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Response to Commentaries

The pendulum still swings

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> "Psychologists do not have much time for thinking about representation". (Gentner, 2010, p. 331)

We would like to thank the commentators for their time and thoughtfulness — the commentaries are, in general, engaging and informative. Interestingly, most of the discussion has to do with the nature of representation, not with our basic critique of nativist infant research. Regarding the latter, there seems to be general agreement. Regarding representation, however, there seems to be general disagreement.

This disagreement extends to basic notions of what constitutes representation — what its ontology is. In the target article, we presented an outline of an alternative, interactivist model of representation in order to show that it is possible to avoid the problems of foundationalism — that it is possible to account for the emergence of representation. Some of the commentaries, however, proceed on the basis of background — or foreground — assumptions about the nature of representation that we have argued are fundamentally flawed, or that don't seem to us to be relevant to either our critique or our alternative model. Nevertheless, there are some important *con*vergences concerning representation as well as concerning the nativist-empiricist debate.

The fundamental characteristic of representation is that it is truth-valued aboutness for the organism itself, with the special criterion of organism-detectable representational error. Only with organism-detectable representational error is the normativity of representation inherent in the organism, not just in the eye of an observer, but also only with such detectable error is error-guided behavior and learning (and development) possible. This central criterion, we argue, can only be met by an anticipatory, action-based model, at least roughly similar to Piaget's; and such a model enables the transcendence of the nativist-empiricist pendulum because it permits an account of representational emergence.

We know that representational emergence has to be possible: Representation did not exist 13 billion years ago, and it does now, so any model that cannot account for such emergence must be

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incorrect. The interactivist model, thus, satisfies multiple criteria of representation. The model, however, does not directly address some of the issues mentioned in the commentaries, such as fleeting vs stable, short term vs long term, presentational vs representational, environmental interaction vs internal interaction, perceptual vs non-perceptual, early vs late, and so on. Some of these can be of interest and importance from some perspectives, but are not relevant, we argue, to basic issues of the ontology of representation, and thus, not to foundationalism.

We have drawn from each commentary one or two of the issues that we argue are central to any adequate framework for developmental psychology. Addressing these issues will in general be in three parts: (1) possible convergences, (2) disagreements and confusions, and (3) pursuit of further topics.

1. Dynamicism

1.1. Representational knowledge and embodiment

The interactivist model is, as Clearfield (this issue) notes, a kind of dynamic systems model. Clearly we are in strong general agreement concerning the necessity of this framework (there is also a strong convergence in the rejection of the standard competence–performance distinction — Bickhard, 1980). Nevertheless, we argue that the interactivist model encompasses additional properties that are necessary to account for cognition and development: (1) interaction, not just organism dynamics per se; (2) thermodynamics and thermodynamic relationships between organism and environment; and (3) truth-valued normativity. The interactivist model is also in agreement that timing is of central importance, though with an emphasis on coordinative timing (e.g., Bickhard & Richie, 1983, p. 90), not continuity of timing per se. Nevertheless, (inter)action is at the core of both DST and the interactivist model.

Clearfield, however, raises two major concerns: (1) the representational status of knowledge; and, (2) the fundamental embodiment of all cognition. The following excerpt captures both:

> More importantly, I think that the strength of DST is precisely that it is not classically representational, that it never becomes so. Making knowledge representational, even if it begins from non-cognitive processes, eventually divorces it from the non-cognitive processes that created it. This is what the Interactivist approach proposes, that knowledge is built through action but somehow becomes abstract, and separate from the perception–action loops that made it. Knowledge, according to DST, is never disembodied and it is never static. It is never divorced from the myriad of interacting processes that created it. Indeed, it is truly an action-based approach. (this issue, p. 137)

Clearfield argues that it is a strength of DST that it is never representational, but, then, what about learning and error guided behavior? Can they occur without representational error, and, thus, without representation? What about anticipation? Is that not necessary in any complex agent? Is that not truth valued? And if so, isn't it, therefore, representational?

Clearfield seems to assume that when we talk about representation we are talking about classic notions of static encoded symbols and that we are trying to provide an account for the emergence of these classic representations out of a foundation that is not itself already representational. While such a reading may indicate insufficient clarity on our part, we couldn't disagree more. The encodingist critique argues that classic notions of the nature of representation make emergence impossible and therefore that they are false as a model of representation. However, in contrast to DST, we do not reject the notion of representation altogether, but rather, argue for a non-classic notion of representation — an action-based model of representation. In this sense we agree with the criticisms of classic conceptions of representation (including those of DST), but we would suggest that to reject representation altogether on the basis of those criticisms is akin to fatally critiquing phlogiston, and concluding that therefore fire does not exist. Our argument is that fire does exist and therefore we need a new way to think about it that does not appeal to the classical notion of phlogiston.

Our general analysis of DST was that, without an action-based model of representation, it remains incomplete in a particularly important respect. Specifically, DST alone does not have adequate theoretical resources to deal with two relevant epistemological properties that any approach to developmental psychology will eventually need to address: truth value and aboutness. We call the anticipatory model of these properties a model of representation (albeit, a non-classical, non-symbolic, non-static, non-encoding, non-correspondence version). But regardless of whether the term "representation" remains useful or not, there is still the need to account for these two basic epistemological properties. While truth value enables the possibility of system-detectable error and therefore error-guided behavior and learning, aboutness means that the content of a system's epistemological processes is about the world. In short, knowledge can be true or false and knowledge is about the world. *Classic* notions of representation require an *interpreter* to supply both truth value and aboutness. In contrast, interactivism provides an account of the emergence of these epistemological properties that is intrinsic to the nature of particular kinds of dynamic systems that are Far From Thermodynamic Equilibrium (FFE).

To keep the discussion brief we will only just highlight the progression of emergent complexities that end with interactivism's anticipatory model of representation (i.e., truth-value and aboutness). To begin, consider that there is a fundamental asymmetry between systems that are FFE (e.g., a chemical bath) and those that are not (e.g., a rock). The former require maintenance of their *far* from equilibrium condition (thus, interaction with their environment) in order to exist while the latter are perfectly content to remain *at* equilibrium for cosmological time. This asymmetry yields the fundamental *normative* ground for all subsequent emergent complexities. Now, consider FFE systems that are able to contribute to their own maintenance — self-maintenant systems (e.g., a candle flame). A self-maintenant system contributes to its own stability as a process that is FFE and therefore those contributions are *functional* relative to the stability of the system that is being maintained. In other words, normative function is emergent in self-maintenant systems.

However, self-maintenant systems are going to be limited to relatively stable environments (e.g., if someone opens the window, then the candle flame will go out and cease to exist). Therefore, more robust systems will need to be able to alter how they contribute to their stability as self-maintaining systems in ways that are appropriate to changes in environmental conditions. These systems will be *recursively* self-maintenant — they will be systems that maintain their self-maintenance. The canonical example of a recursively self-maintenant system is an idealized bacterium (Bickhard, 2009a; Campbell, 1974). In an effort to contribute to its self-maintenance, the bacterium will swim if it detects that it is oriented up a sugar gradient and it will tumble if it detects that it is oriented down a sugar gradient.

Consider now a more sophisticated recursively self-maintenant system — a frog. A frog will typically have multiple potential interactions that are indicated as available given the detection of appropriate environmental conditions — perhaps several potentialities for tongue flicking and eating (e.g., flies and worms). "Importantly, the possibilities indicated for the frog will implicitly predicate that the current environment is functionally appropriate to those possibilities, and those predicated environmental properties can hold or not hold — that is, the predication is *about* the environment and can be *true* or *false*" (Allen & Bickhard, 2011b, p. 109). As discussed in the target article (this issue, p. 127), anticipation is at the core of the interactivist model of representation because it manifests the two relevant epistemic properties — truth value and aboutness. From the interactivist perspective, any internal functioning of the system that involves anticipation is intrinsically representational; however, part of the reason for presenting the interactivist model of object representation in the target article was to demonstrate that anticipation is sufficient to account for more robust and commonly recognizable representational phenomenon.

The second major concern from Clearfield's commentary involves the embodiment of cognition. Interactivism's account of the cascade of phylogenetic and ontogenetic emergent properties and aspects of FFE systems continues well beyond object representation to include cognitive, learning, and developmental processes in general as well as providing models of perception, language, personhood, and so on (Bickhard, 2006, 2009b, in preparation). However, framing and grounding these qualitatively emergent properties and processes, interactivism is committed to embodiment in the strongest possible sense. That is, for interactivism, there is both emergent discontinuity as well as normative

continuity running throughout all forms and types of cognition and development that are ultimately rooted in the existential necessity of FFE systems to exchange energy with their environment. For any system that might be considered cognitive in even the most rudimentary sense (e.g., the bacterium, frog, ape, etc.) the exchange of energy involves interactions. Therefore, from the interactivist perspective, interaction is necessary for *any* form of cognition and cognition is therefore embodied in the strongest possible sense — an existential sense (for an elaboration on the necessity of embodiment for cognition, see Bickhard, 2008).

1.2. Actions as the unit of analysis

As with Clearfield, we are in strong agreement with the general DST framework advocated by van Geert and Steenbeek (this issue), but the convergences, at least in one respect, run deeper than they did for Clearfield. In part, this difference is a consequence of the different brands of DST that have been developed (van Geert & Steenbeek, 2005), and, in part, it is probably a consequence of van Geert and Steenbeek's phenomenological orientation. Van Geert and Steenbeek appear open to higher-level psychological constructs in a way that Clearfield is not and so in that sense are closer to our own approach in that we both acknowledge the need to consider these higher-level constructs. However, we are committed to models that account for the ontological emergence of the phenomena that such constructs refer to, an issue that van Geert and Steenbeek seem to be agnostic about (van Geert & Steenbeek, 2005; Witherington, 2011). Thus, they see presentation as primary for the study of developmental psychology but we would like to understand the emergent nature of 'presentations' — of phenomenology. Presentation is just as representational in terms of truth value and aboutness as re-presentation.

There is also very strong convergence in terms of their efforts to explore some of the methodological implication of their particular brand of a generally action-based approach. Along these lines, they raise a more general methodological issue concerning the relevance of action as the theoretically meaningful unit of analysis for developmental research. Specifically, they ask:

> if the alternative approach described by Allen and Bickhard is action-based, then what is the basis of the approach that leads to foundationalism? We think that the latter is an approach that does not focus on actions as the unit of analysis, but on the psychological states as the unit of analysis. That is, if one's focus of psychological research is on the properties of psychological states such as to know (something), to believe (something), to see (something), and so forth, one tends to isolate these psychological states from the context of activity in which they emerge and from which they borrow their changing and context-specific meaning. (this issue, p. 141)

While we agree that ignoring action as a unit of analysis is consistent with foundationalism, we do not believe that it is the basis for it. Some version of foundationalism is necessitated by the fact that encoding notions of representation cannot account for their own emergence. At this level of analysis, ignoring action as a locus for developmental research is, ultimately, a consequence of the underlying encodingism. Encodings are context-independent representations with no relevant embodiment or intrinsic dynamics. However, there are several more proximal reasons why some researchers disregard action as a unit of analysis that include: (1) the functionalism of the information processing framework; (2) the abstraction away from particular situations and activity in the folk psychology framework (as noted by van Geert and Steenbeek); and (3) the ontological split between competence and performance in the nativist framework.

Further, there are also multiple reasons why various researchers *do* consider action as a theoretically relevant unit of analysis for the study of developmental psychology. Frameworks that are focused on development as a process as well as any generally systems-based orientations for thinking about the mind will entail, imply, or at a minimum, enable action to be constitutive of cognition. Thus, a major point argued for in the target article was not just the familiar message that theory guides methodology, but that it can *mis*guide methodology, and it seems that van Geert and Steenbeek raise another important sense in which theory has misguided researchers away from action as an essential unit of analysis.

2. Empiricism

2.1. Emergence and the nature of interaction

Our major point of convergence with Haith (this issue) concerns the need to develop more sophisticated and nuanced conceptual resources for thinking about the processes that constitute different types and forms of knowing. As part of their critique of infant research, Haith and Benson (1998) highlighted the need to differentiate between changes in a form of energy (sensory encoding) and more canonical forms of knowing (symbolic representation). More broadly, they argued that nativism's failure to adequately attend to the differences among forms of knowing manifests methodologically in terms of rich interpretations of looking procedures. We made the same general argument in the target article but claimed that the nativist's problem was their foundationalism which was itself necessitated by an underlying notion of representation as encoding (more on this with Moore).

Much of Haith's concern seems to revolve around two related issues: (1) the theoretical relevance of emergence; and (2) the nature and function(s) of interaction. With respect to the former, Haith comments as follows:

> Disregard, for the moment, that there is no limit to the age range or domain of this claim; in fact, repeatedly, throughout the paper, the claim is made that "foundationalism" precludes the emergence of new representations or knowledge.
> First, even if original representations are innate (whatever that means) or some other form of foundationalism exists at birth, why in principle, would this assumption necessarily preclude emergent representations later that depend on organism/environment interaction? (this issue, p. 145)

First, to answer Haith's concern about why no age ranges are provided throughout the target article — an ontological model of the nature of representation should apply to *all* representation, independent of kind and independent of age of the individual in which it exists. When we discussed the different types of FFE systems that involved increasing degrees of complexity, there was no need to specify any particular example, however helpful the examples were for illustrative purposes. Similarly, when discussing a possible ontology for the nature of representation throughout ontogenetic development, there is no need to specify any particular age or domain at which the child is thought to instantiate any particular type of representational complexity. What matters is the developmental progression of precursors, prerequisites, and emergent complexities *within* the basic ontology of representation. An ontological model of child development will need to manifest in the actual world in terms of age ranges and domains, but the model itself is one of possibility — arguments about how representation is possible are ontological arguments, and basic representational ontology is not age specific.

Thus our in-principle concern with emergence is not just about origins per se, but rather, how it imposes a theoretical constraint on any purported model of representation that assumes naturalism. In particular, the argument is that *the encoding nature of extant models of representation makes the emergence of representation impossible*. As a consequence, any "new" representations later in development must, at best, be combinatorially constructed out of a pre-given (i.e., innate) set or base of representational atoms (foundationalism). However, genuinely new representations *do* exist, whether in evolution or development or both, and, therefore, the fundamental nature of representation *cannot* be that of encodings. Thus, a successful account of representational emergence, whether early in development or later in development, requires that you have moved away from an encoding notion of representation, and if that is the case, then there is no longer any necessity to assume a foundationalism in the first place (Allen & Bickhard, 2011c).

Finally, as a challenge to our claim about the *essential* relevance of emergence for developmental methodology Haith states that:

> Astronomers and physicists have made a great deal of progress in describing the nature and character of the evolving universe without understanding the specifics of matter in the first nanoseconds after the big bang or, for that matter, how energy and matter came to be. (this issue, p. 145)

However, not having a model of (emergent) origins is not the same as having a model that makes origins impossible. So we agree with Haith that physicists do not need an account of the big bang in order to do physics but our point is that you *do* need to avoid frameworks that would make the emergent origins of the big bang impossible. Avoiding models that make emergence impossible is just as important for astronomers and physicists as it is for psychologists.

The second major focus of Haith's commentary involves the nature and function(s) of interaction.

> Second, since no age range is offered for this analysis, one must suppose that emergent representation, . . ., must apply throughout life. Yet many examples come to mind for which "competent interaction", in any meaningful sense, is missing — unless any mind involvement qualifies as "interaction", in which case the term becomes meaningless; who claims otherwise? (this issue, p. 145)

There are two basic approaches to the relevance of (inter)action for knowledge and cognition. These are captured by the fundamental contrast between (inter)action as *instrumental for* knowledge and (inter)action as *constitutive of* knowledge. While foundationalist approaches accept the former, any substantive action-based approach must also be advocating for the latter. The interactivist model of representation provides a *particular* action-based account of how (inter)action is constitutive of knowledge. Specifically, representation is constituted by anticipation — functional indications of interactive potentialities. While the clearest examples of interaction in this model might involve the body and its external physical environment, this is not the only form that interaction can take. As Piaget pointed out, higher order thought is *internal* interaction. Further, social interaction constitutes its own emergent realm of representational complexity that is importantly different from that of interacting with pebbles (Bickhard, 2012). Finally, neuroscience has made clear that the CNS is continuously interacting with itself throughout development (i.e., certain regions modulate the activity of others).

This way of thinking about (inter)action is very different from one in which (inter)action is instrumental to knowledge — e.g., where access to the environment enables the triggering of representational contents whether of full objects (nativism) or the features of objects (empiricism). From this triggering perspective, Haith is correct that there is little sense why children would require interaction (beyond its triggering role) in order to know about the world; however, that is not what the interactivist model claims or is committed to. So, to restate the point, the concerns addressed in this critique *are* independent of age, and (indications of) interaction potentialities are constitutive of representation, not just instrumental toward representation.

2.2. Encodingism and the necessity of action

In terms of broader frameworks, Moore's excellent book, *The Dependent Gene*, is part of a general systems oriented approach to understanding biological and developmental processes — evolutionary development (Moore, 2002; Oyama, Griffiths, & Gray, 2001). Central to any systems approach is the notion of interaction. Interactions are recognized as taking place across multiple levels and across multiple time-scales. Further, because timing is essential for these interactive processes, systems approaches are intrinsically temporal. The interactive and temporal core of systems approaches means that they are broadly convergent with the interactivist perspective. However, a problem for any systems approach that is applied to developmental psychology is to address how the general framework can be used to model representation and cognition in particular. We have argued that the interactivist model of representation is epistemologically robust while fully compatible with a broader systems orientation.

Much of Moore's response to the target article (this issue) seems to address some of the fatal consequences of encoding notions of representation, and consequently, of the necessity of interactive indications as constitutive of knowing. This tension is to be expected given that encoding notions of representation are perhaps most evident within the atemporal and disembodied functionalism of the information-processing framework — a framework that Moore seems to accept in aspects of his own work.

The interactivist approach argues that *one* of the consequences of encoding notions of representation is that they necessarily conflate the distinction between epistemic contact and epistemic content (Bickhard, 2009b). Within an encoding framework, we agree with Moore that there is indeed "a good reason for this conflation" (this issue, p. 150) — it is necessary to the ontology of encodings. Specifically, this conflation derives from the requirement that encodings represent in virtue of some sort of correspondence relationship, and typically, contact is taken as constituting that special representational correspondence, thus of content as well. The target article then argued that this conflation was part of what we consider to be the theoretical basis behind the methodological issues raised by Haith and Benson (1998) concerning rich interpretations.

In contrast, Moore seems to equate the distinction between contact and content with that between perception and cognition (false); claims that a distinction between the latter two cannot be strictly maintained (we agree that this is true); and then concludes that neither can the former distinction be maintained (this would be a non-sequitur because the two distinctions are not the same). The conflation between contact and content is *one* of the consequences of encoding models of representation; however, the interactivist critique of encodingism is much broader than just that distinction (Bickhard, 2009b).

This brings us to the most general concern that Moore finds so problematic — that encoding models of representation are fundamentally mistaken about the nature of representation. As mentioned in the target article, even Fodor (Chomsky & Fodor, 1980) acknowledges that his nativist conclusions are probably an indication that we are fundamentally mistaken about the nature of representation and learning. Moore's justification for dismissing the encodingist critique is that: (1) encoding notions of representation are too useful and central to cognitive science to be rejected; and, (2) that because we take the problems with encoding notions seriously we are "rejecting all modern theories of psychology" (this issue, p. 150). The first part of Moore's concern is only pertinent if there are no alternatives and the second is over-stated.

Regarding (1), we regret that Moore finds the scope of the encodingism critiques so problematic — after all, it's not the first time that a conceptual error permeated a field — but his skepticism is not a refutation, nor even an attempted refutation, of the arguments involved. Ptolemaic astronomy, phlogiston, caloric, Newtonian physics, behaviorism, and the genome-as-blueprint model have all been very useful and very central to the sciences of their day, but utility and prevalence are not arguments against extant incoherencies. Further, all of these historical approaches were supplanted after their anomalies proved intractable and adequate alternatives were sufficiently well developed (the transcendence of the genome-as-blueprint model is, by some accounts, still underway).

Regarding (2), we are certainly not alone in arguing that there are serious problems with information-processing notions of representation that are at the core of various theories in cognitive science and psychology. The interactivist critique is broader than others and is able to integrate many of the previously explicated problems as properties or consequences of the nature of encodings, but it is far from the only critique on offer. The target article is an exploration of several of the specific implications of encodingism for different aspects of nativism and empiricism. Importantly, nothing about the principles of evolutionary development, dynamic system theory, neuroconstructivism, and other systems-oriented approaches *requires* them to adopt an encoding notion of representation. On the contrary, encoding notions of representation are antithetical to the core of a systems orientation and that is why we claim that the interactivist model would be an appropriate potential complement for any systems approach — including evolutionary development.

While Moore agrees with our concern that nativist approaches are problematic in terms of their lack of a developmental focus, he does not agree that an action-based approach to representation is also necessary. However, the two are intimately related. If there is no in-principle reason why arguments for nativism make the emergence of an innate representational foundation *impossible*, then arguments that criticize nativist approaches for being non-developmental have insufficient bite. That is, in that case, the nativist response that accounting for the emergence of the innate foundation is simply an empirical matter, and is outside the domain of developmental psychology, seems perfectly reasonable. A major purpose of the target article was to explicate why the inability of nativist approaches to account for the emergence of representation is fundamentally incoherent and problematic in ways that can only be overcome by an action-based approach.

3. Constructivism

3.1. Developmental trajectories including the brain

As with the general systems orientation of both brands of DST and of evolutionary development, neuroconstructivism is broadly convergent with the interactivist approach. In contrast to the other commentators, Karmiloff-Smith (this issue) introduces some additional considerations of developmental complexity that are highlighted by her advocacy for "taking developmental trajectories seriously" (this issue, p. 154).

A commitment to different developmental trajectories implies a space of possibility. Consequently, the study of development becomes the study of characterizing the properties and constraints on that space (Bickhard, 1980, 2006). Specifically, taking developmental trajectories seriously means incorporating the historicities of learning into theory and research. Historicities can be understood broadly as how the properties and constraints of old learning influence new learning and is manifest most strongly in systems that are capable of recursive learning. Recursive learning involves constructive processes in which previous learning can be used as a resource for future learning, and such recursivity manifests most powerfully for an action-based approach (Allen & Bickhard, 2010, 2011a).

An action-based approach to representation removes any temptation to think that knowledge — competent interaction systems — could be impressed into a passive mind. Accordingly, knowledge must be generated by a constructivist process and, without prescience, it must be a variation and selection constructivist process (Bickhard, 2006). Therefore, an action-based model forces a (variation and selection) constructivism and it is the constructivism that enables the possibility of recursive learning (Allen & Bickhard, 2010, 2011a). Furthermore, in another strong convergence with Karmiloff-Smith, a recursively constructive process (making use of previous constructions either as units of construction or as loci for variations in a variation and selection process) will manifest progressive domain specificity in its learning and developmental capacities (Campbell & Bickhard, 1992; Karmiloff-Smith, 1992).

Finally, we are also strongly in agreement with Karmiloff-Smith's emphasis on the developmental importance of continued growth and development of the brain. One of the pernicious heritages of computationalism is the continued background assumption of the hard dichotomy between the brain and its mental functioning — the distinction between hardware and software, in which the mind is taken to be the software of (being executed in) the brain. For a computer, this is an acceptable first approximation because the stabilities of the circuitry are due to high energy levels of atomic and molecular structures, while the execution of a program is constituted in orders of magnitude lower energy level processes of flows of electrons in these fixed, stable circuits. In such models of development, the brain is the fixed circuitry and the mind is the processes that take place within that circuitry.

Such a background conceptual framework is misleading in at least two senses: (1) It is not the case that the brain ceases developing, and then psychological development occurs within it. Instead, the brain manifests multiple kinds and variations of kinds of plasticity throughout development, and these plasticities are themselves part of the framework within and upon which psychological development occurs. And (2) biological processes, including brain processes, exhibit whatever stability they have as steady states relative to many different time scales of processes, not as energy well structures (such as soldered wires), and experience modulates those steady state organizations and the processes with respect to which they are relative steady states, rather than inserting inputs into them as fixed circuits. There is no hard boundary between brain development and psychological development.

3.2. Social cognition

While sympathetic to the target article, Lewis, Carpendale, and Stack (this issue) raise a basic yet powerful question when they "wonder why it is still necessary after all these years [to raise such problems with nativism/empiricism]" (this issue, p. 159). In short, we agree — except that the inherent perplexities of foundationalism may be at least a partial explanation: unless and until those are transcended, the pendulum is inevitable.

Further, the very legitimacy of these sorts of theoretical issues is not typically supported as being part of psychology as an experimental science. That is, fully avoiding the problems of foundationalism requires a type of analysis that is not typically accepted as a legitimate part of the science of psychology in the first place. Therefore, a major purpose of the target article was as a demonstration of why psychology as an experimental science needs to take seriously what are considered to be "theoretical" or "philosophical" issues. Finally, and perhaps most importantly, psychology is a social institution and to present all of Gentner's quote from the first page: "Psychologists do not have much time for thinking about representation — the reward structure in our field dictates a steady flow of experiments" (p. 331). Along these lines, nativist methodology has been, and continues to be, extremely productive for producing experiments.

Lewis, Carpendale, and Stack focus their commentary on the latest implementation of nativist methodology for social cognition in general and for false-belief competence in particular. If the scope of the critique from the target article is correct, then this latest application of nativist methodology to false-belief "understanding" should also be problematic. However, as Lewis, Carpendale, and Stack state: "These false belief tasks may not simply be explained away in the ways that A&B critique the drawbridge study assessing infants' object 'knowledge'" (this issue, p. 160). The failure of object and number research to control for perceptual level processes does not seem to apply in any straightforward way to these social-cognition experiments. While the false-belief cycle of research initiated by Onishi and Baillargeon (2005) involves very different experimental situations, it maintains the same basic logic of violation of expectancy, competence–performance, foundationalism, encodingism, passive perception, and as Lewis, Carpendale, and Stack point out, additional assumptions about the metaphysics of meaningful action.

Social cognition research in general and false-belief research in particular is dominated by folk psychology. Folk psychology is a mentalistic framework for understanding how we navigate our social environment. Specifically, the attribution of mentality to the activity of other agents is the purported means by which we explain and predict their actions (a.k.a. folk psychological Theory of Mind, Mindreading, or Mentalism). Thus, the assumption that *meaningful* action is defined by folk psychological mentality forces a wedge between surface behavior present in the physical *movement* of all objects (what is available to perception) and a deeper psychological level present in the intentional *actions* of agents (what must be inferred through the prior knowledge of the mind). Accordingly, this assumption forms the basis for research design and methodology in the domain of social-cognition. Specifically, researchers attempt to demonstrate that infants and toddlers do not respond to mere surface behavior but rather to meaningful (thus mentalistic) action.

Lewis, Carpendale, and Stack have already highlighted the implications of this split for infant false-belief procedures, but it applies to other paradigms as well. For example, imitation research purports to demonstrate that 18-month-olds understand mental intentions by demonstrating that they will imitate the intended actions of an adult model rather than their "literal" body movements (Meltzoff, 1995;[1] Carpenter, Akhtar, & Tomasello, 1998). That is, under certain conditions and at certain ages, children will prefer to imitate the intentional actions of an adult model. However, the conclusion that these types of results demonstrate that the child is mindreading the mental intention of the adult only follows from an assumption that the only way to understand action as intentional is through the attribution of mental states (i.e., mentalism/mindreading).

This assumption derives from the taken-for-granted split between surface behavior and underlying mentality, and the presumed exhaustiveness of this split is demonstrably false. There are alternative non-mentalistic accounts of how the "meaningfulness" of intentional action can be part of what is "directly" perceived in the activity of other agents (Byrne, 1999; Gergely & Csibra, 2003; Marken, 1982; Wilkerson, 1999). However, mentalist researchers tend not to include control conditions for these more interesting non-mentalistic alternatives. Instead, they address alternatives based on the

[1] Interestingly, when older children were tested with the same procedure used by Meltzoff, they did in fact imitate the "literal" actions of the adult model (Huang, Heyes, & Charman, 2006). Additional research seems to indicate that children from the original study were acting on the basis of the object affordances not the mental intentions of the adult model (Huang, Heyes, & Charman, 2002).

taken-for-granted split between surface behavior and underlying mentality and argue by elimination that if they rule out the former, then they have demonstrated the latter. Accordingly, the theoretically motivated absence of such relevant control conditions *does* mirror the case for object and number cognition.

Also as noted by Lewis, Carpendale, and Stack, an action-based approach does not assume that there is a fundamental split between mind and body. Consequently, there is never any inclination to assume that meaning has to be added to our perceptions of the world. Meaning is constituted in the (inter)activity itself and this applies to agents as much as it applies to objects. For interactivism, learning about agents is fundamentally the same as learning about objects; however, there is an additional layer of complexity. The interactive possibilities afforded by other agents is going to depend on the situation. For example, the interactive possibilities afforded during a soccer game are very different than those of a board meeting, even if the same people were involved. Therefore, social cognition is going to involve learning about different types of situations. Further, if our interactions with other agents are going to be coordinated, we need to have a mutual understanding of what type of situation is involved — Bickhard (1980, 2006, 2009b) has termed these mutual understandings *situation conventions*. For interactivism, situation conventions form the basis of navigating our social environment and offers a robust alternative to the mentalism/mindreading approaches of the folk psychology framework.

4. Conclusion

The commentaries demonstrate nicely that although we are all in agreement that nativist research has a number of problems, we are not at all united in terms of what they are, and consequently, what to do about it. The varying levels of convergence between the different authors and ourselves is directly related to the nature of the broader frameworks involved. For those systems oriented researchers we find general convergence while for those of more classical formulations we find general divergence. Regardless, we have highlighted some basic issues that any approach to human development will need to consider about the nature of representation, learning, and development.

Since writing the target article, it has been pointed out to us that nativist number researchers have responded to the prior push of the pendulum *away* from nativistic thought with a new cycle of experimental results that seem to *support* nativism. However, if the real problems with this research are theoretical in nature, then new experiments will only be able to push the pendulum one way or the other. From our perspective, this most recent cycle of results indicates that nativism hasn't learned from its mistakes so much as it has learned to become more sophisticated about making them. It is still swinging on the pendulum.

There are two central levels of analysis in the critique that we have offered. At one level, the main point is that nativist (and empiricist) studies of infant and child development have been systematically distorted by methodological consequences of background assumptions concerning the nature of representation — in particular, assumptions that force a foundationalism, and, thus, focus experiments on issues concerning the nature of and the scope of such an innate foundation. Consequently, they systematically ignore methodologies that might test against non-foundationalist, non-encoding, models of representation, cognition, and development.

At a broader level, this critique illustrates the general point that theory not only guides experiment, but theory, and theoretical frameworks, can *mis*guide experiment as well. If theory were to necessarily grow up out of data, as the naïve (Machean) inductivist philosophy of science that dominates psychology would hold, then this point would not make sense. But theory does *not* emerge from patterns of data (Bickhard, 1992); psychology needs to take much more seriously than it does issues of theory and of the ontologies that underlie theory.

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