Institutions, Ontogenesis, and Evolutionary Metatheory

Commentary on Packer and Cole

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Packer and Cole (this issue) are to be congratulated for writing a timely and provocative critique of cultural evolution theory (CET) and shared intentionality theory (SIT), which Packer and Cole nest in an ongoing metatheoretical debate concerning the potential reconceptualization of evolutionary theory. It seems fair to boil Packer and Cole's proposal down to the suggestion that cultural evolution and shared intentionality theorists, and, by extension, evolutionary and developmental sciences in general, have overlooked the enabling role that human institutions play in human evolution and development (cf. Searle, 2005). Unfortunately, doing justice to Packer and Cole's lengthy and informative article in a single commentary is simply not possible, and I will focus primarily on what is novel about their proposal in light of contemporary evolutionary theory and metatheory. In essence, although I agree with many of their conclusions and criticisms of extant theory, in my estimation their paper is unlikely to change many minds one way or the other.

Evolutionary Theory and Metatheory

Packer and Cole spend a sizable chunk of their article introducing the neo-Darwinian Modern Synthesis (MS) and contrasting it to the so-called Extended Evolutionary Synthesis (EES), the latter of which they apply to CET and SIT to demonstrate their respective shortcomings from an EES perspective. They do an impressive job of exposing a species of modern synthetic logic in Boyd and Richerson and Tomasello by showing essentially that when either camp speaks of evolution, genes, or adaptation in gen-

eral, they presuppose a neo-Darwinian metatheory – even when they speak of culture. However, the shortcomings in CET and SIT are also shortcomings to the extent that the EES is taken to be a more adequate general evolutionary framework than what, for example, Wray and colleagues, in a defense of neo-Darwinism, term standard evolutionary theory. Here I think Packer and Cole might be in some difficulty. First though, let me underscore, paraphrase, and unpack what Packer and Cole have accomplished.

CET and SIT are commonly understood to have been constructed to solve different sorts of problems; the former is mostly of interest to students of evolutionary and cultural theory, the latter mostly to developmentalists. In brief, Boyd and Richerson (1985) built on Dawkins' (1976) earlier suggestion that memes (understood as units of cultural information) could function in a manner analogous to genes. Although there were other mathematical treatments of cultural evolution before Boyd and Richerson (1985), their version of CET is seen as a landmark work; its popularity, and indeed plausibly, depended on the rigor of its mathematical modeling of the evolution of social learning. In particular, they were able to demonstrate that social learning would outcompete individual (trial-and-error) learning in circumstances where (a) individual learning is costly, (b) environmental change is relatively slow, and (c) selection is negative frequency dependent, meaning that social learning would lose its competitive edge when it becomes a very common strategy.

The motivation for CET includes some degree of dissatisfaction with the neo-Darwinian modern synthetic conception of evolution as a process whereby natural selection acts solely on populations of genes; the products of social learning, including culture, afford a second stable source of variation upon which selection can act. There are numerous examples of cultural information affecting gene frequencies in populations, the canonical one being lactase persistence; the rapid evolution of lactose tolerance has also been documented in several populations. The fact that pastoralism could strongly select for particular genes also underscored the extent to which adaptation to the Holocene could affect evolutionary processes. This enabled gene-culture coevolutionary views that would compete with the sort of evolutionary psychology that equated evolutionary change to genetic change with the attendant implication that evolutionary change, given its dependence on genetic change, must be slow and humans must be adapted to a long-vanished way of life that existed in the Pleistocene. Similarly, human behavioral ecology is an evolutionary theory that studies and conceives of adaptation as a real-time process involving differential reproduction and survival in a modern context. (For more detail about these major evolutionary theories of human behavior, see Laland and Brown, 2011.)

I think it is fair to say that CET, human behavioral ecology, and especially the sort of evolutionary psychology associated with Buss, Cosmides, Pinker, and Tooby do not directly challenge the neo-Darwinian "orthodoxy" in that these theories assume a common view of genes, culture, selection, adaptation, innateness, ontogeny, phylogeny, and a variety of related concepts. One could say that all three assume the neo-Darwinian metatheory. Thus, although CET explicitly includes what Jablonka and Lamb (2014) would later call a second dimension of inheritance, and human behavioral ecology explicitly includes dynamic real-time individual processes of adaptation in a way that arguably might make some room for developmental systems theory (DST), both approaches to human behavior do so within what Wray and colleagues call standard evolutionary theory. Similarly, the theory of Tomasello and colleagues, which is not an evolutionary theory in the way that are the aforementioned but rath-

er a developmental theory that relies on evolutionary evidence, also does not challenge conventional neo-Darwinian thought. The reasons are essentially the same and include additional considerations about the importance and nature of psychological states that are only shared with evolutionary psychology and not CET or human behavioral ecology. Despite evolutionary psychologists and Tomasello both seeming comfortable in their use of neo-Darwinian adaptationist reasoning and a representational theory of mind, evolutionary psychologists of course envision a large number of independent evolved information processing mechanisms each adapted for its own function; Tomasello postulates a more domain-general mind and a very small number of specialized information processing mechanisms.

The Need for an Extended Evolutionary Synthesis

To a first approximation, one can take the EES to be predicated on the conviction that evolutionists should not put undue emphasis on a single factor, be it genetic, epigenetic, cultural, material, psychological, behavioral, and so on. In addition, and here it gets a bit trickier, there are views of reciprocal causation in the EES that Packer and Cole justifiably see in CET because of its reliance on niche construction theory (NCT). However, the EES is really a nod toward a more pluralistic view of evolution, one that certainly “extends” neo-Darwinism, but which is constituted by a large number of processes and effects about which population level differences in genes frequencies have very little to say. NCT trades on an active view of the evolutionary process and arguably, by extension, one could think of DST in a similar manner and even evoke reciprocal causation. Packer and Cole also mention evolutionary developmental biology (evo-devo) as part of the EES, which it is understood to be, but which really has little in common with niche construction and developmental systems in the sense of an active and/or reciprocal view of the evolutionary process.

Although many aspects of the EES have applications in areas of biology and other disciplines that draw on evolutionary theory, evo-devo is arguably the only “component” of the EES that is truly a paradigmatic alternative to the MS. Changes in population-level gene frequencies through mutation and recombination do not explain the evolution of developmental processes; one needs to evoke evo-devo concepts like conservation, regulator genes, and so forth. Thus, also evo-devo cannot be accommodated within the MS and shows something about the limits of neo-Darwinism; it is an entirely separate question whether grouping evo-devo together with a variety of phenomena and theories adds up to anything like an alternative general evolutionary metatheory. To make things even muddier, in addition to evo-devo, NCT, and DST, the EES is also said to be constituted by, for example, complexity theory, multilevel selection theory, genomics and networks theory, epigenetic inheritance, plasticity and accommodation, evolvability and modularity, niche construction, contingency, and ecology (e.g., Pigliucci & Müller, 2010). Although this is an important collection of topics and approaches, I think it is fair to say that the aforementioned do not and arguably cannot provide a metatheoretical alternative to the MS because they hold few principles and commitments in common. This is not surprising if the purpose of the EES is to serve as criticism of the MS rather than to serve as a general alternative framework. It is important as well that we subject neo-Darwinism – and for that matter any explanatory framework with broad utility – to due scrutiny.

If we let go of the idea that the EES, and perhaps more to the point of Packer and Cole, NCT and DST, can serve as an alternative evolutionary metatheory, things get quite a bit simpler. I am personally persuaded by Sterelny's (2007) suggestion that developmental systems ideas, within which he nests NCT, are better thought of as a human evolutionary theory rather than a general evolutionary theory, and, if so, a fortiori cannot be an alternative to standard evolutionary theory in some broader sense. As such, Packer and Cole need not have taken on the implicit burden that evolutionary processes *in general* involve an active organism changing the process of adaptation and selection, or that evolutionary systems analyses can be usefully applied at phylogenetically remote levels of biological organization. I also agree with Sterelny's (2007, p. 181) claim that "Human niche construction increases both the instability and diversity of human environments." This is one of the reasons why cultural evolutionary theorists and human behavioral ecologists believe that the human mind is selected for a large amount of phenotypic plasticity as an adaptive response to unpredictable and nonuniform environments. Also, as put by Fine, Dupré, and Joel (2017, p. 668), "The more nearly the environmental factors are sufficient for the development of a trait, the more the ability to quickly acquire or learn adaptive behaviors will be genetically selected for and inherited, rather than the behaviors themselves." So far, so good.

Unlike Pigliucci and Müller (2010), Sterelny (2007) and Fine et al. (2017) are advancing specific evolutionary arguments in an attempt to clarify evolutionary explanation and do not get bogged down in defending the adequacy of the EES as an alternative metatheoretical framework. Although Packer and Cole do assume and claim to build on an extended alternative, their points mostly speak for themselves. To my mind, the debate that Packer and Cole briefly allude to between Laland et al. (2014) and Wray et al. (2014) shows that the standard versus extended evolutionary debate largely turns on definitional grounds and that the two "sides" mostly agree on the empirical and theoretical adequacy of many of the common concepts employed; the issue, again, is whether phenomena like niche construction or the need for a developmental systems level of analysis are sufficiently general that NCT and DST should take their rightful place among Mendelian genetics and Darwinian selection. Framed this way, it would seem that the at least provisional answer should be no; niche construction might be fairly common but is certainly not general across species and a developmental systems take on the evolution of all species, or even e.g. human Huntington's or sickle cell disease, seems like overreach. However, and again, *human* evolution and development are arguably generally well conceptualized through NCT and DST. Of course, it is probably still going to matter which aspect of phenotypic development in which we are interested and whether evolutionary considerations are, strictly speaking, relevant in a particular case. For, as far as I can tell, although development is usually fruitfully conceptualized as occurring within various sorts of systems, it is not as obvious, and certainly not nearly as widely agreed upon, that a systems level of analysis is obligatory in human evolutionary studies.

Packer and Cole on the Evolutionary Function of Institutions

Packer and Cole review Boyd and Richerson's CET and Tomasello and colleagues' SIT to set up and defend their view of the importance of institutions in human evolution and development. For Packer and Cole, what is unique about human

institutions, including what they consider to be the first such one of kinship, is their deontic nature; they introduce the concept of a “deontic niche” to underscore this point. They argue, in turn, that the deontic nature of human institutions is critical because human agents are bound by, for example, obligations to family that are not mere matters of convention in the way that they are envisioned, for example, in CET. In addition, Packer and Cole claim that human institutions bind obligations over many time scales from the distant past to the momentary present to the far future; as such, human institutions themselves have the potential to guide evolution. Finally, and furthermore, the human capacity for self-consciousness is tied up with and emerges through the deontic niche; human social cognitive development therefore occurs within human institutions.

Although Packer and Cole criticize Tomasello’s maturational view of the development of the capacity for shared and collective intentionality, as they note, Tomasello’s position is that the capacity seems to develop without explicit teaching or learning. One could, and arguably should, give some pause as to whether absence of explicit learning is a sufficient criterion for a skill to be seen as biologically prepared (i.e., in some sense innate). Tomasello’s view is also manifestly *not* a developmental systems perspective. I would imagine though that Michael Tomasello, like the evolutionary psychologists he sometimes criticizes, might be left scratching his head why his extensive empirical research attempting to test his theory is given short shrift. In either case, I find myself agreeing with Packer and Cole in their critique of Tomasello’s SIT, but for somewhat different reasons; for me, it is because Tomasello’s explanation is not actually developmentally very useful. To wit: based on observational and experimental studies of human and nonhuman primates, why and how did human *x* develop capacity *y*? Answer: maturation. It reminds me of using, for example, habituation studies, studies of which Tomasello and colleagues have been critical (e.g., Buttelmann, Carpenter, & Tomasello, 2009; see also Tafreshi, Thompson, & Racine, 2014) to support core knowledge claims. To be clear, both things might be the case, but they tell us very little about development. To put it differently, although they might seem to be, they are not really developmental explanations (see Lehrman, 1953, as cited in Packer & Cole, this issue).

Here, then, is a concern. If Tomasello’s is not, strictly speaking, a developmental account because it pulls out a single explanatory factor from a potentially complex set of influences, then should we not hold Packer and Cole to the same standard? After all, it is not obvious that the kin qua first and critical institution could hold up to the same scrutiny. Although it is true that a newborn human infant is thrown into a social life that other primates are not, to claim that “even the human newborn is *already* participating (with help from caregivers, of course) in collaborative activities” (Packer & Cole, this issue, original emphasis) does not seem very different to my eyes from Tomasello’s maturational account. In addition, I would suspect that Tomasello is likely to continue to wonder how it is that human infants could possess a capacity for shared or collective intentions that eludes our great ape cousins, for it is fairly clear that raising a chimpanzee as a human infant has diminishing returns. There is also a potentially important distinction to make between acting in accord with a rule and following a rule that speaks to the role of intention. In summary, although Packer and Cole’s argument works very well as a critique of other approaches and offers a plausible evolutionary analysis of the potential importance of the role of institutions in human evolution, it is not clear to me that

singling out the evolution of a deontic niche does an adequate job of explaining why and how human ontogenesis occurs. Additionally, to the extent that it does, it might be seen to do so at the expense of its evolutionary and developmental systems roots.

Final Considerations

It is easier to be a critic than to create useful models of human evolutionary and developmental processes, and I thank Packer and Cole for their interesting article; I expect it will not surprise them that not everyone will consider it the final word on the matter. What I suspect as well is that it might be easier to be a theoretician than it might be to create explanations that do justice to the frequent complexity of the natural world. However, the history of science contains many examples of simplified models that helped to move the process of theory construction and scientific discovery along; I expect that Packer and Cole's will likely be one of them. One would be hard pressed though to find a better recent example of a successful simple model than the MS itself. It is remarkable that roughly 100 years ago, Darwinians and Mendelians were at odds with each other and that a reconciliation would be slowly made possible some 20 years later through the synthesis of Darwinian selection and Mendelian genetics. It is also humbling to bear in mind that although neo-Darwinism and, more recently, its potential limitations are obvious to contemporary academics, biologists did not see it coming in the 1910s and 1920s.

It might be tempting to believe, and indeed I have heard the argument, that the EES parallels the state of pre-synthetic evolutionary biology. Although it seems clear that evo-devo can be thought of as something like a separate evolutionary paradigm, I think that the similarities end there; and, as far as I can tell, something else is going on when it comes to the advocacy for extended evolutionary theories. The bitter disagreements in the 1970s between E.O. Wilson, Stephen Jay Gould, and Richard Lewontin provide one way to frame the issue. Whatever side one takes in the "sociobiology wars," there seemed to be more at stake than the adequacy of sociobiological methods and their application to humans; issues of our sociobiological constitution are often controversial and also speak to fundamental concerns about what it is to be human, the nature and extent of free will, and the meaning of life more generally. One would be hard pressed to find anyone that better represented the extended-standard dialectic than Gould and Lewontin versus Wilson, and I often wonder whether more recent extended versus standard evolutionary theories and debates track something about these sorts of existential concerns. It is interesting to me as well that someone like Tomasello who has done more than most to show the importance of our collective lives for human development is, and I think accurately, understood by many contemporary developmentalists to be on the more standard side of things. I suppose this is progress.

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