Great Apes*, by Call and Tomasello. Yes, you read that right, Call and Tomasello!

Then, turning to Povinelli, Racine et al. say: “More likely, the improved performances of the chimpanzees, compared to the human children, simply reflected either their superior grasp of the task requirements or their greater motivation for their rewards, *contra* Povinelli and

colleagues.” (Racine. P. 75). They continue their less-than-flattering comments of Povinelli. “Thus, when Theall and Povinelli failed to find a difference in the rates of the hapless chimpanzees’ attention-getting behavior between the so-called “attentive” and “inattentive” conditions, it is entirely unclear whether this is attributable to the apes’ inability to discriminate attentive from inattentive states, as the authors concluded, or whether, as seems much more likely to us, the chimpanzees were displaying attention-getting behavior to enigmatically unresponsive experimenters, irrespective of their attentional state.” (Racine. P. 75).

Tomasello receives a much better treatment. After noting that “virtually all existing studies purporting to compare humans with apes suffer from one or more of [the three methodological flaws mentioned earlier].” Racine et al. open the third to last paragraph of their paper with the following: “A more informative and nuanced approach to comparative cognition is to sample apes from varied backgrounds, comparing them to human children. This kind of protocol was pioneered by Tomasello and his colleagues.” (Racine. P. 75). The paragraph continues with applause for this protocol and ends with, “Thus, when within-species variation in rearing history is properly accounted for in ape-human comparisons, the apparent influence of species as a factor in social-cognitive development is reduced, a conclusion also reached by Tomasello et al.” (Racine. P. 76).

The point I am making here is that the Racine paper, the single most cited source in S, S, & G’s review, does not support their indictment of Tomasello. The Racine paper does support their criticism of current methodologies of cross-species comparisons. But, in doing so, it unambiguously singles-out Tomasello as a notable exception, implementing an “informative and nuanced approach to comparative cognition.” You would never know this from the Shaw, Scheel, and Gardner review.

Ignoring Other Sources.

In several places S, S, & G complain that Tomasello does not cite cross-fostering studies. For example, after quoting a particular statistic from *A Natural History of Human Thinking* they say, “Failure to consider evidence from cross-fostering studies, in favor of studies that systematically confound living conditions and species, represents a fundamental misunderstanding of experimental method.” (Shaw et al. p. 127). And they follow this with the second of their three Racine citations about misconceptions regarding apes raised by humans and those reared in a cross-fostered environment. But, as noted above, that very passage in Racine et al. is followed by a cautionary qualifier that the whole enterprise of comparative cognition should *not* be considered as meaningless. (Racine et al, p. 73). This is very different from the tone they have set with comments like “Entire programs of research, cited frequently

in Tomasello's book, confound species membership, developmental histories, and testing conditions." (Shaw, p. 125)

In another place they say, "However, Tomasello again fails to cite any cross-fostering studies. Instead, he cites his own collaborations with Call (Call & Tomasello, 1996; Tomasello & Call, 2004). Tomasello fails to specify what he means by 'space.'" (Shaw et al. p. 129). I can help a little bit with this one.

By "space" Tomasello is referring to Knowledge of the Physical World; a topic he discusses in depth in Part 1 chapter 2, of his 1997 book *Primate Cognition*. The demands of foraging for food, locating it and manipulating it, for example, put unique stress on the primate's ability to understand large-scale foraging space (cognitive mapping). Equally important is remembering the movements of food and other objects from one place to another in small-scale manipulative space (object permanence). The demands are very different for home-reared chimpanzees where humans provide the food, and do the remembering of where it is. Also the demands of spatial manipulation is different due to the kinds of objects provided by humans as well as the types of manipulations of those objects encouraged by caregivers.

Getting back to S, S, & G's complaints; a third time, they say, "By completely overlooking research programs involving cross-fostered chimpanzees, Tomasello missed substantial evidence of just the sort of behaviors he failed to find in his caged chimpanzees." (Shaw et al. p. 128). Left unaddressed, the reader of this book review might infer that Michael Tomasello is either unaware of, or possibly dismissive of, research going on around him. But in the quote above where S, S, & G get confused by "space," they include Tomasello's references, (Call & Tomasello, 1996; Tomasello & Call, 2004). These references unambiguously show that he is drawing on much more than his own collaborations. To make this clear I would now like to turn our attention to those references.

What did he know and when did he know it.

The 1996 book chapter by Call and Tomasello, called *The Effect of Humans on Cognitive Development of Apes*, is focused directly on cross species studies. In this chapter they explore the degree to which human cultural environments affect the cognitive development of apes such as chimpanzees. They note that it is quite common for researchers to find differences in the cognitive skills both within studies and across studies. And that undoubtedly the outcomes are influenced by the degree to which, or manner in which, particular individuals have been in contact with the human cultural environment. Then Call and Tomasello make comments that are almost indistinguishable from those of S, S, & G. "The major problem in attempting to

review evidence related to this issue is that humans may intervene in the lives of apes in many and varied ways that do not always fall neatly into categories. In addition, there are many ape individuals who have spent different parts of their lives in different settings. To make matters even more complex, in many published reports scientists do not report, or report only briefly, on the histories of the individuals being studied.” (Call & Tomasello 1996. p. 372). Call and Tomasello then produce a list of general descriptive terms identifying the context in which animals have been raised that they are going to review. They are: Wild, Captive, Nursery-Raised, Laboratory-Trained, and Home-Raised. The Home-Raised category includes, “Apes raised by humans in something like a human cultural environment (sometimes including exposure to or training in symbolic skills); the environment need not literally be a home but must include something close to daily contact with humans and their artifacts in meaningful ways.” (Call & Tomasello 1996. p. 372).

Under each of these headings Call and Tomasello provide a table listing studies that evaluated Object Permanence, Object Manipulation, Symbolic Play, Tool Use, Use of Mirrors, Categorization, and Numerical Skills. (Call & Tomasello 1996. table 17.1) They also provide a second table (Call & Tomasello 1996. table 17.2) evaluating, under each contextual category, social knowledge such as, Social Attention, Social Referencing, Joint Attention, Intentional Communication, Social Learning, Teaching, Cooperation, and Theory of Mind. In these tables and in their discussions Call and Tomasello refer to all the studies, to that date, in which chimpanzees were cross-fostered.

While it is true that these details are not expanded in *A Natural History of Human Thinking*, the references *are* there. And, even a cursory reading of those references shows that it is not true that Tomasello completely ignores problems of cross-fostered chimpanzees.

One further point on this. In a 2011 paper called *Methodological Challenges in the Study of Primate Cognition*, Tomasello and Call offer a unique suggestion for the very kind of problem that S, S, & G imply that he ignores. “Laboratory studies of primate cognition face the problem that captive populations of a species are not always comparable, and generalizations to natural populations are never certain. Studies of primate cognition in the field face the problem that replications are expensive and difficult, and again different populations are not always comparable.” Their suggestion is: “To help remedy these problems, we recommend the creation of data banks where primary data and videotapes may be deposited (perhaps as a requirement of publication) to facilitate cross-examination, replication, and, eventually, the pooling of data across investigators.” (Tomasello & Call, 2011, p. 1227).

Don't let the perfect be the enemy of the good.

Many, perhaps most, scientific experiments are flawed in some way. If we waited until everything was ideal, then we would probably learn very little. One way to think of this is that you do the experiments you *can* do; you learn from your mistakes, you turn those mistakes into opportunities, then you do that again. The now famous 1978 Premack and Woodruff study, *Does the chimpanzee have a theory of mind?* was not perfect, as others, Savage-Rumbaugh et al, for example, were quick to point out. (Savage-Rumbaugh et al., 1978). But from the ashes of this, so to speak, came the sub-discipline in developmental psychology called Theory of Mind. Another example of this process comes from Tomasello and Call's *Methodological Challenges in the Study of Primate Cognition* paper. Gorillas in captivity, they state, "are quite proficient at using tools in a variety of extractive foraging tasks, although they have not been observed to use them in the wild. Because all of the other great apes also use tools proficiently in the laboratory, this suggests that the common ancestor to all apes used tools—an hypothesis that could never be formulated if gorillas were not tested for tool use in captivity." (Tomasello and Call. 2011. p. 1227).

Scientific progress is a temporally extended, exasperatingly slow, fits-and-starts endeavor. And confidence is not built from a single domain of inquiry. It is accrued via the convergence of multiple, independent sources of information. Judgments have to be made about convergence across a broad range of observations; in this case from observations in the wild, observations and manipulations in captive field environments, studies with caged animals in controlled experiments and, of course, cross-fostered animals. All of these domains have their difficulties. While the issues that S, S, & G raise regarding cross-species comparisons are valid, it is certainly not the case that *all* of the individuals in these controlled experiments are cognitively traumatized as they, at least implicitly, imply.

If Tomasello does not discuss cross-fostered studies to the degree that S, S, & G are happy with, perhaps he has reasons; reasons that are not elaborated by the authors. Perhaps it is because he understands that, like the gorilla example above, apes raised in stimulating environments will develop behaviors and strategies complementary to the affordances of those environments. Also, we know that the numbers of cross-fostered studies are small, the statistics are sparse, and the intimacy with caregivers present special problems of interpretation. So the nature of the cognitions behind the examples S, S, & G give of Washoe, for example, jointly turning the pages of a book and playing a game of catch with Roger Fouts, cannot be evaluated on just the reports of those who raised and tutored him. I'm just saying.

Something worthy of noting.

In the other reference that S, S, & G cite (Tomasello & Call, 2004, *The Role of Humans in the Cognitive Development of Apes Revisited*.) we see something that these reviewers could have benefited from noticing. And that is Tomasello's ability to adjust to new findings, admit that he was wrong, and utilizing the new understandings, move forward; or in S, S, & G's parlance, turn back the clock.

In this 2004 Commentary in the journal *Animal Cognition*, Tomasello and Call review a basic finding that they arrived at in the 1996 book chapter just cited above. The "finding," at that time, was that enculturated apes seem to have developed special cognitive skills in two domains: imitation and gestural communication. The hypothesis they settled on was that this was simply due to being treated as intentional creatures, and that it was this factor that could lead to a fundamental change in their social cognition. Underwriting this hypothesis, based on then available studies, was the assumption that apes could not understand others as intentional agents. In the 2004 paper, Tomasello and Call say that they were wrong. And that "recent studies suggest that non-enculturated apes may have some understanding of intentions in others." (Tomasello and Call. 2004. p. 213). I'm not qualified to opine on how this new understanding is going to shake out, it's a work in progress. But I will note that this flexibility, if not professional courage, is a trait of the author and his collaborators that we would not pick-up on from the tone of S, S, & G's book review.

For our present purposes, this is more than just a passing observation. In the preface to *The Natural History of Human Thinking*, Tomasello announces that this current work emerged out of this adjustment to new data. And, in explicating this, Tomasello makes it clear that this is not a "stand-alone" work (my term). The opening sentence is: "This book is a sequel – or, better a prequel – to *The Cultural Origins of Human Cognition*." (Tomasello, 2016. p. ix). One of my motivations for writing this commentary concerns the neglect on the part of S, S, & G to reflect the history and context of *A Natural History of Human Thinking*. At the end of their review the authors say, regarding their evaluations of Tomasello and his collaborators, that "these fundamentally flawed studies cannot support the conclusions Tomasello presents in *A Natural History of Human Thinking*." This comment could be interpreted narrowly as simply a review of the contents of one book. But, as I see it the reviewers drift far and wide, indicting whole swaths of research as well as neglecting to reflect the context of the book as it is set out in the preface.

Summary

Shaw, Scheel, and Gardner's review of *A Natural History of Human Thinking*, devotes the entire first paragraph to telling us what this book is about. And with those 150 words the

review part of this book review comes to an end. (More or less). The reason for such a quick dispatch of the six chapters in Michael Tomasello's book is that that is not what S, S, & G came to talk about. They've come to talk about time travel, "Tomasello Turns Back the Clock;" stuff that's not in the book; "His claims of species-wide deficit in chimpanzee cognition;" and things that are (arguably?) not true: "Entire programs of research, cited frequently in Tomasello's book, confound species membership, developmental histories, and testing conditions."

So, have Shaw, Scheel, and Gardner made their case that Tomasello's dependency on such fundamentally flawed studies undercuts support for the claims in *A Natural History of Human Thinking*?" I'll let readers decide. But, for what it's worth, I believe that S, S, & G have overstated their case. That they have understated the weaknesses of the cross-fostering methodology. That, paradoxically, they seem to have missed the generous support for Tomasello from Racine, their most often cited source. They seem to have not noticed that Tomasello understands the weaknesses of cross-species comparisons, has written about them, and according to Racine, taken measures to compensate for them. And, lastly, they have not conveyed something that they undoubtedly know; that advancement in science is contingent upon the convergence of multiple, independent strands of evidence. Independent lines of convergence that have the capacity to offset or override the conflicting variance of any single mode of inquiry.

At the beginning of this commentary I said that S, S, & G were looking for that loose thread which, when pulled, will unravel all the rest. The loose threads that they are looking to pull are the confounds that they believe are built into entire research programs of cross-species studies. The immediate unravelling they intend is the demonstration that the "results of these fundamentally flawed studies cannot support the conclusions Tomasello presents in *A Natural History of Human Thinking*." (Shaw et al., p.129). But I also alluded to a greater tapestry that I thought the authors were also interested in unravelling; undermining the legitimacy of these studies in general. My goal in this commentary was to show that the threads S, S, & G are tugging at are stray strands, lint, and, perhaps well intended, but self-woven illusions. The book and the tapestry holds.

There is one remaining question: is Tomasello turning back the clock? And the answer is: Yeah, back to the future! And speaking of temporal things, Tomasello has a more recent book called *A Natural History of Human Morality*. Which, together with *The Cultural Origins of Human Cognition*, and *A Natural History of Human Thinking*, forms a trilogy of sorts. I recommend these books to all members of our species. Certainly readers will learn more about chimpanzees than they ever thought they would. And, perhaps, be better humans for it.

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