Introduction

A number of recent psychological proposals have attempted to reconcile the history of principled limitations inherent to nativist and empiricist positions (Elman, et al., 1996; Thelen & Smith, 1994; Karmiloff-Smith, 1992; Newcomb, 1998, 1999, 2002; Mandler, 2004; Russell, 1999). These proposals share in their rejection of the nativist-empiricist debate as misguided or altogether incoherent and their resolutions of the debate have tended to take the form of some eclectic union or outright dismissal. One of the central theses of this paper is that, in dissolving or ignoring the distinctions and problematic commonalities between nativism and empiricism, researchers have failed to accomplish their shared goal of transcending the limitations inherent to the respective positions. Nativism and empiricism are two distinct attempts to account for the source and development of our knowledge. While different with respect to source, both share in their commitment to foundationalism and both have a strong tendency towards anti-constructivism. I will argue that these commitments frame the nativist-empiricist debate, and, therefore, that rejecting them undercuts that debate altogether.

As a core epistemological assumption, epistemological foundationalism precludes emergence and in so doing violates naturalism. The expression of foundationalism in developmental psychology has been particularly damaging for emergent constructivist action-based thinking in general and Piaget's theory specifically. Developmental nativists were active in contributing to the growing trend to supplant Piagetian theory with models and ideas that were derivative from the broader information-processing framework in which they were situated. Drawing on the resources of the competence-performance distinction, along with their own methodological innovations, these researchers proceeded to move beyond Piagetian theory. However, their research agenda

will be criticized on two separate grounds: first, many of the assumptions present in nativist research are implicitly presupposed and,it will be argued that there are good reasons for doubting their adequacy. Second, some of these implicit assumptions preclude alternative emergent constructivist interpretations a priori and in so doing beg the question against such alternatives.

The potential problamtic[1] of precluding alternatives a prior is manifest with regard to both conceptual and methodological aspects, though of course the two categories are intimately related. At the "strictly" conceptual level, nativist research is problematic because it presupposes foundationalism. At the "strictly" methodological level, nativist research is problematic because of its a-theoretical construal of habituation, in addition to its failure to adequately control for perceptual level variables. And, directly implicated across both levels is its inherently biased application of the competence-performance distinction. Importantly, the possibility of ever discovering such problems, should they exist, is itself only possible within a framework that accepts conceptual argumentation as a proper part of science. That is, the type of inadequacies argued to be present in nativist research could only ever be discovered through conceptual analysis.

If the current article's conclusions are correct, then the problematic elements of the developmental nativist research program of the last thirty years can be understood as a contemporary (particular) instantiation of deeper epistemological issues regarding foundationalism. Further, given that *empiricist* positions are equally committed to

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[1] Ratcliffe (2007) raises a similar concern in the context of folk psychology: "The assumption that adults understand each other by attributing internal propositional attitudes was written into these studies from the start. It motivates them, influences their experimental design and operates as a framework for the interpretation of the results (p. 55)".

foundationalism they are also equally plagued by its fundamental inadequacies. The particular way in which these inadequacies play out in empiricist research is going to differ from that of nativist research, but it is fundamentally no better off. The ultimate conclusion drawn from the current analysis is that an emergent constructivist action-based approach is necessary to fully transcend the debate between nativism and empiricism and ground mind in the natural world.

This article is divided into three major sections: The first section provides a historical overview of the recent dialogue between nativist and non-nativist positions along with contemporary attempts to transcend the general debate between nativism and empiricism. The discussion then turns it attention to the dominant anti-Piagetian trend that developed in psychology during the 70s and 80s and how this trend was then used as a catalyst for promoting a developmental nativist research agenda which itself further extended and elaborated the anti-Piagetianism. The existing dialectic between "refuting" Piaget and promoting a generally nativist position was taken to the extreme through the use of new methodological procedures (i.e. looking paradigms) and constitutes the focus of section two.

Section two concerns itself with the empirical foundations of the nativist research program. There have been a series of recent conceptual and methodological criticisms of nativist experimentation from the 80s and 90s. Two of these experimental procedures have received most of the attention and are therefore the focus of this section. First, Baillargeon's "draw bridge" procedure is presented and evaluated in the context of her critics; followed by Wynn's "calculation" procedure. The general conclusion of these critics is the methodological point that these procedures systematically failed to control

for perceptual level variables. One of the central conclusions of this article is that this systematic failure was itself motivated by the underlying foundationalism inherent to nativist positions.

Section three deals with more conceptual issues with particular focus on the centrality of representation for understanding genuine development. The resulting conclusions suggest that the primary problem with nativist competence models can be understood as one manifestation of a more general error related to the notion of representation as constituted by some form of encoding relationship. The encoding critique (and partial alternative) is then used to evaluate the adequacy of a Parallel Distributed Processing (PDP) model's attempt to account for the idea of genuine development in terms of graded representation. Concluding that PDP does not resolve the representational issues, a final section reminds the reader that empiricist positions are equally committed to foundationalism and as such are equally problematic in their attempt to fully understand the mind.

Section 1: The historical context

Two intellectual pillars of modern nativism: Chomsky and Fodor

Before focusing on the dominant nativist climate within mainstream developmental psychology that emerged during the 70's and 80's, consider that this climate shift was itself part of a broader atmosphere of ideas that had recaptured academic thought concerning the innateness of multiple aspects of mind. A major component of Chomsky's critique of Skinner's model of language (Chomsky, 1959) concerned what could and could not be learned given the "mental" apparatus of

behaviorist psychology. His poverty of the stimulus arguments (POS) pointed out that our exposure to spoken language (in conjunction with behaviorist theoretical resources) was not sufficient to account for language acquisition and therefore, we must have an additional source of information available to us; this additional source of information was to be found in the genes. The more general shift away from behaviorism, which constituted the "cognitive revolution", did not itself require a nativist agenda; but, with behaviorism out of the way the door was now open.

Chomsky's pioneering work (Chomsky, 1965) set the linguistic agenda for at least the next 30 years and thus the ubiquitously assumed innateness of our language faculty. While the implications of this for developmental psycholinguistics was straight-forward and direct, Chomsky's ideas had a far more convoluted contribution to developmental psychology proper through the use of his competence-performance distinction. As a theoretical tool, this distinction played a central role in the presumed refutation of Piagetian theory; and with that out of the way, there were few remaining barriers towards a full blown developmental nativism (more on this later).

From a philosophical front, Fodor was deploying his more general conceptual framework in an effort to extend the boundaries of innateness so as to encompass virtually all of our conceptual content (Fodor, 1975). The form of Fodor's argument was different from that of Chomsky's POS in that, for Fodor, learning cannot even get off the ground without already having innate content. Fodor's point was essentially that, if learning has the form of hypothesis generation and confirmation, then the content of those hypotheses must already be available to the organism in order to formulate them in

the first place[2]. Further, and this is the "pessimistic" part (Cowie, 1999), if most concepts are non-definitional[3] and thus do not have internal structure[4], then they must be basic; that is, they cannot be learned; thus, if most concepts cannot be learned then they must be innate.

Anti-Piagetianism as a catalyst for developmental nativism

A definitive turning point for the conceptual battle over innateness could be considered the 1975 debate between Piaget and Chomsky[5]. However, within developmental psychology proper there were various other battles taking place. While different developmental researchers seemed more or less inclined to favor or reject Piaget's ideas, virtually all of them attempted to assimilate his theory into their own (typically behaviorist or information processing) framework thereby frequently introducing distortions and misinterpretations of his work. One of the consequences of this assimilation was that Piaget was being attacked on fronts for which he had made no claims. Compounding and obscuring the problem of misinterpretation was the very (experimentally) productive use of the competence-performance distinction. By

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[2] Popper (1968) proposed a similar argument with respect to induction (though typically criticizing induction as warrant he also criticized induction as origin w/o the nativist conclusion).

[3] However, Cowie rejects Fodor's argument that internally structured concepts could not have some, alternative, non-definitional constitution: "If concept learning does not in fact involve the representation of definitions, in short, the empiricist acquisition theory is immune to Fodor's critique (p. 78)".

[4] The empirical basis for this claim came from reaction time studies in which responses to most lexical items took equally long. The conclusion was that people were not constructing these lexical items because if they were then it would have taken more time for those words that had internal structure – required construction (Fodor, Bever & Garrett, 1974). The obvious rejoinder is to consider the possibility that once constructed those items with internal structure can be retrieved whole. That is, Fodor's conclusion assumes that the result of a constructive process cannot itself be stored (Bickhard, personal communication).

[5] Though of course it involved many others that crossed disciplinary boundaries.

controlling for certain "performance" factors researchers were claiming to have demonstrated that a given competence had emerged earlier than Piaget's theory had postulated. It was from within this anti-Piagetian context that developmental nativism took hold resulting in an explosion of nativist studies (some of which are now classic).

The response from non-nativist

One of the major counter influences to the broader nativist domination of the time was Parallel Distributed Processing (PDP) research. Having suffered a major setback from the in-principle argument against the adequacy of the 'Perceptron' as a model of human cognition (Minsky & Papert, 1969), research that evolved out of the perceptron was slow to regain momentum. These new PDP models were able solve the problem that Minsky and Papert had proven was not possible for a perceptron to solve. Further, PDP research seemed to provide a much more powerful and natural approach to learning than did the work done from within the classic symbol manipulation approach. Importantly, PDP networks could generalize beyond their specific training, something that eluded the rigidity of symbol manipulation accounts of learning. Finally, the success of explicit PDP models in psycholinguistics (McClelland, Seidenberg, , 1986; Seidenberg & McClelland, 1989; Plaut, McClelland, Seidenberg & Patterson, 1996) contributed significantly to a growing dissatisfaction with the nativist agenda set by Chomsky and once again the tide began to turn. The subsequent emergence of dynamical systems theory as an alternative to representational frameworks altogether was explicit in its rejection of the competence-performance distinction and further contributed to the momentum away from nativist positions. Finally, neo- and post-Piagetians maintained what they took to be the core of Piagetian theory and attempted to rework the "details". All of these programs were united in their anti-nativism and a number of subsequent proposals (Elman, et al., 1996; Thelen & Smith, 1994; Karmiloff-Smith, 1992; Newcomb, 1998, 1999, 2002; Mandler, 2004;

Overton, 2004; Russell, 1999) [6] shared in their rejection of the nativist-empiricist debate altogether; but this global rejection of the debate has been importantly influenced by what exactly researchers took to be the problem.

Attempts to transcend the debate

Newcomb

The following subsection will briefly consider some representative examples of these recent attempts to transcend the debate between nativism and empiricism. For many, the debate is seen to be fundamentally misguided: the claim is not so much that nativism is wrong and empiricism correct (or vice versa) as that the distinction artificially polarizes something that is inherently both. Newcomb (1998, 1999, 2002) has rallied for a compromise between the two poles that she refers to as the 'radical middle'. The 'radical middle' is presented as an alternative stance for those weary of positions that are either radically nativist or radically empiricist. The stance is motivated by the recognition that there are a number of dichotomies in development for which radical nativism and radical empiricism offer opposing support. A move towards the 'radical middle' need only require assuming that the poles of these dichotomies are not mutually exclusive. If what position a researcher explores is considered to be more a matter of emphasis[7] than truth, then the different perspectives on cognitive development (nativism vs empiricism[8]) can "… be seen as advocating a partial truth. The trick, as in mixing the perfect martini, is to get the proportions just right" (p. 458). The essential point of this proposal is idea that some aspects are learned and some aspects are innate and how much of each depends on the *particular* domain/structure/etc being studied. Stated in this way there would seem to be

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[6] The last two references in particular have number of important convergences with the current analysis, but the nature of their diagnoses and subsequently the alternative proposals offered differ in important ways from the current project.

[7] See Cowie (1999) or Simpson (2005) for an elaborated analysis of this idea.

[8] Newcombe includes constructivism as one of the developmental perspectives though nothing in the current discussion depends on it being present and may take away from the point being made.

little to disagree with; so, where the differences have traditionally emerge is when we specify the *amount* and *type*[9] of stuff that we are talking about (Cowie, 1999; Simpson, 2005). At its core the nativist-empiricist debate is about knowledge and the specifics of where it comes from[10].

Elman, et al.

Elman et al. (1996) attempt to clarify the issue when they suggest that "… the problem is not so much that we do not know what the sources of knowledge are [genes and environment]. The problem is rather in knowing how these sources combine and interact … what is necessary is to understand the nature of that interaction" (p. 357). The intuition for this position can be captured by the authors' earlier observation that development is, at all levels, the product of interactions between the genes and the environment (hence their expression 'interactions all the way down'). One of Elman et al.'s major concerns is that the notion of innateness is empty or vacuous because it says nothing about the prior complex interactions that had to take place. That is, to claim that something is innate does little to explain the process by which that something came about. This type of concern is reminiscent of criticisms over the proliferation of instincts used in early nineteenth century psychobiology to "explain" behavior because all it really did was label (Johnston, 2001).

Notice however that both sides of the debate fail to take seriously the problem of emergence and thus the absence of an emergent[11] constructivist alternative. Nativist

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[9] Analyses typically place the primary locus of the dispute with the "amount" of stuff that is considered innate with less concern for the "type" of stuff that is innate (Cowie, 1999; Simpson, 2005), more on this below.

[10] However, Cowie (1999) suggests that nativism has historically conflated what are two distinct problems: what provides the foundations for a rationalist epistemology and where what is in our minds comes from? The former being an epistemological question for philosophers and the later being a psychological question for which the "nativist hypothesis" may provide an answer. I will point out however that this construal of the situation seems to presuppose the representational divide present in Descartes and institutionalized by Hume. That is, that origins are causal and justification is normative.

[11] While most contemporary nativist and empiricist positions characterize themselves as constructivist, they lack a commitment to an action-based framework and consequently

positions do not seem problematic, in-principle, if the claim is simply that they lack any of the details concerning how the starting state came about.

Spelke and Newport (1998) exemplify just such a response by suggesting that, because all theories of developmental psychology have the same general form in which they must characterize a starting state as well as the developmental processes that transform that starting state into mature form, that nativism is no less empty than any other theory. However, whether or not this holds seems to depend on what is involved in characterizing the starting state. Let us suppose that Fodor (1975) is correct and virtually all concepts are innate, then it would seem that we are left with a "finished" developmental psychology. In other words if all characterizing the starting state amounts to is a characterization of the mature form, then there is no point in having a developmental psychology because it doesn't do anything. But this example only demonstrates that a successful radical developmental nativism would leave one with few job opportunities. There would seem to be nothing principally wrong with putting your energy into characterizing the starting state and leaving an account of how that starting state came about to another discipline (Spelke et al., 1992; Samuels, 2002).

However, an emergent constructivist interpretation of the point that Elman et al. seem to be concerned with involves an assumption about the benefit of a developmental perspective per se in which one anticipate insight into a things nature (characterization) via an understanding of how it came about (developmental processes). Full appreciation of this interpretation requires fully recognizing the necessity of emergence. That something must be capable of emergence places powerful theoretical constraints on the characterization of that something. Framing the issue in terms of the scope of starting state, regarding

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are limited to what could be called, combinatorial constructivism. The idea here is that constructions are limited to the re-organization of foundational elements. These positions are not constructivist in the Piagetian sense of the word and so the term emergent constructivism will be used to explicitly contrast itself with these foundationalist notions of constructivism.

cognitive development, ignores the necessity of emergence because the starting state is itself already assumed to have emerged in evolution (nativism).

Analysis

The central locus of issues regarding emergence is with respect to representation and so regardless of the adequacy of Spelke's response, the epistemological core of the nativist-empiricist debate is quite tangential to the fact that development involves 'interactions all the way down'. It is representational nativism that Elman et al. type connectionists want to eliminate (Fodor, 1998) and regardless of the gene-environment interactions that take place[12], it is accounting for representational content that is the crux of the issue between nativists and empiricists.

While connectionism has played a central role in arguments for those who are united in their opposition to nativism, within the sphere of cognitive science, connectionism was equally a reaction to the staunch symbolism of classic A.I. Andy Clark (2001) has characterized connectionism as the second revolution in cognitive science. Within this sphere, the focus was on the discreteness of representational symbols and the serial processing computer metaphor. Connectionism was championed to have overcome discrete homuncular symbols with their use of distributed representations and exploitation of non-linearity. While these conceptual resources clearly provided for some important insights beyond that of classic A.I., connectionism has yet to fully appreciate that it does little to address the basic representational issues concerning content (Bickhard & Terveen, 1995; more on this later).

Dynamical systems theory

An historically parallel reaction to the symbolism of classic A.I. (though one that was slower than connectionism to gain momentum) was characterized by Clark (2001) as the third revolution in cognitive science: it was the emergence of dynamical systems theory

[12] Though understanding those interactions may importantly inform us about the ontology of representation.

(VanGelder & Port, 1995; Thelen & Smith, 1994). Dynamicists can be understood as rejecting the entire space in which the nativist-empiricist debate takes place by denying the need to consider representation (as discrete symbol) altogether. For the dynamicist, it is not so much that the debate is misguided or even incoherently characterized; rather, the debate is irrelevant. Understanding cognition does not require the notion of representation at all: "From a broadly dynamical perspective, cognition is seen as the emergent outcome of the ongoing interaction of sets of coupled quantitative variables rather than as sequential discrete transformations from one data structure to another" (p. 12, VanGelder, 1999). Some of the early success on the development of walking and reaching demonstrated how looking at the non-linear, coupled dynamic properties of certain systems could provide superior models for behavior that was previously assumed to be driven by representational programs (Thelen & Smith, 1994, Thelen, 2000).

However, to the extent that dynamicists reject representation (and thus the whole basis of the nativist-empiricist dichotomy) they have had difficulty modeling those areas that, as Clark & Toribio (1994) put it, are 'representationally hungry'. That is, a dynamical systems perspective may work for those areas that, as a matter of ontological fact, do not actually require "full blown" representations, but surely some of the problems of cognition do use (require) representation and for those problems Dynamical Systems Theory (DST) would seem ill-equipped for the task. In an attempt to address this concern dynamical field theory (DFT) has emerged (Spencer & Schoner, 2003) with the promise of 'bridging the representational gap'; however, it is unclear how activation fields resolve any of the basic representational issues that motivated the extreme anti-representational move of DST in the first place[13].

Summary

[13] What the dynamicist really needs is a model of representation that constitutes a genuine alternative to the symbols of classic A.I. (Bickhard & Terveen, 1995).

For both revolutions (connectionist and dynamicist) then, the basic issue concerns representational content and if it was empty symbols that led many away from traditional A.I., then it would seem that they have simply been replaced by either empty vectors in the case of connectionism or empty activation fields in the case of DFT. Despite the recent proposals to the contrary, there is a substantive distinction to be made between nativism and empiricism and any attempt to resolve the issues through collapsing it in favor of an interaction approach or dissolving it by eliminating representation altogether does not solve the basic issues concerning the nature, origin and development of representational content.

The nativist-empiricist distinction is substantive

It was mentioned earlier that representation (epistemology) is at the core of what is interesting about the distinction between nativists and empiricists and it is in accounting for the source(s) of that representational knowledge which makes for a substantive distinction between the two. That is, nativism and empiricism are opposing positions concerning the source of representational knowledge. Contrary to modern caricatures, they are distinct positions precisely because they disagree on the nature of the interaction between biological and environmental factors not because either denies that the interaction exists altogether.

Perhaps some of the clearest discussion concerning the substantive difference between nativist and empiricist positions can be found in the 1975 debate between Piaget[14] and Chomsky (Piattelli-Palmarini, 1980)[15]. For the nativist "all structure comes from within. Environment *reveals* this structure; it does not *imprint* its own patterns on the system (p. 12)", alternatively "forms… preexist… and are forced to materialize under appropriate conditions (p. 13)". In both renditions of nativism the environment plays a necessary role in the (perhaps complex) series of interaction that must take place between biological and environmental factors; but, the central point is that the structure comes from within the organism. Spelke and Newport (1998) suggest that in asking whether an ability

[14] Not that Piaget was an empiricist.
[15] See also Fodor (1997), pp. 148 & 149.

is innate, one is asking whether it is independent of learning. While what exactly you take learning to be is going to differ depending on your perspective, their suggestion seems to capture much of what is at issue.

For the empiricist, structure comes from the environment and imprints itself upon us (consider Aristotle's canonical signet ring imprinting in wax). While contemporary accounts of such imprinting or "transduction" are not as simple, involving processes of abstraction and induction, the function of such processes is still, ultimately, to transfer the structure of the external world into the mind (Bickhard & Ritchie, 1983) such that mental content reduces to experiential content. However, as Fodor (1997) succinctly puts it, "that there is generally more in the content of a concept than there is in the experiences that prompt us to form it is the burden of the traditional rationalist critique of empiricism (p. 150)". Here then is a basic outline of the substantive difference between nativist and empiricist positions.

Foundationalism: below the surface

A central point of this paper is that, despite this inherent difference with respect to source, both nativism and empiricism share in their commitment to foundationalism. Foundationalism is the assumption that knowledge of the world is built up from a preexisting representational base and it will be argued that it is precisely this assumption that constitutes a fundamental error concerning our attempts to understand the developing mind. Classically, foundationalism was motivated by the problem of skepticism in the sense of providing a solution to it: ones foundation constitutes what is beyond doubt (Taylor, 2003). That is, it was the *certain* base upon which to build the totality of our knowledge. Further, foundational representations are the (supposed) means of our epistemic connection to the world. In their attempt to reduce mental content to experiential content, traditional empiricists proposed causal sensations of the factual

world (e.g. 'color patches') as the base for their foundation. In contrast, traditional rationalists accepted that there are aspects of knowledge that experience can principally never provide and so proposed to include these aspects (e.g. necessity) in the base of their foundation.

Set within the context of foundationalism, contemporary nativist and empiricist positions seem to differ simply with respect to the "richness" of the innate representational foundation that they presuppose (Fodor, 1981, 1998; Cowie, 1999, Keil, 1998; Simpson, 2005; Samuels, 2002). That is, the debate seems to center on how much of the foundation is innate and how much is the product of passive experience (in the sense of the passive transmission of content). Empiricists typically want to presuppose as little initial representational structure as possible in their account of mind, hence the general learning mechanisms. While nativists take seriously the principled arguments for why such a paucity of structure is inadequate and subsequently propose a much richer set of innate structures.

Emergence as constraint

Standard notions of representational content (common to non-action-based frameworks) require foundationalism because their own emergence cannot be accounted for, in principle (Fodor, 1975, Bickhard, 1991, 1995). However, just as the foundational substances earth, air, fire, and water did not account for their own emergence (nor the emergence of any new substances), foundationalism about representational content is equally contrary to an emergentist perspective in particular and scientific naturalism[16]

[16] While naturalism is often equated with physicalism, its use here implies a notion in which rocks, rivers, trees, and mind are all related and integrated as part of the same natural universe (Bickhard, unpublished manuscript).

more broadly. That is, foundationalism does not account for its own foundations, rather, those foundations are presupposed and as such foundationalism explicitly precludes its own emergence. By assumption, foundationalism has no account of itself, and, further, it precludes emergent constructivist alternatives a priori; if knowledge already exists, then what need is there for constructivism at that level. Foundationalism "solves", by assumption, the problem (origins of knowledge) that an emergent constructivist position assumes has to itself be explained. Whether the source of that knowledge is thought to reside out in the world (empiricism), or thought to reside within the system (nativism), is mostly irrelevant.

The in-principle failure of a foundation to be capable of emergence would constitute a violation of naturalism and thus imply that either mental representation is itself epiphenomenal (Kim, 1993) or that it resides in a separate realm (metaphysical dualism). That is, if we demand an explication of representational content that is consistent with our understanding of the rest of the natural world then such content must have emerged at some point in the history of the universe (emergent constructivism). On the one hand, if mental representation is assumed to be reducible to physiological states of the brain, then the only emergence in need of explication concerns biochemistry (neuroscience). Eliminative materialism is a philosophical position that captures that perspective (Churchland, 1981). It takes naturalistic considerations seriously but assumes (or argues) that mind is ultimately epiphenomenal (causally inert). Eliminative materialism doesn't necessarily reject emergence of the natural world, rather it denies the causal reality of mind (as distinct from brain), and thus leaves nothing mental in need of emergent explication. On the other hand, if 1.) the cost of eliminating the reality of mind

is too great (mind doesn't eliminatively reduce to brain) and 2.) natural emergence is precluded a priori (foundationalism), then 3.) there must be a realm beyond that of the natural world in which mind resides: metaphysical dualism. The proposal of this paper is that both of these two positions are unacceptable and thus the emergence of a causally efficacious mind must be possible.

Aversion for nativist positions

The anti-naturalism of foundationalist positions seems to become intolerably salient with the need to presuppose copious amounts of innate content and that may explain why many researchers have an intuitive distaste for nativist positions: they leave too much beyond the realm of natural explanation. Cowie (1999) draws a similar conclusion concerning "empiricists' gut opposition to nativism" though she considers that opposition to be derivative from the pessimism associated with Fodor style impossibility arguments rather than from the proliferation of foundationalism per se. Further, Tooby, Cosmides and Barrett (2005) have a vibrant discussion concerning the reason for the reluctance by researchers to endorse nativist proposals[17]. They suggest that despite good arguments to the contrary, non-nativists remain unconvinced because most contemporary nativist arguments concern phenomena that appear (at some level[18]) to be objectively and publicly present in the world (i.e. language/grammar); therefore it is always (seemingly) possible that some future model within an empiricist framework will succeed without the need to posit specialized innate machinery.

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[17] Tooby et al. are evolutionary psychologist who are ultimately arguing for accepting Darwinism as central to making psychology a science, but their point concerns the failure of POS arguments in particular, and rationalist critiques of empiricism more broadly, to penetrate non-nativist thinking.

[18] While language is clearly present in the environment, the point of Chomsky's POS arguments is that some aspects of it are not (i.e. grammar).

To overcome the-believed-to-be-objectively-present-in-the-world problem, Tooby et al. suggest finding competencies based on patterns that are clearly not available to the senses (because they are not present in the world). Tooby et al. suggest that a convincing argument for nativism could appeal to motivational competence (value assigning competence). However, Tooby et al.'s argument seems to unintentionally highlight the sense in which foundationalism must be presupposed by any framework that cannot account for the emergence of phenomena taken as its own base; in this particular case, normative phenomena (motivational competence). In sum, Tooby et al. have demonstrated that the normative aspects of the mind cannot come from the current environment (though neither can they come from our evolutionary environment[19]). So perhaps then the crucial failing is rather that researchers don't fully appreciate the extent to which normativity penetrates all aspects of mind rather than the further step of supposing that that normativity must be innate.

Finally, in addition to the potential adverse reaction researchers may have towards a nativist stance, empiricism has its own powerful allure because in some broad sense empiricism must be the case. Our knowledge is about the world and as such ought to be (in some sense) answerable to it (Bickhard, 2006). That is, it would be bizarre if the source of what our knowledge is about is not (in some sense[20]) the world.

Unexplained innate foundations

As alluded to above, the standard rejoinder to concerns about presupposing too large an innate foundation is that evolution/biology is not part of developmental

[19] Hume's no *ought* from *is* applies equally to evolutionary *is* as it does to developmental *is*.

[20] Of course it is in the details of this process (whatever it is) that things become more difficult.

psychology and so accounting for the presupposed foundation need not concern us as psychologists (Spelke et al. 1992; Fodor[21], 1980, 1981; Samuels, 2002). Recall that Spelke and Newport's (1998) specific response was that because all theories must characterize a starting state as well as the developmental processes that transform that starting state into mature form, that nativism is no more empty than any other theory. Samuels (2002) argues that innateness claims are claims that such and such should be considered *primitive* with respect to the domain in which the claim is being made. And while an explanation of a *primitive's* acquisition is possible, it is not for the scholar of that domain to determine. That is, psychology need not explain its own primitives: that is (by definition) the job of another science.

The problem with this stance is two-fold. First, as mentioned above, a genuinely developmental perspective presupposes that our understanding of how something is acquired can importantly influence our assumptions about its ontology, so then adopting a nativist stance would seem contrary to the spirit of the developmental perspective, it would seem to reject the relevance of understanding origins as central to understanding ontology. Of course providing a characterization of a possible ontology for representational knowledge in the innate starting state does not in itself necessitate an explicit explication of its origins; however, a potential and likely problem arises because some account of origins (natural or super-natural) will be implicitly presupposed by any characterization so given (innate or not). Further, the subsequent attempt to model the origins of new knowledge acquired after the innate starting state will be highly constrained given the original decision not to account for the origins of the starting state. Finally, if one does have an account of how genuinely new knowledge is acquired later in development, then what reason is there to

[21] As a consequence of his nativism Fodor (19??) suggests a minimalist developmental psychology in which learning is relegated to the study of belief fixation about contents that are already available – innate.

suppose that that account does not apply equally to the innate starting state[22]. In other words, if you have a genuine account of how new knowledge emerges later in development, then why does that account not apply equally to the starting state.

The second problem deals specifically with the potential concerns involved with the implicitly presupposed account of origins. Bickhard (1991) points out that Fodor's argument against learnability is a logical issue concerning hypothesis formation and confirmation processes in general and there has been no argument nor any indication concerning why the evolutionary processes of variation and selection are relevantly different from those of learning processes. Conversely, on the assumption that evolution can in fact account for the emergence of content, there is no reason to suppose that the processes involved in learning and development are not also able to account for such content. Bickhard points out further that what Fodor's argument has really demonstrated is that, if learning is constituted solely by hypothesis formation and confirmation, then natural emergence of content is not possible. Consequently, if no natural origins are possible, then the ontology must be incorrect. That is, the nature of representational content and learning (how they are characterized) must be different than what we assume them to be. This then is the in-principle danger of trying to characterize the starting state in isolation from an explicit understanding of its origins.

That Fodor's argument is actually a reductio for some of our assumptions about knowledge and learning was acknowledged by Fodor himself during the 1975 debate between Piaget and Chomsky (Chomsky & Fodor, 1980).

> … I am inclined to think that the argument has to be wrong, that a nativism pushed to that point becomes unsupportable, that something important must have been left aside. What I think it shows is really not so much an a priori argument for nativism as that there must be some notion of learning that is so incredibly different from the

[22] This line of reasoning is the inverse of a point made by Bickhard (1991) discussed below.

> one we have imagined that we don't even know what it would be like as things now stand (p. 269).

Despite Fodor's acknowledgement, at times, that some of his own arguments are probably reductio's for some of our assumptions about learning, development and representation, he is fond of pointing out that our current assumptions constitute 'the only game in town' and that we don't have a clue concerning how to naturalize representational content. For example, in his discussion of Rethinking Innateness Fodor (1998) asks the authors to consider accepting representational innateness at least some of the time because "it's not as though, in general, there's a plausible alternative on offer; often there is none (p. 147)". However, contrary to Fodor's suggestion, and as mentioned above, there is an alternative to the classic framework of assumptions. With their precursors in Peirce's pragmatism, action-based approaches constitute an alternative framework to the foundationalism inherent in both nativist and empiricist perspectives. Piaget's model is the major example of such an alternative; however, its distorted interpretations meant that researchers failed to appreciate precisely this point. That is, what made Piaget's 'third way' qualitatively different from all other prior foundationalist attempts to understand the mind was precisely his action orientation.

Piaget (like everyone else) allowed for innate structure but the key was that this structure was non-representational, and from action alone he attempted to construct the representational mind. The point is illustrated by Papert's (1980) comment during the debate with Chomsky "That is really the issue between you [Fodor] and Piaget: not whether something has to be there from the beginning, but rather *how much* and *what kind* of something"[23]. Piaget's primary interest was accounting for new forms of knowledge. In an effort to distinguish more adequate forms of knowing from less

[23] While this observation is superficially similar to the earlier discussion concerning the difference between nativists and empiricists, the key is to note that the type of stuff for Piaget was not representational (conceptual).

adequate forms, Piaget sought to understand how the former developed from the later (Chapman, 1988). With respect to foundationalism Piaget's broader interest in new forms of knowledge can be understood as having as a special case the origins of what might be called 'first knowledge'. For most researchers, 'first knowledge' is coextensive with the innate starting state and controversy has revolved around its size. For Piaget, the starting state is comprised of certain goal oriented motor capabilities[24] and knowing is an emergent product of constructions that use them.

Anti-Piagetianism

In spite of the enormous impact of Piaget's thinking on developmental psychology, three interrelated themes helped contribute to his seeming irrelevance for contemporary research: 1.) unfortunate misinterpretations of his account, in conjunction with 2.) the competence-performance corollary of nativist arguments, united with 3.) the preferential looking methodology to "refute" Piaget; and with him, what is arguably his most important insight – the unaddressed and unrecognized anti-foundationalism inherent in his action orientation concerning the origins of knowledge. Despite there being problems with all three of these contributing factors, the end result was that Piaget's theory was mostly ignored in its details and heavily distorted in its spirit[25]. The specific sense in which all three of these factors were problematic will be discussed below, with primary emphasis focused on the first two. The ultimate outcome however – the consequence of distorting and ignoring Piaget's theory – was that the only well

[24] Another common misinterpretation of Piaget was to assume that these were reflexes (Gelman & Williams, 1996).

[25] This is evidenced by the preface in Chapman's (1988) book where he explains the reason for writing it: his reading of Piaget's work lead him to discover ideas that were very different from the ones that had been assimilated by developmental psychology.

developed and prominent action-based alternative had been set aside, and with it, Piaget's anti-foundationalist constructivism.

Misinterpretations of Piaget

The information-processing paradigm provided the framework into which those aspects of Piaget's theory that were translated were most often assimilated by researchers in North America during the 60s and 70s. The basic problem with this was that Piaget's ideas were not themselves formulated within the context of such a framework; instead, Piaget was pursuing a program of genetic epistemology (i.e. the "genesis of knowledge") via his critical method. As a consequence of not understanding Piaget from "within" he was often attributed with positions that he never endorsed (Lourenco & Machado, 1996) and subsequently when these positions were "refuted" it had, at most, unclear implications for Piaget's actual proposals (Smith, 1993). This point will be demonstrated concretely by considering two related misinterpretations of Piaget that figured prominently in the purported refutation of his model.

Adherence to age norms

One of the most illustrative errors of interpretation concerned the supposed adherence of Piaget's stage model to age norms. That Piagetian protocols offered certain age ranges for corresponding tasks at a given operational level was taken by some to be "one of the most important and straightforward predictions of Piaget's theory" (p. 146, Lourenco & Muchado, 1996). The methodological problem with empirical instantiations of this criticism was that the ability to solve a problem does not in itself demonstrate that children are using the form of knowledge being studied; because, for example, at least some operational tasks can be solved with preoperational methods. Compounding the

issue was the fact that researchers rejected Piaget's methodological criteria that the child must justify their judgments as part of the task (Brain, 1959; Brainerd, 1973). Altering the criteria for success enabled researchers to claim to have found a given competence earlier than assumed by Piaget's age norms (more on this below). The more important and conceptual problem with the criticism derives from the fact that Piagetian theory was concerned with the sequence of acquisition of new forms of thinking not with the absolute age at which children demonstrated those forms.

Structures d' ensemble (structures of the whole)

Still more central, influential and far-reaching was the controversy surrounding the notion of Piaget's *structures d' ensemble* (structures of the whole). Specifically, a challenge was mounted against Piaget's stage theory in virtue of the fact that children demonstrate asynchronous acquisition of performance on different tasks that presumably belonged to the "same" stage. Because *structures d' ensemble* were understood to imply synchrony across the stages, Piaget's theory was empirically refuted by these results (Lourenco & Machado, 1996). Chapman (1988) argues the sense in which this line of thinking was derivative from assimilating Piaget's structural-stage model into a functionalist perspective, in essence taking what Piaget had intended as a description by a common set of formal properties to be a causal explanation of behavior[26]. Chapman goes on to differentiate three classes of asynchrony/synchrony involved in developmental stages (horizontal decalages). The first two are called content decalages: these are

[26] Campbell and Bickhard have argued cogently the sense in which Piaget's stage of formal operations, unlike earlier stages, is committed to a functional explanatory interpretation; however they do not attempt to use the competence performance distinction to endorse Piaget's structural stage model as a competence model (Post-Piagetians), nor do they use the distinction to reject his insights altogether (Nativists).

developmental lags between different areas or domains of content (e.g. decalages between weight and volume conservation) or developmental differences within domains of content (e.g. conservation or transitivity operations); and finally, procedural decalages: different versions of the same task are solved by children at different ages. As the pioneer of several examples of content decalage, Piaget was fully aware that

> formally analogous groupings could be expected to appear at different points in development with respect to different contents[27]… When content-specific actions are interiorized and grouped together as operations, the resulting structures accordingly retain the content specificity of the operations of which they are composed. Horizontal decalages result because the grouping of certain actions is more difficult than the groupings of others… he [Piaget] never implied that all behaviors belonging to a given stage are bound up in a single functional unity, regardless of content … he explicitly rejected this view (Chapman, 1988; pp. 43-44).

In summary of the content decalages and *structures d' ensemble* Chapman says

> the *structure d' ensemble* is neither a functional totality uniting manifestations of the "same" structures in different areas of content nor a unity of different groupings within a single area of content. Each individual grouping is a relational totality resulting from a grouping of particular operations and as such is differentiated from other groupings according to both content (e.g. quantity,

[27] See Campbell and Bickhard (1986) for an explicit discussion of how and why this property is intrinsically manifest from their interactive levels of knowing hierarchy.

> weight, and volume) and type of operation (e.g. classification, seriation, multiplication of classes, and multiplication of relations) (p. 348).

However, even if Piaget's theory were able to account for both types of content decalage, the third type of decalages involving different procedures for the administration of the "same" task would seem to remain problematic. In this instance, the decalages are from within the same content area and for the same operatory structure (Chapman, 1988).

The basic strategy of critics utilizing this form of decalage was to demonstrate that extraneous "performance factors" had masked the child's true competence and thus the age at which the child demonstrated that competence. One of the first to explicitly Implement of this strategy was Braine (1959): "he [Piaget] fails to eliminate important variables which are not involved in the definition of the processes he sets out to investigate, and which hinder many of his Ss from responding to the appropriate cues (p. 40)" and followed up by others (Gelman, 1969, 1972; Bryant & Trabasso, 1971, Borke, 1975). Of course the validity of such challenges required that it was in fact the same competence being tested in both versions of the task. That two procedurally different tasks were in fact tapping the same competence was typically justified by the procedures mutual commitment to some single underlying "logical structure". The "logical structure" was taken to be some abstract feature common to all procedural variations of the original task (Chapman, 1988). This assumption is a sort of competence-specific version of understanding Piaget's *structures d' ensemble* as functional totalities.

Begging substantive issues in advance of empirical inquiry

Chapman (1988) elaborates on this error of interpretation. Piaget's operatory logic was concerned with formal properties of the child's actual reasoning, not some abstract norm of inference found in formal logic that remains invariant across procedural variations. If the modified procedures enabled children to solve the task with a different form of reasoning, then their success does little to undermine Piaget's theory. Consequently, several of the findings that were taken to have demonstrated precocious operational abilities were later demonstrated to be solvable using preoperational forms of reasoning (Chapman, 1988; Smith, 1992, 1993). As mentioned previously, a rejection of the judgment plus justification aspect of Piaget's critical method (Brainerd, 1973) compounded the issues surrounding whether or not variations of some task required the same form of reasoning. The obvious consequence of liberalizing the criteria for success on some task was that children would "succeed" on the task earlier than Piaget had reported. The more insidious consequence of eliminating the justification aspect of Piaget's method was that it sidestepped the substantive issue concerning whether the child's justification is constitutive of their competence or not, as well as dismissing Piaget's primary interest regarding necessity (Smith, 1993, 1992). In essence, the elimination of justification collapsed, by stipulation, a substantive distinction and dismissed the role of necessity in knowledge.

The broader point to take notice of is that the mere possibility (in the sense that it is *consistent* with the observational evidence) of attributing operational reasoning to the child does not demonstrate that that is in fact the case because it does not differentiate between alternative interpretations. Further, when substantive issues are begged in advance of empirical inquiry, as was the case in the attempt to "empirically" refute

Piaget's stage theory, such investigations do little to settle the disputed issues[28]. Presupposing such a confirmatory approach to empirical research is a vestige of the positivist philosophy of science from which psychology has yet to fully free itself (Bickhard, 1992).

Competence-performance distinction is explicitly introduced into development

Given: 1.) the "functional-totalities" interpretation of Piaget's *structures d' ensemble*, 2.) the failure to differentiate between Piaget's operatory logic and formal logic, and 3.) the *modus operandi* of attempting to control for extraneous performance factors, it was only a matter of time before the conceptual apparatus of the competence-performance distinction as discussed in linguistics (Chomsky, 1965) was formally incorporated into developmental theorizing (Flavell & Wohlwill, 1969)[29]. The basic logic of the competence-performance distinction was then used to spawn a large industry of research that was used to demonstrate that Piaget's theory was either: false (nativists) or incomplete (neo-Piagetians).

In both cases, findings that were contrary to the researcher's agenda were attributed to extraneous performance factors. The competence-performance distinction itself was a direct corollary of Chomsky's (1965) approach to linguistics. The assumption of the psychological reality of Chomsky's competence model entailed the need for a performance model. Performance functioned in the space of actual processes involved in

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[28] Unfortunately this confirmatory approach to research and the begging of substantive issues replayed itself exactly with what happened a decade later in infant research. In essence, the same errors have simply manifested themselves in a new literature that is only just now started to really recover. More on the parallel with more recent infant research below.

[29] While not forgetting these three threads are themselves situated within the broader information-processing paradigm with its focus on the computer metaphor.

cognition and development while competence remained in the realm of idealized abstraction.

Competence-performance: theoretical vs methodological distinction

A formal commitment to the competence-performance distinction is easily motivated by the casual observation that various abilities are not manifest under all circumstances. However, the shift from our intuitive grasp of competence to the standard distinction involves a crucial and fallacious distortion of a basic methodological distinction[30]. As a methodological distinction, wanting to control for extraneous factors that influence task performance is intuitive and perfectly acceptable, but as a theoretical distinction competence-performance is in error (Campbell & Bickhard, 1986). What is considered extraneous is always relative to a specific hypothesis and as such does not become theoretically uninteresting in general. Different researchers have considered memory, attention, means-ends coordination, language, etc. as 'performance' factors; but surely other researchers are justified in taking these variables as a 'competence' of primary interest (Thelen & Smith, 1994).

In a similar vein, Pinard and Pinard (1985) have suggested that the inherent ambiguity of whether some factor is ontologically in the competence class or in the performance class is the primary problem with the distinction and indicative of the more traditional problem that they refer to as the Cartesian dilemma: the problem of knowing in what way the use of a concept depends on having the concept. A methodological version of the competence-performance distinction, a version that is a consequence of the underlying information processing framework, is the assumption that action is not

[30] For an explicit example of precisely this conflation see Wynn's (p. 333, 1997) response to Sophian (1997).

relevant to the ontology of cognition. Instead, action is understood as the output of the processes that make use of cognition. Consequently, if action considerations are not relevant to cognition, then action incompetencies are just performance issues relative to cognitive development. In contrast, from within the Piagetian framework knowledge was intrinsically related to use and accordingly Piaget explicitly rejected the competence-performance distinction because "logical form and physical content are inseparable (As cited in Lourenco & Machado, 1996, p. 149)". Further, Campbell and Bickhard (1986) argue that the more fundamental error concerning the competence-performance distinction is derivative from the conflation of description (of task performance structures) with explanation (of how the tasks are accomplished) that is inherent to competence models in general (and Chomsky's model in particular; more on the competence-performance distinction below).

The modus operandi for developmental research

Thelen and Smith (1994) suggest that Gelman's classic 1969 paper set the methodological agenda for use of the competence-performance distinction in developmental research: "Define the *essence* of some knowledge structure, do a thorough task analysis, strip away the supporting process and performance variables that could limit successful use of the essential knowledge structure, and see if children possess the "essential" knowledge (p. 26)". Chandler (1991; See also Sophian, 1997) has argued that applying the competence-performance distinction to development, with the understanding that competencies are causal antecedents of concrete behavior, involves a "natural bias"

toward earlier and earlier attributions of those competencies[31]. The reason for this "natural bias" is that those behaviors that appear to demonstrate the presence of some ability are understood to be a direct consequence of that competence; in contrast, those behaviors that do not appear to support a given ability are dismissed as resulting from extraneous performance factors. The ultimate result of this asymmetry was that, with the accumulation of experimental evidence, the age at which a given competency was attributed to the child was systematically relocated to earlier and earlier ages, and it was the infant looking paradigms that exploited that bias to the extreme.

Competence-performance in infant research

The *methodological* strategy[32] used in infant research was to relocate the "presence" of certain competencies so far back into childhood (infancy) that no model of development was adequate to account for the findings, including Piaget's. The ultimate conclusion from this line of research was that various aspect of mind are innate. The subsequent explosion of innateness studies that appeared during the 70s and 80s came to dominate early developmental thinking and "challenges" to Piaget rarely attempted to take his emergent constructive position seriously.

The corresponding *conceptual* strategy used in infant research is attributed to Quine's (1960) riddle concerning the indeterminacy of translation (Thelen & Smith, 1994)[33]. The essential point was that in order for a stranger to learn a new language from

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[31] This "natural bias" also requires a nativist agenda because as mentioned above the competence-performance distinction was also used by neo-Piagetians to buttress Piaget's structural stage model from criticism.

[32] Fisher and Bidell (1991) have called this "The Argument form Precocity".

[33] While, Quine's riddle is most naturally assimilated by psychologist in the context of children's word learning, Thelen and Smith are making reference to Spelke et al.'s (1992) explicit discussion of Quine in the context of physical reasoning. Murphy (2002)

another group there must be some commonality between them that reduces the number of possible inductions. This problem was taken by developmental nativists to demonstrate that the failure of learning as unconstrained induction compels nativism because sense experience underdetermines what is to be learned[34].

Two general forms of innateness can be seen to have developed from these interrelated strategies: innateness of representation and inference (Baillargeon, Spelke, Wynn) and innateness of constraints (Gelman, Spelke, Keil). Despite being separate, they are intimately related in, at least, the sense that the later depends on the former. Fodor's impossibility argument demonstrates why this is the case: proponents of constraint nativism want to limit the space of possible inductions (hence the innate constraints), but to specify any hypotheses at all would require the innateness of the concepts that those inductions are about. The point then is that, while constraint nativism may (or may not) have some interesting differences from concept nativism, the more fundamental issues will revolve around concept nativism, and any problems it faces apply equally to its counterpart[35].

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has pointed out the sense in which appealing to Quine in the context of language development is already to miss much of his original intent as well as to fail to appreciate his very different underlying assumptions (p. 342-345). The further distortion by nativist infant researchers not only fails to appreciate what Quine was actually doing, but ultimately attributes to him what is much closer to Chomsky's POS argument. Finally, Wittgenstein used the inductive difficulties involved in ostensive definition to motivate why that assumption about learning word meaning must be wrong. In sum, there seems to be little point (and potentially a lot of confusion) in trying to derive nativist conclusions from "Quine's" riddle.

[34] If all we have access to is sense experience, then the number of possible inductions consistent with that data is too large to ever learn any particular one (i.e. a variant of Chomsky's POS argument).

[35] Notice that this point holds at a more general level even if recourse to induction is some how avoided. The aspect being exploited in the case of induction is with respect to

Summary

Armed with the necessary conceptual and methodological machinery, cognitive-science inspired developmental nativists proceeded to empirically "refute" Paiget's theory (Gelman, 1969, 1972; Baillargeon et al., 1985; Spelke et al., 1992) and in so doing promote their own nativist agenda. And while several of the classic nativist studies have recently been criticized for both methodological and conceptual failures (Haith, 1998; Bogartz et al., 1997; Schoner & Thelen, 2006; Meltzoff & Moore, 1998), the outcome of such criticism is typically limited to the endorsement of the given critic's own framework. That errors in the early nativist research also dissolves the purported refutation of Piaget has been mostly lost in the prior iteration of the historical dialogue. However, Piaget's theory continues to be the most comprehensive example of an emergent constructivist framework; and in so being, avoids the foundationalism that has been a crucial barrier precluding any real transcendence of the nativist-empiricist debate. Therefore Piaget's theory has contemporary relevance for any naturalistic attempt to understand the origins, development and ontology of mind.

Section 2: Infant studies – the "empirical foundation" for the nativist program

The experiments and their critics

There have been a series of recent criticisms leveled against some classic nativist studies for what can be considered both methodological and conceptual failures[36]. With respect to the former it has been empirically demonstrated that many of these

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the anti-emergence of representational content and the general problem will hold for any version of representational nativism, constraint or otherwise.

[36] Of course these categories are not mutually exclusive. The conceptual framework utilized and the methodology employed are bi-directionally related and subsequently so are the criticisms.

studies failed to adequately control for perceptual processes in their original research designs (Mix, Huttenlocher, & Levine, 2002; Rivera, Wakely, & Langer, 1999). Further, there are those who have pointed out the relevance of the inherent dynamics involved in the procedures used to study infant development (i.e. the process of habituation itself) (Schoner & Thelen, 2006; Schilling, 2000; Kagan, 2002). The point here is that there are important "content free" dynamics involved in looking (and looking away) that must be taken into account when conducting this type research. With respect to the conceptual issues, it is argued that these classic studies indulged in what has been termed "rich" interpretations of the data (Haith, 1998; Bogartz, Shinskey, & Speaker, 1997; Simon, 1997; Reznick, 2000). The basic point being that the empirical evidence from these nativist studies required fairly gratuitous interpretations to achieve the rich cognitive conclusion that nativist researchers were attempting to support.

In the domain of object research, Baillargeon's "drawbridge occlusion" (1987a), "car rolling down a ramp" (1986) and "short rabbit – tall rabbit" (Baillargeon & Graber,1987) procedures have all been subject to alternative perceptual interpretations (Schoner & Thelen, 2006; Schilling, 2000; Bogartz, Shinskey, & Speaker, 2000; Cashon & Cohen, 2000, Wakely et. al., 1999; Kagan, 2002; Berthier, De Bois, Poirier, Novak & Clifton, 2000; Hood, Carey & Prasada, 2000; Meltzoff & Moore, 1998; Bogartz, Shinskey & Speaker, 1997). In addition, Kellman and Spelke's influential "two-rods" procedure (1983) has also been reinterpreted in non-conceptual terms (Bogartz & Shinskey. 1998, Meltzoff & Moore, 1998).

With respect to number research, Wynn's (1992) calculation procedure (See also, Wynn, 1995, 1998, 2002) has received attention both from critics who promote an object

enumeration perspective (Simon, 1997; Xu & Carey, 1996; Feigenson, Carey & Spelke, 2002; Uller, Carey; Huntley-Fenner, & Klattt, 1999) as well as from those who argue for strictly perceptual level alternatives (Cohen & Marks, 2002; Mix, 2002; Mix, et al. 2002; Clearfield & Westfahl, 2006; Clearfield & Mix, 2001; Clearfield & Mix, 1999; Wakeley, Rivera & Langer, 2000; Kagan, 2002).

The current discussion will focus primarily on the two procedures that have received the lion's share of attention, Baillargeon's (1987a) drawbridge procedure and Wynn's (1992) calculation procedure. The analysis will reveal that epistemological foundationalism has systematically motivated nativist research both in terms of their design methodology and in terms of their cognitively rich interpretive stance. Foundationalism is contrary to an emergent developmentalist perspective and, so I will argue, it is foundationalism that constitutes the ultimate limitations of nativist research.

Looking paradigms as a tool for studying infant cognition

Virtually all variants of the looking paradigm deployed by nativist researchers use as their dependent measure the amount of looking time to test displays to indicate the infant's sensitivity to conceptual content. However, one of the major problems with this general paradigm, and one of the central points of this paper, is that these experiments often failed to adequately control for perceptual level processes and it is precisely the foundationalism of the nativist position that motivated that failure. A foundationalist position precludes emergent constructivist possibilities and thus ignores the need to control for alternative constructivist interpretations. In infant research, the failure to control for constructivist alternatives manifests itself as a failure to consider perceptual aspects precisely because perception is taken to be the simple evocations (transductions)

of innate contents with no methodologically relevant complexities; thus the lack of perceptual level controls. Here then is an intrinsic link between foundationalism and the lack of perceptual controls found in nativist experiments.

The preferential looking paradigm has evolved into many different variants since Fantz (1964) first explored some of its potential with infants. The basic logic (Schoner & Thelen, 2006) of the paradigm is derivative from the general tendency of animals to decrease reactivity with repeated exposure to a stimulus. In infant habituation studies, babies are repeatedly exposed the some display or event until they disengage their attention sufficiently (i.e. habituate). During the testing phase, infants are exposed to items or events that are novel on some dimension relative to the habituation phase. If looking time recovers and infants dishabituate, then they must have been able to detect some difference between the two items or events.

To probe the infants conceptual repertoire experiments are designed such that, purportedly, the only important differences between test conditions are with respect to their conceptual content. Consequently, if the infants look longer at the test display that requires conceptual content to discriminate, then they can be attributed with possessing that conceptual knowledge. The fundamental problem with this logic is that the ability to detect differences does not itself constitute knowledge of what those differences are about, and, because perceptual differences will always be present with "conceptual" manipulations, it is especially important that proper attention has been allocated for perceptual level controls[37].

[37] Notice further that the possibility of *conceptual* habituation is merely assumed on the basis of an analogy with *perceptual* habituation. That is, there is never any argument for the sense in which conceptual habituation is even possible; in fact, even the analogy is

Object Representation

Baillargeon's "drawbridge procedure".

Some of the most influential habituation studies have been derived from Baillargeon's drawbridge procedure (Baillargeon, Spelke & Wassermann, 1985; Baillargeon, 1987a; Baillargeon, 1987b; Baillargeon, 1991). In the canonical version of the task (Baillargeon, 1987a), infants were habituated/familiarized[38] to a display in which a paddle moved in a 180° arc; after habituation, a block was placed directly in the path of the paddle; finally, during the test phase infants saw alternating trials in which the block traveled either all 180°, seemingly passing through the block (a physically impossible event) or only 112°, seemingly coming to rest on the edge of the block (a physically possible event). In a control condition, infants received the same habituation and alternating test procedures but without the presence of the block in order to rule out the possibility that infants had an inherent preference for one or the other of the test displays. Average looking times were typically greater for the "impossible" event (accompanied by equal looking time during the control condition). As a result of these findings, Baillargeon concluded that, contrary to Piaget, infants as young as 4-months understood that the box continued to exist (object permanence) and that the screen could not rotate through the space occupied by the box (inference).

Methodological criticisms

(typically) left implicit in the interpretation and design of nativist methodology. In sum, assuming that perceptual and conceptual knowledge have important differences, there has been no argument why the process of habituation is not one of them.

[38] The word familiarization is used when the number of "habituation" trials is minimal ,approximately 1-4, (Aslin & Fiser, 2005) but any amount familiarization to events prior to the test trials are probably best thought of as varying degrees of habituation.

As mentioned above the literature is replete with alternative, non-conceptual, interpretations (Schoner & Thelen, 2006; Schilling, 2000; Bogartz, et al. 2000; Cashon & Cohen, 2000, Wakely et. al., 1999; Kagan, 2002; Meltzoff & Moore, 1998; Haith, 1998). The implications from these criticisms differ with respect to their breadth of application. At the narrow end is Wakely et al. (1999) whose primary critique applies to the specifics of Baillargeon's drawbridge procedure whereas Schilling (2000), Cashon and Cohen (2000) and Bogartz, et al. (2000) provide an analysis that spans certain aspects of the habituation paradigm in general. Schoner and Thelen (2006) extend and elaborate on that work by providing an explicit model of habituation dynamics that incorporates all of the known behavioral consequences of the procedure. Finally, Metzoff and Moore (1998) and Fischer and Bidell (1991) provide more of a conceptual analysis regarding developmental nativist theory and interpretive stance.

Rivera et al.

Rivera et al. (1999) point out a fairly straight-forward methodological confound involving amount of motion. The two test displays in the drawbridge procedure systematically differ in terms of the amount of motion involved (180° of paddle arc versus 112°). Recall that Baillargeon (1987a) included a condition to control for this possibility and found no difference in looking time. However, Rivera et al. note an inconsistency in the overall logic of Baillargeon's experiment (acknowledging the expectation of a novelty preference for the experimental condition but ignoring it in the control conditions) thus invalidating the results of her control condition. Specifically, according to the logic regarding habituation, there should have been a novelty preference for the 112° control condition, not equal looking to both (as was found). The basic logic

of the broader habituation paradigm is such that after habituation, infants will dishabituate (increase looking) to a perceptually novel display demonstrating their sensitivity to the change from habituation. In fact, intentional bias has been introduced into research design so that the more perceptually novel of the two test displays is paired with the "possible" event while the more perceptually familiar display is paired with the "impossible" event (Spelke, 1985). The reason for this pairing is to preclude counter claims that longer looking at the "impossible" display was merely the product of a novelty preference induced form the prior habituation.

Rivera et al. (1999) claim that the reason infants in Baillargeon's control condition did not differ in terms of looking at the two test displays was because their habituation-induced novelty preference for 112° was pitted against their (hypothesized) preference for more motion (180°), resulting in equal looking to the two displays. To disentangle novelty from motion the authors modified Baillargeon's control condition by omitting the habituation trials. The results indicated that infants looked significantly longer at the display involving more motion (180°) than at the display with less motion (112°) even though there was no "impossibility" manipulation (i.e. no block was involved). Further, the magnitude of looking at the 180° test display for the modified control condition was the same as the magnitude of looking at the 180° test display for the modified (no habituation trials) replication of Baillargeon's original experimental condition findings, indicating that the same preference for more motion that was clearly at work in the modified control condition was likely to have been operating during their modified replication of Baillargeon's original experimental condition. Taken together these findings were suggested to indicate that longer looking to the "impossible" rotation

"is due only to simple perceptual preference for events that display more motion (p.433)".

With respect to theoretical interpretation, Rivera et al. point out that even if longer looking to the 180° rotation of the paddle was the result of the object obstructing its path, such findings would be entirely consistent with an earlier stage of Piaget's six-stage account object permanence. Infants (as young as nine weeks) in stage two of object permanence are already capable of expecting a disappearing object[39] to reside at the location in which it vanished (Piaget, 1954 as cited in Rivera et al). Further, Haith, Hazan and Goodman (1988) experimentally investigated the development of visual-perceptual expectancies of dynamic spatial-temporal events in 3.5-month-old infants and established that they are able to rapidly develop expectations of visual events that are independent of their own actions. The point then is that these visual expectations, whether developed previously or during the experiment itself, are sufficient to account for the looking behavior displayed by infants participating in occlusion studies without recourse to object permanence (Meltzof & Moore, 1998).

The two experiments by Rivera et al. can be collectively understood as a rebuttal that is wholly on Baillargeon's own terms. That is, they accept both the theoretical and methodological framework from within which the drawbridge procedure is operating and exploit an inconsistency in the original work (i.e. acknowledging a novelty preference for the experimental condition but ignoring it in the control condition). Further, the decision not to provide habituation prior to test is entirely consistent with Baillargeon's (1987a)

[39] Of course in this context it is not a "conceptual object" for the infant with the consequent property of permanence. Assuming that it must be, as nativists do, is simply to beg the question against emergent constructivist alternatives.

own speculation that it was not theoretically relevant with respect to the results of the procedure. Their approach is both powerful and limited: It is powerful in that it followed both the test procedures[40] and logic used by Baillargeon herself; however, it is also limited by those same two factors in that it only establishes that the original experiment confounded amount of motion (via disregarding novelty preferences in the control condition) and ignored possibly important dynamics of the habituation process itself (via removing that aspect of the experiment altogether) without providing any insight as to what those important dynamics might be. Despite its limited scope, Rivera et al. exposed both methodological and conceptual failures that are expanded and elaborated by other critics.

In particular, a series of recent experiments have applied the well-documented (Hunter, Ames, & Koopman, 1983; Hunter, Ross, & Ames, 1982; Rose, Gottfired, Melloy-Caminar, & Bridger, 1982) perceptual processes involved in the preference for familiarity to reinterpret the results of the drawbridge procedure (Schilling, 2000; Bogartz, Shinskey, & Schilling, 2000; Cashon & Cohen, 2000; Roder, Bushnell, & Sasserville, 2000). The earlier set of findings on familiarity established that infants prefer familiarity after brief habituation exposure and that the novelty preference emerges

[40] Despite their explicit attempt to replicate the procedure (i.e. similar stage like enclosure, identical yellow boxes decorated with clown faces, striped walls, same sound cues, etc.) a recurrent theme in Baillargeon's response to critics is to point out that not everything in the procedure was identical (dark v.s. brightly lit rooms, infants positioned 100 cm (from the pivot of the screen) v.s. 65 cm from the screen, 10s v.s. 2-3s intertrial interval). Unfortunately, these differences detract from others that are potentially more interesting, though in all cases there is little argument as to why an infant that possesses these putative conceptual cognitive capacities should be so effected by modest to minor perceptual differences (more on this below).

only after longer exposure times[41]. Further, they demonstrated that the familiarity to novelty shift occurs faster if the display is "simple" or if the infant is older[42]. The methodological point made by the latter group of researchers is that the drawbridge experiments confounded perceptual processes that manifest as a familiarity preference with the "impossibility" of the display[43] leaving open the possibility that infants were responding on the basis of familiarity rather than impossibility. The habituation experiments were specifically designed to avoid confounding a *novelty* preference with the "impossible" event; but in so doing, they neglected to consider the earlier emerging familiarity preference and created the opposite confound between *familiarity* and "impossibility".

Bogartz and colleagues

To investigate the possibility that familiarity was responsible for looking behavior in the drawbridge procedure, Schilling (2000) systematically varied the infant's degree of

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[41] Kagan highlighted the importance of familiarity and novelty preferences some time ago (1971) by arguing that infants attend optimally to events that are not too "different" or too "similar" from what has been processed before. While Spelke (1985) acknowledges such non-linear preferences she did not really address the issue then and certainly does not address the slightly different issues raised by the current criticisms because what is familiar or novel depends on the infant's degree of habituation.

[42] Wang, Baillargeon and Brueckner (2003) have suggested that the properties of the habituation process involved in these experiments might belong to a different functional system from the properties of the habituation process involved in their experiments because they each tap different abilities. However, to suggest that there are different systems involved is simply to ignore the issue. Further, given that perceptual aspects are always present, it seems incoherent to suggest that perceptual habituation dynamics are not involved in their "conceptual display".

[43] As mentioned earlier, the basic logic of the habituation paradigm depends on the fact that after prolonged exposure to a stimulus, infants (and animals) will dishabituate to a novel event and thus demonstrate that they are sensitive to that type of novelty (this was why the "impossible" condition was designed to be the more familiar of the two, so that dishabituation to the impossible event could not be attributed to the novelty preference).

habituation via the manipulation of the number of familiarization trials (7 or 12) and differences in age (4 or 6 months). His results indicated that looking behavior at test was a function of those two factors. Bogartz et al. (2000) adapted the standard Event Set x Event Set design[44] using 5 1/2-month old infants to statistically test the competing perceptual (familiarity/novelty) and conceptual (impossible/possible) hypotheses at issues. Their analysis indicated that infants did not respond on the basis of impossibility but rather their looking behavior was the product of familiarity/novelty preferences that were themselves a function of the number of habituation trials, changes in screen rotation, and the presence or absence of the block, all of which are perceptual level variables.

Cashon and Cohen (2000) used 8-month-old infants and as many as 20 habituation trials to examine the time course of novelty preferences. Using an infant-controlled procedure[45] to ensure habituation, their results indicated that fully habituated infants prefer the perceptually novel/possible test display versus the perceptually familiar/impossible test display. Further, those infants who did not habituate (even after 20 trials) produced the opposite pattern of behavior thus replicating Baillargeon's standard findings. These results are consistent with those of Schilling (2000) and Bogartz et al. (2000) and further demonstrate the relevance of not having controlled for the

[44] The standard design is presented in Bogartz, Shinskey & Speaker (1997). The basic idea is to habituate three different groups of infants to one of the three events involved in the original procedures (ie the original familiarization event, the possible test event or the impossible test event) and then to alternate testing them on the other two events. This 3x3 design enables the authors to evaluate the amount of variance that can be attributed to each of the factors under hypothesis (impossibility, possibility, familiarity, amount of motion).

[45] This procedure simply means that the number and duration of habituation trials is determined by the looking behavior of each individual infant.

perceptual level processes involved with familiarity and novelty preferences in the original experiments.

Finally, Roder et al. (2000) examined the individual time-course of the emergence of a consistent novelty preference in 4 1/2-month-old infants so as to avoid any ambiguities resulting from individual differences in processing speed and artifacts of averaging data across infants. Their results indicated that infants selectively attended to familiar stimuli prior to their novelty preference and thus support a non-linear model of memory formation during infancy. Individually and collectively these results demonstrate the relevance of not having controlled for the perceptual processes involved with familiarity and novelty preferences in the original experiments[46].

While River et al. (1999) exploited an inconsistency from within Baillargon's own study that accepted the underlying logic and methods of the original experiments, Bogartz and his colleague's alternative interpretation systematically explored the confounding of familiarity with "impossibility" and highlighted the relevance of habituation prior to test. In so doing, their analysis has demonstrated the broader value of understanding at least one of the important dynamics involved in the habituation process itself–the familiarity to novelty shift. The most immediate consequence from this collection of studies is that, contrary to Baillargeon's speculation (1987a), the process of habituation itself has important systematic non-linear internal dynamics that influences subsequent looking behavior[47]. The historical fact that the original set of studies

[46] See Baillargeon, 2000; Munakata, 2000; & Aslin, 2000 for comments sympathetic to the original studies as well as Bogartz, Cashon, Cohen, Schilling, & Shinskey, 2000 for their reply to these comments.

[47] For a partial "acknowledgement" of this point see Wang, Baillargeon, & Brueckner, 2004.

neglected to consider these dynamics as relevant constitutes a methodological failure to control for perceptual level processes resulting from a preoccupation with demonstrating conceptual level knowledge. More deeply, this failure is derivative from the non-emergent foundationalist assumption that if it is a world of objects and their properties that the infant is looking at then it is a world of objects and their properties that they are seeing (Bickhard, 2001)[48]. Realize however that this anti-emergent foundationalist assumption is equally present for both nativists and empiricists. In the empiricist version conceptual content about the world imprints itself upon the mind, while in the nativist version that content is evoked (transduced) via direct causal exposure with the world.

While the empirical demonstration that looking behavior at test is, at least in part, a function of the internal dynamics of habituation, the specific details of those dynamics are going to differ among researchers. With their focus on familiarity and novelty effects, studies operating from within an information-processing perspective have understood habituation dynamics to result from the processes involved in completing the representation (Sokolov, 1963; Bogartz, Shinskey & Speaker, 1997). The idea here is that infants need a certain amount of time to fully encode what they are seeing in order to form a complete representation. With few habituation trials encoding is incomplete and thus the familiarity preference; with sufficient habituation the representation is completed and thus the novelty preference[49]. Because simple stimuli are encoded faster and because older children are quicker information-processors, novelty preferences appear with fewer habituation trials. While this model does provide an explanation for phenomena centered

[48] A similar point was made by Fischer and Bidell (1991) with their use of the term adultocentrism (more on this below).
[49] For version of the same idea in terms of schemata see Kagan (2002).

around familiarity and novelty, Schoner and Thelen (2006) point out that it is unclear what exactly is meant by 'encoding' or at what level the 'representations' are built, as well as why infants need to complete them. There would seem to be additional difficulties concerning the very idea of 'completing a representation'. Complete relative to what? The obvious answers are either: the world – but that is what we are trying to represent in the first place; or memory – but these are new potential representations which is why they need time to be encoded in the first place.[50]

Schoner and Thelen

Schoner and Thelen (2006) provide their own model of habituation that incorporates and expands on the known empirical results: (1) declining interest with repeated trials; (2) an initial increase in response prior to the decline; (3) more complex stimuli slows the rate of habituation; (4) dishabituation is a function both of the dishabituating stimulus as well as its intensity/complexity; (5 & 6) a familiarity preference with few habituation trials and a novelty preference with many; (7) order effect of test stimuli produces asymmetrical preference responses; (8) dishabituation to novel stimulus can reinvigorate interest in the familiar stimulus; (9) non-specific activation boost can reinvigorate interest in the familiar stimulus; (10) individual differences in the rate of habituation determine rate of dishabituation. In so doing, Schoner and Thelen are able to successfully provides a comprehensive account of *all* of the findings surrounding Baillargeon's drawbridge procedure.

[50] The other general theory of habituation comes from the dual-process model (Thompson and Spencer, 1966) but is not pertinent to the current discussion.

Schoner and Thelen's habituation model is a particular instance of the more general class of dynamic field models (DMFs): "Field theories are based on the assumption that actions are the dynamic (and often non-linear) function of both the immediate stimuli in the environment and both the recent and longer term history of the system in similar situations (p. 277)"[51]. Given the empirical fact that the perceptual processes involved in the familiarity and novelty preference depend on the infant's recent habituation history and the assumption that perceptual aspects can be modeled by a metric space of varying activation strengths, DFT would seem ideally suited for investigating the alternative perceptual interpretations proposed for habituation procedures.

The DFT model simulated visual inputs through two coupled and interacting fields: the first is an activation field that drives looking. This field represents two metric[52] properties of the stimuli – their perceptual similarity and their activation strength. The second field is the inhibition field which drives looking away. It receives input from the activation field and thus represents the level of habituation for each type of stimuli. Importantly, it is the nonlinear interaction of these two fields driven by some perceptual properties of the stimulus along with timing that determine the level of habituation that takes place and subsequently the looking behavior associated with dishabituation during test.

After successfully modeling the looking behavior associated with basic habituation (i.e. initial increase in looking followed by attenuated interest, role of inter-

[51] For an overview of the dynamic field approach see Spencer and Schoner, 2003.
[52] Free of knowledge or meaning such that the perceptual dimensions (e.g. spatial position, direction of movement) of stimuli can have overlapping activation strengths to the extent that they are metrically close.

stimulus interval, and fast and slow habituators) and dishabituation (e.g. novelty and familiarity effects, and order effects) processes, the model was applied to, and accounts for, virtually all of the findings from the drawbridge procedure as it was used on both sides of the debate. Importantly, based on two empirically supported assumptions about the perceptual properties of the events: that while habituation and test events share some overlap, the 180 rotation provides more input (Rivera et al. 1999) and that the block boosts activation (Bogartz et al. 2000; Cashon & Cohen, 2000), Schoner and Thelen have answered Baillargeon's (2000) challenge to provide a coherent and comprehensive account of all the data[53].

The more elaborate, ontological, dynamic field model of habituation by Schoner and Thelen directly informs their deeper analysis of the systemic biases present in nativist studies. Specifically, they suggest that the tendency for infant experiments to take on a particular ad hoc quality derives from their disregard for the nature of habituation itself. Their model demonstrates explicitly how subtle changes in the stimuli or in timing can alter whether infant's behavior accords with familiarity or novelty preferences. Further, they point out that the number of potential displays that can be construed by adults as being about containment or support or object permanence is probably unbounded. From these two observations, it follows that the stimulus displays can be adjusted to produce

[53] Baillargeon actually claims that a researcher would have to show that all 30 experiments from a list of putative demonstrations of object representation are better accounted for by a single coherent perceptual interpretation to refute her position. However, the logic of this claim is simply incorrect. First, perceptual confounds are confounds whether or not the theoretical apparatus needed to unify them is available. Further, systematic conceptual and methodological errors do not require an exhaustive enumeration of its instances. Chomsky's (1959) in-principle rejection of behaviorism did not proceed by re-interpreting the vast literature of experimental studies, nor did Minsky and Papert's (1969) in-principle refutation of the perceptron require instantiating every possible model to determine what it was (and was not) capable of computing.

virtually any outcome one is looking for[54]. Ironically, this conclusion actually seems to be supported by one of the main features of Baillargeon's reply to critics. Specifically, that subtle methodological difference in perceptual aspects of the display (including habituation parameters) are responsible for the differences in outcome found by other researchers (Baillargeon, 2000; Wang et al. 2004). This creates somewhat of an unfortunate situation for nativist researchers because not only are the empirical results of their critics convincing (in terms of their non-cognitive conclusions regarding object permanence) but they have, incidentally, demonstrated the ad hoc character of experiments that don't have theoretically driven constraints on the (thought-to-be-irrelevant) habituation parameters. This potential limitation encompasses the majority of nativist studies given that habituation parameters have rarely been considered theoretically relevant by these researchers.

In sum, given some rather modest assumptions about perceptual activation and inhibition, Schoner and Thelen have been able to model effects that were designed to demonstrate conceptual knowledge. Further, they have been able to account for the subsequent results from critics of the original experiments as well as interpret other robust findings (e.g. order effects) that were either dismissed as theoretically uninteresting (Baillargeon, 1987) or left unexplained (Rivera et al., 1999; Cashon & Cohen, 2000). Finally, they have been able to accomplish all of this through an understanding of the non-linear *content-free* dynamics involved in the habituation process itself.

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[54] This isn't too suggest that researchers are intentionally engaging in such practices but rather that, through pilot, null result and previous studies, they have unknowingly set the relevant perceptual display and timing parameters such that they find what they are looking for.

Depth of critique and divergence of the alternative are reciprocally informative

The three sets of criticism discussed so far provide a progressive elaboration of the extent to which classic nativist studies systematically ignored perceptual level variables in their design methodology. In each case, the depth and nature of their criticism importantly influenced, and was influenced by, the scope and nature of their alternative solution. In particular, the criticism from Rivera et al. (1999) was limited to some specifics concerning amount of motion and the logic of what to expect in control conditions. Consequently, their solution was to eliminate an aspect of the procedure (i.e. remove habituation altogether). For Bogartz and colleagues, the nature of the problem was a systematic confound derived from not acknowledging any of the dynamics involved in the habituation procedure itself and so their alternative was a model of habituation that could accommodate what they took to be the relevant properties (familiarity/novelty). Consequently, their analysis concluded that the failure to consider habituation was most directly the result of designing the original experiments within the context of possibility/impossibility rather than one of familiarization and novelty. By providing a more robust model of habituation Schoner and Thelen (2006) exemplify more clearly the bidirectional influence between critique and alternative. Similar to Bogartz et al., they also developed their habituation model on the basis of an analysis that nativist studies were in error for not looking at the dynamics involved in habituation and that model was then used to examine how and in what specific ways ignoring habituation mattered. Their results further informed their critique regarding the specifics on how the habituation dynamics can produce the sometimes ad hoc character of looking paradigm research.

Conceptual criticism

Fischer and Bidell

At a more conceptual level of critique, Fischer and Bidell (1991) point out that it is from the fully competent adult perspective of an external observer that the events are characterized as possible or impossible. The implicit forced choice between these two categories entails that the looking methodology is intrinsically committed to a dichotomous present/absent conclusion regarding the concept of interest (e.g. object permanence, number, etc.). However, using the looking paradigm in this way presupposes a non-developmental perspective that precludes constructivist alternatives a priori. Fischer and Bidell (1991) highlight their point by arguing that the variability and gradual nature of cognitive developments means that behavioral findings cannot be taken out of the context of: (1) the developmental *sequence* before and after the ability of interest; (2) the developmental *synchronies* found in other domains; and, (3) the *cluster* of behaviors that "move" together with the target behaviors, without also removing the necessary constraints on generalizations concerning the abilities that such behavior is taken to represent. Only by designing dichotomous yes-no experiments that ignore developmental variability and are isolated from the context of sequence and synchrony are nativists able to presuppose that they know the meaning, for the infant, of the behavior under study and conclude that they have refuted Piaget's model of the object concept.

Irrespective of the failure to control for perceptual level variables, Fischer and Bidell remind us that Piaget's model of the object concept consisted of six developmental stages; as such, the findings from the drawbridge procedure are not inconsistent with his

model at all. Piaget had already demonstrated that 3-4 month old infants showed object-related behavior and so the obvious alternative interpretation of dishabituation in the drawbridge procedure is that it represents an earlier stage of object development. Only by collapsing (implicitly and a priori) the six developmental stages into the end state were nativists able to use their yes-no procedures to reject Piaget's model. That is, even ignoring all of the issues surrounding the intrinsic dynamics of the habituation process itself, Fischer and Bidell argue that the drawbridge procedure probably only requires that infants know that objects exist (stage 3)[55] and while this information is necessary for object permanence, it is not synonymous with it (note the consistency of this point with the anti-emergentist, anti-constructivism of foundationalism).

Meltzoff and Moore

Meltzoff and Moore (1998) argue a variant of this point in more detail. They differentiate between object representation, object identity, and object permanence. Their argument is essentially that young infants are able to represent objects before they are able to track the identity of objects, before they are able to "know" or "reason about" the permanence of objects and that "evidence" for the former has been conflated with evidence for the latter. Their ultimate conclusion is that infants are demonstrating the capacity for representational identity (that a representation of the once-visible object and its spatiotemporal parameters is maintained), not permanence, when they look longer during occlusion experiments. Meltzoff and Moore elegantly capture the issue concerning such conflation when they say "*it is important not to collapse the distinction between the persistence of infant representations and infants' belief in the permanence of*

[55] They consider another alternative hypothesis in which dishabituation behavior indicates a transition from stage 3 to stage 4.

external objects (p. 215)". There is actually a deeper issue here that falls under a general class of errors in which a property of a representation itself is assumed to be part of what is represented (See also Lawrence & Margolis (2005) on Gelman's use of the accumulator model????). For example, my representation of a dog presumably involves multiple bio-chemical properties of the brain, but those properties are not themselves part of the content of my representation of the dog. In the current situation, Meltzoff and Moore have argued that the property of persistence of the representation is assumed to be part of what is represented and thus the conflation between object identity and object permanence.

Importantly, it is only from the perspective of an external observer that the apparent instantiation of some property of an epistemic system could be assumed to constitute knowledge of that property. This assumption is a more general and insidious variant of the point made by Fischer and Bidell (1991) with their use of the term adultocentrism: "if an adult looking at an infant's behavior sees it as implying a concept of number or a concept of object, the inference is made that the infant must be using the concept (p. 210)". The general failure of nativists to conceptually differentiate levels of object knowledge (instead collapsing them all into the adult end-state) is indicative of the assumption that "because it is an object that the infant is seeing, therefore the infant must be seeing (it as) an object" (p. 364, Bickhard, in-preperation)". That is, the failure to conceptually differentiate stages of development is a direct consequence of assuming that the adult end-state is exhaustive of what it is to know the world at all (i.e. foundationalism).

Number

In the domain of number, some of the most influential and controversial nativist claims have derived from Wynn's (1992, 1995) calculation procedure. Although the current paper implies that there are no fundamental presuppositional differences between nativist proposals across object and number domains, the debate surrounding innateness of number knowledge is particularly valuable because the position fractures in ways that are more numerous and visible than with studies concerning object knowledge. The general reason for this greater transparency is at least twofold: first, numbers are inherently abstract whereas objects are concrete; and second, the age at which children demonstrate clear indications of numerical operations (i.e. addition and subtraction) occurs much later than that of the full object concept. In more general terms, understanding representation as a correspondence relationship means that number knowledge has nothing in the world to attach to; and second, innatist foundationalism is more difficult to maintain when development is protracted into early childhood. Before elaborating on these two aspects, some of the extant objections concerning strong innateness claims about numerical knowledge and reasoning will be discussed.

There are two relevant distinctions to be made with respect to number research. The first distinction is between studies that look at set size discrimination (Starkey & Cooper, 1980; Xu & Spelke, 2000, Wynn, 1996) and those that look at more complex tasks (addition and subtraction – the current focus). The second distinction concerns two general classes of objections to the numerical conclusions based on Wynn's "calculation" procedure (Wynn, 1992). The first group of objections (the current focus) consists of strictly perceptual level interpretations (Cohen & Marks, 2002; Mix 2002; Mix, et al. 2002; Clearfield & Westfahl, 2006; Clearfield & Mix, 2001; 1999; Wakeley, et al. 2000;

Kagan 2002) while the second includes some recourse to perceptual preferences but they are understood from within a framework that presupposes knowledge of objects as objects – the object concpept (Simon, 1997; Xu & Carey, 1996; Uller, et al. 1999; Feigenson, Carey, & Spelke, 2002).

Wynn's "calculation" procedure

In the canonical version (1992) of Wynn's task (1 + 1 = 1 or 2) infants first saw a single toy doll on stage followed by its occlusion, then a hand placed a second toy behind the occluding screen, the screen was then removed and there was either one toy (the impossible/violated outcome) or two toys (the possible/expected outcome). Outcomes with 1 or 2 dolls were alternated for a total of six trials across three blocks. From this basic procedure Wynn (1995) concluded that: "Infants can mentally represent different numbers and have procedures for manipulating these numerical representations to obtain further numerical information (p. 172)".

Object enumeration objections

Before examining the "strictly" perceptual processing interpretations of the Wynn procedure, consider the underlying rationale of the object-representation class of objections. This group accepts that infant's longer looking is the result of a violated conceptual expectation but denies that the nature of that expectation is based on numerical information. For them, it is the violation of a conceptual expectation about the physical world that drives looking behavior: during the procedure infants build up object representations for each doll and when there are fewer (or more) dolls in the current visual representation than "existed" in the infant's prior representation there is a violation, but it is based on their representation of objects not on numerical competencies

per se[56]. Simon (1997) rightly points out that while "infants' behavior is consistent with arithmetical operations based on cardinal representations of quantity, this does not mean that they posses the conceptual competence of number and arithmetic (p. 351)". This point is particularly relevant when there are other alternative possibilities that account for the number-like-behavior without recourse to number-specific knowledge. To assume that number-like-behavior requires number-specific knowledge is to preclude a developmental perspective that takes the emergence of knowledge seriously (i.e. emergent constructivism).

However, the basic logic and assumptions of the Wynn number procedure are fundamentally the same as that for objects: "since infants look longer at outcomes that violate their expectations, if they are anticipating the number of objects that should result [from the "addition" operation], they will look longer at the inconsistent outcomes than the consistent ones (p. 41 Wynn, 1995)". While for Wynn the expectation is about numbers per se and for the object representation group the expectation is about objects and their unexpected appearance/disappearance, both of these frameworks presuppose the unitization of the objects/events to be counted/represented.

However "entities" can be quantified in ways other than through the use of discrete number (Mix, et al. 2002). When total quantity is presented all at once (visually) there are a several continuous spatial cues (collectively called spatial extent) that are also available: surface area, volume, contour length, and density. Again, when total quantity

[56] Notice that, in the context of number knowledge, this position highlights the same issue that was raised by Meltzoff and Moore regarding the potential conflation between properties of a representation and the content of that representation. In the current situation this would be a conflation between the fact that *each* representation has the property of oneness with oneness being represented.

is presented sequentially (using audition or vision) there are other perceptual cues that co-vary with number: rate, duration, and rhythm.

Quantification: perceptual alternatives

Clearfield, Mix and colleagues

Clearfield and Mix (1999) have investigated whether the often confounded continuous variable of contour length could be responsible for looking preferences that were previously taken as evidence for number knowledge. Two groups of infants were habituated to sets of squares of size two or three. During test each group saw alternating displays of either the same number of squares but with different total contour length or different numbers (n +/- 1) with the same total contour length. The critical finding was that infants looked significantly longer with changes in amount of contour length but not for changes in number.

In a follow up study, Clearfield and Mix (2001) replicated their earlier findings (Clearfield & Mix,1999) and attempted to disentangle contour length from area. While it has been known for some time that when spatial extent is pitted directly against number, it is extent that determines infants looking behavior (Fantz & Fagan, 1975; Fantz, Fagan, & Miranda, 1975), the current results suggest that infants at this age are not sensitive to number at all, provided that amount has been controlled across conditions. Despite some studies having attempted to control for spatial extent variables (Starkey & Cooper, 1980; Starkey, Spelke & Gelman, 1990; Xu & Spelke, 2000) Mix et al's. review of the literature (2002) has argued that their controls did not adequately ruled out infant's use of non-numerical cues.

Perhaps some of the strongest empirical evidence for the conclusion that infant looking behavior must be the result of number of entities per se, that inherently avoids spatial extent variables, are studies using event sets (Wynn, 1996). These studies are event sequences that unfold over time (e.g. infants are habituated to a sequence of two or three jumps possibly within a continuous stream of motion) and while spatial extent variables do not apply (it is the "number" of jumps that is manipulated), rate, duration and rhythm variables do. In fact, Mix et al. (2002) point out that despite prior research demonstrating that infants are sensitive to changes in rhythm, it was never considered as a variable of potential interest. Clearfield (2004) later demonstrated empirically that looking behavior in Wynn's (1996) task could be accounted for by amount of motion and did not utilize enumeration processes.

Familiarization alternatives

While many studies concerning infant's knowledge of number have at least recognized, however modestly, the possibility that perceptual variables may be relevant, the calculation studies pioneered by Wynn (1992) almost completely ignore the possibility that uncontrolled perceptual aspects could be responsible for the results. Further, while the procedure itself does not attempt to induce a novelty preference via habituation, the potential impact of familiarization is left completely unaddressed. Having demonstrated the potential relevance of familiarization dynamics for object research, Cohen considered what influence that process could be having in number research (Cohen & Marks, 2002).

Cohen and Marks

The authors proposed that infant behavior in the addition and subtraction tasks could simply be the result of responding to familiar rather than to novel displays. In principle, the basis of such a familiarity preference could be either the number of objects (discrete variable) or the overall quantity created by those objects (continuous variable). The authors point out that for both conditions in Wynn's task, infants are exposed more often to the incorrect/impossible result: for the addition task they receive more exposure to the single doll (incorrect result of 1+1) and for the subtraction task they receive more exposure to the two dolls (incorrect result of 2-1). Consequently, "the conditions would seem optimal for infants to look longer at the impossible event, not because it is impossible, but because it is more familiar (p. 188)".

To test whether infants were responding on the basis of conceptually rich computations or instead were displaying a perceptual level familiarity preference[57], the authors included two additional displays during test in which infants saw 0 and 3 objects as well as the 1 and 2 objects typically presented. While the predictions from both perspectives are obviously the same for outcomes with the standard 1 object versus 2 objects [less looking at 2 (correct/unfamiliar) compared with 1 (incorrect/familiar) for addition and the opposite for subtraction] they are exactly opposite for outcomes with 0 and 3 objects for both addition (1+1) and subtraction (2-1) conditions[58] thus enabling

[57] The authors were also testing a third position advocated by Simon et al. (1995) in which infant behavior in the Wynn task was the result of directional same/different discriminations requiring some ordinal understanding.

[58] On the arithmetic account, outcomes of 0 and 3 are equally as impossible ($1+1 \neq 0, 3$; $2-1 \neq 0, 3$) as outcomes of 1 or 2 for addition and subtraction respectively (implying equally **long** looking at all three impossible conditions for both addition and subtraction). For the familiarity hypothesis, outcomes 0 and 3 are equally unfamiliar as outcomes of 1 or 2 for addition and subtraction respectively (implying equally **short** looking at all three unfamiliar conditions).

empirical differentiation between the two positions. The collective results from their study support a dual-process explanation in which a familiarity preference was superimposed on a preference for more items. Importantly, there was no support for an addition-subtraction plus more items hypothesis.

One of the standard rejoinders by nativists is to dismiss problematic findings on the basis of procedural differences (Baillargeon, 2000; Wynn, 2002). Wynn (2002) suggests that there were two crucial problems with Cohen and Marks study: failure to replicate fully her original results; and procedural differences. Wynn suggests that the former demonstrates that the task used by Cohen and Mark's is not tapping the same cognitive processes that are operating in her task and that such a failure is the result of the changes to the procedure.

Interestingly, Wynn herself does not always obtain results consistent with her cognitively rich interpretation, even with very modest variations on the original procedure. For example, Wynn (1995) did not find a meaningful difference in looking time across "possible" and "impossible" test displays for the "2+1" addition condition. However, not only does Wynn accept that the significant difference in the subtraction condition (3-1) as evidence for her calculation account but actually dismisses the relevance of the addition result: "Inferring the results of the addition in these experiments appears to be more difficult, *for some reason*, than inferring the results of the subtraction (Italics mine, p. 49)". Perhaps the mystery surrounding such results is an artifact of assuming that the infant is calculating in the first place (For a detailed summary of conditions within different studies that did or did not conform to a calculation interpretation see Wakely, Rivera, & Langer, 2000b). The real tradeoff of trying to

attribute contrary findings to small differences in procedure is that, to the extent that the subtle variations matter, the phenomena looses its generality (Cohen, 2002) and for what are interpreted as conceptual competencies, the phenomena looses its status as conceptual.

Clearfield and Westfahl

Whether or not Cohen & Marks (2002) pose a serious challenge to the calculation interpretation, they have, at minimum, demonstrated the relevance of familiarization in number research. Following up on their lead, Clearfield and Westfahl (2006) have also investigated the potential role of familiarization/habituation in infant addition research; however, they have eliminated the possibility of counter claims that procedural differences, from that of the original experiment, render their criticism inert. They accomplish this by using the same test display procedures as used in Wynn (1992). Consequently, the results of their first experiment replicated Wynn (1992) exactly[59] (i.e. for all three test blocks – for Cohen & Marks the replication was only for the first test block).

In their third experiment infants were familiarized/habituated to either 1 or 2 dolls prior to the standard test display procedure. The results indicated that infant's looking behavior was driven by a novelty preference irrespective of the impossibility/possibility

[59] Technically, it cannot be determined if their replication was exact. As Wakely et. al. (2000b) point out, in the original study, Wynn (1992) did not actually report the statistical differences between correct and incorrect addition or correct and incorrect subtraction (as would be natural given the interpretation). Instead, she compared differences between correct and incorrect addition with difference between correct and incorrect subtraction. In any event the results are statistically consistent with the rich cognitive interpretation that Wynn was attempting to demonstrate.

of the task[60]. That is, they looked longer at the possible event if they were familiarized/habituated to 1 doll and longer at the impossible event if they were familiarized/habituated to 2 dolls. Regardless of the details, the power of this study is derivative from the fact that prior exposure to 1 or 2 dolls can determine infants looking behavior to test displays irrespective of the "conceptual" content of that display. Indifference to conceptual content is precisely what one would expect if looking behavior in these situations is instead driven by perceptual level processes. Finally, notice that the impact of this research is not limited to Wynn's numerical interpretation only, it is equally relevant for the entire class of researchers who presuppose that their experiments involve impossibility with respect to violating object expectations; for both, the outcome of the test display is impossible (despite having different ideas about the basis of that impossibility – number/object).

Object nativists versus number nativists

Feigenson et al.

While the previous study provides direct evidence against an impossibility interpretation of the Wynn procedure, it only indirectly provides evidence that a familiarity preference was in fact driving behavior in Wynn's original task. The most direct evidence for what was driving infant looking comes from a series of experiments conducted by Feigenson et al. (2002). Clearfield and Mix (1999, 2001) used the habituation paradigm for a 2 vs 3 comparisons of 2-D blocks to demonstrate that when

[60] If taken in isolation, it could be argued that the novelty preference simply overpowered the violated expectancy of the impossible event; however, recall that it is standard in habituation studies to pair the novel test display with the possible event and nativist conclusions require that the impossibility of the familiar event overcomes any novelty preference to produce longer looking to the violation.

continuous variables (contour length and area) were controlled infants showed no sensitivity to number. Feigenson et al. (2002) elaborated on such findings by using a 1 vs 2 comparison of 3-D objects (similar to what is used in the Wynn task). Again, 3 of their experiments demonstrated that infants do not show sensitivity to number when surface area has been controlled. These finding are crucial because stimuli in all calculation tasks have been of a constant size.

After replicating Wynn's original (1992) findings in experiment 6, the authors proceeded to separate the number/extent confound by controlling for surface area such that total spatial extent was held constant across the numerically "impossible" outcomes and allowed to differ for the numerically "possible" outcomes. For example, in the 1+1 condition, one small doll was added to another small doll. At test, infants saw either one large doll (unexpected number – expected spatial extent) or two large dolls (expected number – unexpected spatial extent). Their results demonstrated that infants in the Wynn task are responding to changes in spatial extent and, importantly, they are not responding on the basis of an unfulfilled numerical expectation.

Consistent with other nativist attempts to construe perceptual level findings in such a way so as to be consistent with previous conceptual conclusions (Wang, 2005; Baillargeon, 2000; Wynn, 1998) Feigenson et al. suggest that infants are creating object-files for the objects that are involved and that the perceptual variables related to spatial extent are property information that is bound to the object-file. On this construal, what the current empirical results indicate is that infants choose to use the property information rather than their complete object representations. This proposal is indicative of the power

of the nativist ideology: if nativism must be true then virtually any empirical result can be construed in such a way that it will appear consistent with that framework.

Foundationalism applies equally to object as to number knowledge

The empirical research on number (especially the Wynn task) clearly highlights how the *idealization* required for preferential looking to succeed as a measure of *conceptual* knowledge may be a *practical* impossibility (Schoner & Thelen, 2006). Recall that the basic logic of the preferential looking paradigm (as used to probe conceptual knowledge) is suppose to be such that the only important differences between the two test conditions are with respect to the conceptual content of interest. For the study of object knowledge, the conceptual content seems to necessarily coincide with the physical medium that produces that content (i.e. the physical object). This means that perceptual level processes will always vary with changes in conceptual content and thus the obvious need to control for perceptual aspects of the display.

However, for number research there seems to be no necessary connection between the numerical content and the physical mediums that potentially manifest that content. Physical mediums can be given numerical values but those mediums are not themselves numbers. In other words, numbers are inherently abstract. That is, they are purely conceptual, and as such, number is the perfect candidate given the basic logic of the preferential looking paradigm. If infants are sensitive to visual displays that systematically differ only in terms of number then it must be on the basis of number that detection is taking place. The situation is exactly the same for the greater degree of abstraction involved in arithmetic operations. Addition is inherently abstract and lacks any inherent connection with the physical mediums that manifest it. Thus, if infants

demonstrate differential behavior for displays that systematically differ only in terms of correct and incorrect addition then it must be on that basis that infants are behaving.

The problem here is that it is the designer and their adultocentric (conceptual) perspective on the task that is determining the researchers conclusions about infant's differential looking – if it is a number/addition task then it must be testing number/addition. The further point is that, while for inherently abstract knowledge (i.e. when there is nothing in the world for the conceptual content to correspond with – number/arithmetic) the potential for overly rich interpretation is more apparent, the same potential is present for object knowledge and thus the "impossibility" that an infant can be looking at a toy block and not see it as such (anti-constructivism).

In sum, the basic logic required for nativist interpretations of infant preferential looking assumes that their displays are only about the conceptual content of interest; however, the empirical results to the contrary demonstrate that, in many cases, the conceptual content was only ever in the mind of the adult researcher.

Foundationalism applies equally to empiricism as to nativism

While emergent constructivists share with empiricists in their criticism of nativist infant research failing to control for perceptual aspects, they do not share with empiricist their commitment to foundationalism. Fondationalism is a common presupposition of empiricist and nativists positions with differences centered on the source of that foundation. For the nativists "all structure comes from within. Environment *reveals* this structure; it does not *imprint* its own patterns on the system (p. 12, Chomsky & Fodor, 1980)". For the empiricist structure comes from the environment and imprints itself upon us through experience. Therefore, whether the structures/contents of knowledge are thought to be *revealed* from within ourselves or discovered to reside in the environment does not

impact the underlying commitment to foundationalism. The specific methodological and conceptual problems with nativist research discussed above constitute one (contemporary) manifestation of nativist's commitment to foundationalism and while empiricist may disagree with nativists in the context of infant research, they do not themselves resolve any of the general problems with foundationalism.

Summary

That many experiments conducted by infant researchers to promote a nativist agenda failed to adequately control for perceptual level alternatives is a historical fact. The further demonstration, in many cases, that perceptual level processes can both account for and displace earlier nativist interpretations is an empirical fact. One of the central theses of the current article is that these nativist errors are highly motivated; and, that that motivation needs to be explicated in order for there to be a proper diagnosis and subsequent full transcendence of the ultimate limitations of this research. If valid, this insight would constitute a principled argument against both nativist and empiricist positions (given their mutual commitment to foundationalism) while compelling an emergent-constructivist, action-based alternative.

Section 3: The Conceptual foundations of the nativist program

Interestingly, the "empirical" displacement of Piagetian theory in developmental psychology was as much about the refutation of Piaget as it was about promoting a nativist framework. As suggested above, three interrelated themes coalesced to defeat Piagetian theory: (1) unfortunate misinterpretations of Piaget's account, in conjunction with (2) the competence-performance corollary of nativist arguments that, united with (3) the preferential looking methodology. Together these themes squeezed the emergent-

constructivism out of mainstream developmental theorizing leaving a nativist agenda as the primary benefactor.

Construing the preferential looking paradigm in conceptual terms opened the possibility for re-exploring early infant development. The looking paradigm had been used in previous decades to explore multiple facets of perception, but it was the nativist turn that justified exploration of all things conceptual. With the agenda set, most of the intellectual energy and creative insights concerned new methods for confirming the scope of what was innate. Not only were there opportunities to demonstrate competencies earlier than predicted by Piaget but also the exploration of the circumstances under which infants could and could not demonstrate their precocious abilities. The pragmatic productivity of the preferential looking paradigm was probably sufficient for subsequent generations of researchers to follow suit, but it was only able to reach such a pinnacle because of the conceptual resources provided by the competence-performance distinction and consequently the vacuum that was left with the absence of Piagetian theory.

The competence-performance distinction

A conceptual issue

The competence-performance distinction has been discussed earlier with reference to its natural tendency towards a nativist position. Recall, because any positive results were accepted as evidence for the presence of the competence in question and any negative results were attributed to the presence of performance factors, with the accumulation of experimental "evidence" the age of acquisition was systematically relocated to an early date (Chandler, 1991). Importantly, what is taken to constitute

positive or negative results has also depended on the a priori theoretical agenda[61] of the researcher which is why the competence-performance distinction was used both by, neo-Piagetians to reinforce Piaget's structural stage model, and also by nativists to refute his position more globally. However, the inherent flexibility of the distinction is precisely what makes it experimentally vacuous as an engine for interpretation. The self-serving and self-protecting aspect of the competence-performance distinction insulates competence models from empirical refutation and in so doing requires that competence models be evaluated on the basis of conceptual merit alone. That is, a conceptual analysis of competence as an account of cognition is required to evaluate those models. Unfortunately, however, the logic of the competence-performance distinction motivates a confirmation approach to science, thus, in conjunction with founationalism there was no room for a critical element in nativist infant research.

Sophian

In her attempt to move 'beyond competence', Sophian (1997) recognizes the inherent flexibility of the distinction. She points out that while used most powerfully as a framework for criticizing Piaget, competence-performance is equally applicable to findings of successful performance (appearing to have knowledge that one doesn't) as to findings of poor performance (appearing not to have knowledge that one does). Sophian concludes that the fundamental implication of this inherent symmetry is a central concern for how children generate the answers that they do. That the processes that underlie performance are equally as meaningful as the actual outcomes. Her central thesis is that "a much more interactive conception of the relation between competence and

[61] Whether explicitly acknowledge or not, though typically not.

performance is needed to understand cognitive development (p. 283)". Specifically that the conceptual structures that shape performance must themselves be subject to that performance. Performance is necessarily interactive and therefore constitutes our only access to the environment. If competence is not itself subject to performance then children cannot learn from their interactions with the world and developmental processes are excluded from the core of developmental theory. Sophian argues that as a consequence, competence models cannot account for the broader course of development precisely because they cannot account for learning that involves qualitatively new things[62] later in development. In essence, her argument is that learning new things requires that conceptual constraints change with development; interactive performance provides a natural basis for such changes; consequently, performance ought to be able to influence competence.

Of course, the "mad dog" nativist could agree with the validity of the argument while rejecting the premise that qualitatively new learning takes place. In fact, the resources of the nativist program preclude the emergence of qualitatively new knowledge in later development precisely because it has not accounted for the emergence of knowledge at all. That is, to the extent that the nativist program is not able to account for the emergence of initial core knowledge, it is equally unable to account for the emergence of new knowledge anywhere in development. Further, to the extent that the emergence of new knowledge later in development is accounted for, there is no obvious

[62] However, she does accept that competence models account for the acquisition for early knowledge.

reason why that account cannot be applied to our initial knowledge[63].

While the spirit of Sophian's analysis is consistent with the current discussion, it seems that she does not go far enough. The current position is that the ultimate problem with the competence-performance distinction derives from problems inherent to the ontology of competence models in general and Chomsky's (1965) model in particular. According to Campbell and Bickhard's (1986) analysis, it is not so much that competence models need to change with development as that they commit to a fundamental epistemological error in which they conflate and confuse systematic descriptions of task performances with explanations of how those tasks are accomplished. Competence models are descriptive accounts of the classifications of possible human performances. They classify the range of tasks that can be accomplished from those that cannot (the type of judgments that will be made from those that will not). Capacity descriptions, if correct, (i.e. correctly classify possible performances) provide an important constraint on subsequent theorizing in the sense that any proposed explanation must be able to account for the set of possible performances. However, it is an error to assume that capacity descriptions explain how the performances that they classify are accomplished. This reification of capacity descriptions into explanation (in terms of internal structures) is especially tempting when considering domains with near infinite possibilities because an adequate description must itself employ generative rules (as is the case for language).

At a more general level, this is an epistemological error that conflates a description of what is known with a theory of knowing. The assumption that a correspondence relationship exists between the structure of what is known and the

[63] If this sounds familiar recall that it is essentially the same set of points made by Bickhard (1991) in reference to Fodor's impossibility arguments.

knowing process commits to a ubiquitous epistemological error that is equivalent to positing foundational encodings, which are themselves incoherent (Campbell & Bickhard, 1986). Notice the overlap of this with the analysis of nativist habituation studies and their empiricist assumption that whatever the structure of the world that is the structure to which our knowledge corresponds. The familiarity is indicative of the underlying encodingism common to both.

Bickhard's Interactivist Model

Encodingism

Encodingism is the assumption that foundational representations are encodings (Bickhard & Terveen, 1995). Encodings are representational stand-ins that possess an epistemic connection with what they represent. However, they posses that connection, as well as their content, only in so far as some epistemic agent is present to provide it to them. That is, an encoding requires an epistemic agent to provide both its content as well as its connection (in the world) to what it represent (Campbell & Bickhrad, 1986). Bickhard's (1995) canonical example of a conventional[64] encoding relationship is the Morse code correspondence relationship (e.g. "…" stands-in-for "s"). Morse code is useful because dots and dashes can be sent across telegraph lines while characters cannot, but in all cases the encoding relationship requires an epistemic agent to provide the representational content to the characters (e.g. "s"), the pattern of dots and dashes (e.g. "…"), and the stand-in relationship between them (e.g. "…" means "s"). "In other

[64] Bickhard (in-press) notes that, despite Morse code being conventional, the same point applies for naturally occurring correspondence relationships (informational, nomological, etc.) as well. For example, suppose a neutrino count in some physics lab encodes fusion properties of the sun. That encoding relationship holds only in so far as the physicist already knows about neutrino counts, fusion properties of the sun and the informational relationship between the two.

words, encodings change the *form* of representations, but borrow the *content* from elsewhere, which entails that, in order for encodings to have content, that content must already be available elsewhere (p. 18; Bickhard, in-press)".

The need for encodings to have their content supplied to them from an external epistemic agent is precisely the implication of Piaget's copy theory argument. Piaget argued that our knowledge of the world could not be understood as somehow a copy of it precisely because one would need to already have knowledge of the original (the world) in order to make the copy; however, knowledge of the original is exactly the same problem all over again. Encodingism's inherent circularity as an account of the origins of content illustrates the problem with any version of foundationalism: their inability to account for emergence. That is, encodingism requires foundationalism because it precludes the possibility of emergent representation.

Further, it is an encodingist construal of representation that is at the heart of Fodor's impossibility argument. That argument turned on the idea that models of learning do not account for the generation of new representations but rather apply only to the confirmation/disconfirmation of already-present representations. What Bickhard's encodingism critique demonstrates is that Fodor's innateness conclusions (his foundationalism) follow from a characterization of representation that itself already precludes the emergence of content altogether. In the explicit context of the encodingism critique it becomes clear why Fodor's nativism does not solve the logical error concerning emergence of representational content – encodings inherently preclude it.

Epistemic contact versus epistemic content

Encoding models are assumed to posses their content in virtue of a correspondence relationship[65] with what they are taken to represent. These correspondences often take the form of a causal relationship between the object in the world and neural activity in the brain (via transduction). However, assuming that causal correspondences constitute representations of what those correspondences are about conflates the causal capacity to detect with normative knowledge of what those detections are about. Everyone will accept that the thermostat's sensitivity to temperature constitutes an ability to detect differences between above set point temperatures and below set point temperatures but that the thermostat does not have conceptual knowledge regarding what those detections are about – temperature. That is, despite the thermostat's sensitivity to temperature there is no representational knowledge involved. For the thermostat, it is agreed that detection does not constitute conceptual knowledge of what those detections are about. For humans, the potential to *also* have representational knowledge of what some detection is about is of course possible, but for encoding models, representational knowledge is assumed to be constituted by the detection itself. That is, encoding models inherently conflate a crucial distinction between epistemic contact (detection, differentiation) and epistemic content (knowledge, representation).

With respect to infant research, the conflation between contact and content strongly motivates a nativist interpretation of the experimental evidence. The fact that infant looking behavior differs between test displays indicates that they have detected a difference. Given that standard encoding models of representation conflate detection

[65] It makes no fundamental difference what type of correspondence relationship is assumed (e.g. informational, causal, nomoloigcal, structural, isomorphic, etc.).

with knowledge of what those detections are about, nativist conclusions that infants have representational knowledge regarding the content of what they detect follow naturally.

Reznick (2000) highlights the problem with this conflation in the context of categorization research. He illustrates how detection of some category does not constitute an understanding of the conceptual basis for it:

> As a *reductio ad adsurdum*, consider the category of things that causes a particular infant to develop an allergic rash or to smile. The infant responds similarly to all members of the category, there is no visual feature that category exemplars share, and the infant can emit this behavior without any cognizance whatsoever about the intension of the category (p. 64).

Not only does this example highlight the difference between contact and content but it also illustrates the relevance of the infant as a situated agent. Specifically, Reznick points out that when exemplars do not share perceptual properties (as is the case in the example above) the established equivalence of category members must involve the infant's interactions with those members. Reznick speculates on some of the interactive mechanisms that might form the basis of such established equivalences. For example, spatial propinquity might enable categorization on the basis of "things I can see from my crib". Alternatively, function can enable categorization on the basis of "objects that can be rolled", "objects that bounce, fit in the hand", etc. Reznick's discussion highlights three points that are central to the current analysis: (1) established equivalence does not require/constitute knowledge of what the equivalence is about; (2) the mechanisms for established equivalences concern system internal states; (3) as a consequence of (2), established equivalences are system accessible.

Implicit definition

Bickhard (1995) provides a model of interactive differentiation that captures Reznick's notion of established equivalences – more broadly however, it is a model of our epistemic contact with the world. Consider that any interaction of a system with its environment will depend in part on the nature of the system and in part on the nature of the environment. As such, the internal outcome state of the system (after the interaction with the environment) will serve to categorize those types of environments that leave the system in that internal outcome state from those that leave it in some other internal outcome state. For simplicity, consider a system that has only two internal outcome states, A and B. Interactions with certain environments will leave the system in internal outcome state A while interactions with other environments will leave it in outcome state B. Thus internal outcome states A and B serve to differentiate A-type environments from B-type environments. Importantly, the system has no knowledge of the environments that it has differentiated and consequently "... detection of an A-type environment is just differentiation, not a representation (p. 39; Bickhard, in-press)".

Epistemic contact with the world in terms of internal states of the systems has the crucial benefit that these states are system accessible. Their accessibility means that they can be useful to the further functioning of the system and useful in such a way that is intrinsically sensitive to the current environment. It is through the indication for potential further functioning of the system that representational content is emergent from interactive differentiation. Bickhard (in-press) elaborates on the emergence of representational content from differentiated contact:

> It might be learned, or hard-wired, for example, that, if state A is encountered, then an indication of the possibility of tongue flicking and eating of a particular sort can be set up. Such an indication is future oriented, anticipatory, and, therefore, involves content: it is about the current environment, and it could be true or false. But, to reiterate, setting such an indication up should be contingent on having engaged in a prior differentiating interaction with the right kind of internal outcome (p. 39).

Haith's Challenge

A major aspect of Haith's (1998, 1997) criticism of nativist research also turns on his recognition of problems that derive from failing to distinguish between contact and content. Haith suggests a distinction be made between representation as "sensory encoding" (when energy undergoes a transformation) and representation as "symbolic representation" (images or schemas used to make inferences and create beliefs). On the basis of this distinction Haith suggests that the relevant question for infant researchers to ask is when one is dealing with sensory encodings following occlusion events versus symbolic representations recovered from memory involving potential manipulation. While this distinction is able to illuminate the important difference between detection (sensory encoding) and representational knowledge of what those detections are about (symbolic representation) it does not consider the possibility that encodingism is present in both. That is, sensory encodings could be understood simply to differ from images and schemas with respect to the nature and complexity of the correspondence relationships involved.

Regardless of the completeness of Haith analysis he is quite right that confusion regarding the concept of representation is at the center of many of the issues concerning infant cognition.

> Now that we recognize that even fetuses "do it", the need for a full-scale developmental model of representation that incorporates the notion of partial accomplishment is obvious. We can not get by with a single term whose meaning spans the full distance from energy transformations in the CNS to mental manipulations of symbols (p. 175).

The relevant question for Haith is two-fold: (1) which form of representation do infant studies reveal? and (2) what does a graded account of representation underlying partial accomplishments look like? The discussion up to this point has attempted to provide a conclusive response to the first question with the answer involving perceptual level "representations". The second has yet to be addressed directly and will be used to illustrate explicitly the sense in which non-nativist (broadly empiricist) approaches are equally committed to foundationalism.

Graded Representations

Abilities appear gradually

There are many authors who have pointed out that theories of development must account for the fact that children's abilities appear gradually over time. However, it is important to keep in mind the distinctions made by Keil (1981) regarding the different possibilities that could produce such gradual "development". Keil points out that there are at least three logical possibilities, however the current discussion need only consider two of them: first, the fully mature competence/representation/knowledge is present from

an early stage and development is primarily a process that involves increasing access. Increasing access is itself cashed out in terms of *ancillary deficits* involving general processing abilities (e.g. memory, means-end ability, inhibitory control, etc.). Second, the competence/representation/knowledge itself undergoes non-maturational transitions resulting in partial forms. These partial forms are themselves "complete" in that they fully enable certain interactive possibilities but are talked about as partial because they still preclude other possibilities that are considered part of the adult form.

Part of the reason that nativist proposals are explicitly (or implicitly) advocating the all-or-none presence of various competencies is precisely because they presuppose the non-developmental-increasing-access possibility mentioned above. Without a developmental account of partial forms there is no possibility except complete absence or complete presence. Further, Aslin and Fiser (2005)[66] point out that the looking-time methodology used in most nativist research is itself constrained to providing "a Yes-No answer to a given research question (p. 92)". This doesn't mean that truly developmental questions cannot be asked in principle, but that the methodology employed, lends itself to a construal of knowledge that is also all-or-none (present or absent).

In summary, the above difficulties do not mean that nativist positions have to deny the gradual "development" of children's ability to interact competently; however, it does mean that they are committed to an all-or-none stance on whatever competency they are "testing" to be innate[67].

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[66] Recall that a similar point was made in our earlier discussion involving Fischer and Bidell (1991).

[67] At the conceptual level, this commitment is ultimately a consequence of the deeper assumption regarding foundationalism. A genuinely developmental account explains the emergence of forms which is precisely what foundationalism precludes.

The adaptive process account (APA)

With respect to graded representations of objects, the meaningful divergence between nativists and non-nativists is whether the object representations themselves can have partial forms. Munakata and her colleagues (2001, 1997) have attempted to reconcile some of the seemingly contradictory findings that have emerged from the infant literature[68] with their notion of graded representations. They have looked at situations in which "Two tasks supposedly tap the same knowledge, but the same infants succeed on one and fail the other? … [asking specifically] Why do infants fail to retrieve hidden objects until 8 months and even then show the A~B error, if they have a concept of object permanence many months earlier? (p.686, Munakata, McClelland, Johnson & Siegler, 1997)". After demonstrating that the standard means-ends deficit theories of children's failure to retrieve hidden objects is not sufficient, they proceeded to explain the looking-reaching task dissociation in terms of their Adaptive Process Account (APA). The central idea of this approach is that representations are graded in nature and it is this property that explains the task dissociations. Specifically, representations become stronger throughout development and it is the strengthening of these representations that enables the infant to perform competently on some task but not on others. For example, a weak representation of a hidden object may enable infants to succeed on various looking time procedures, while still not being sufficiently strong for them to succeed on procedures that require them to reach for an object (due to the increased complexity required for reaching).

[68] Munakata (2001) actually discusses a number of domains involving behavioral dissociations between measures.

Using and implementing the notion of graded representation to reconcile the apparent conflict between different measures of infant competence is both provocative and insightful. The author's proposal is explicitly contrasted with what they call principle-based approaches (competence models) in which "knowledge takes the form of principles that function like propositions: that is, the principles are construed as generally accessible inputs to a reasoning system (p. 687, Munakata, 1997)". In general agreement with the earlier discussion of Campbell and Bickhard (1986), they go on to suggest that, while the use of these principles as a description of behavior might be potentially useful, the danger of such an approach " …is the tendency to accept these descriptions of behavior as mental entities that are explicitly accessed and used in the production of behavior (p. 687)" (i.e. conflating description with explanation).

In response to these concerns, the authors offer an alternative conceptualization of knowledge that purports to provide the epistemic foundation for their account. Broadly speaking, this alternative conceptualization seems to shares some affinities with an action-based approach. In particular the authors talk about knowledge as "evolving", "experience-based", "embodied" and guided by learned "representations". Further, their general idea that representations themselves develop and that such development enables differential interactive possibilities (sucking, looking, reaching, etc.) flirts with the core of an action-based approach. However, it is suggested here that the account in fact suffers from, and is ultimately damaged by, its commitment to the parallel distributed processing approach that is itself set within the broader information-processing framework.

The connectionist model

The APA approach to representation can be understood as a connectionist variant of Sokolov's (1963) notion of "completing the representation" mentioned during our discussion of information-processing models of habituation (Bogartz, Shinskey & Speaker, 1997). Recall there, that the completeness of the representation was used to explain why infants show the familiarity and novelty preferences that they do (incomplete encoding results in a familiarity preference, while complete encoding results in a novelty preference). Recall also that Schoner and Thelen (2006) raised a number of concerns regarding the very idea of 'completing a representation' (most importantly, complete relative to what?). The obvious answer was, of course, the world; but that is the problem of representation all over again. Despite the time difference (perceptual time v.s. developmental time) notice the general parallel with the APA: weak (incomplete) representations result in looking behaviors while strong (complete) representations result in reaching behaviors. The APA is far superior in that it attempts to provide an explicit and detailed explanation of what strong and weak representations could possibly mean as well as the mechanism for why they develop in that way. However, to reiterate a point made earlier, while connectionism is championed to have overcome the representational limitations of discrete homuncular symbols through their notion, and implementation of, distributed representations, it does not transcend the basic representational issues concerning content (Bickhard & Terveen, 1995) and is thus, in this regard, no better off than any of the other information-processing models.

Distributed representations are still encodings

Specifically, distributed representations are constituted by activation vectors that correspond to aspects of the world. They can be trained to "extract"[69] information (correlational correspondences) that may in fact be present in the environment (i.e. the input patterns) but their purported knowledge of what those correlations are about is necessarily from the perspective of an epistemic agent that can already interpret the correspondence relationship. That is, connectionist networks are committed to an encodingist model of representation. They differ from the classic symbols of GOFAI in that classes of inputs patterns can be *trained* to settle into the same output pattern which then enables them to *generalize* to novel inputs (Bickhard & Terveen, 1995); however, the meaningfulness of any such categorization depends entirely on an external observer (both so that training results in the "correct" classification and also so that those correspondences are contentful). Connectionist networks are trained transducers that are "at best differentiators of classes of environments – in general, passive, non-interactive differentiators (p. 238; Bickhard, 2001)". To assume that connectionist networks represent what they differentiate is precisely to conflate epistemic contact with epistemic content.

The graded representational aspect of the APA is fundamentally constrained by its commitment to the broader connectionist framework[70] and that framework's failure to

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[69] Extract is in scare quotes because the experimenter has already done much of the epistemic work required by an actual system, in the actual world, in order for the input to be as it is. That is, networks often extract correlational regularities from input classes that have already been structured by the experimenter as though that structure is trivially present in the environment. Of course this assumption is precisely one of the sense in which foundationalism is present in contemporary empiricist frameworks.

[70] The authors explicitly acknowledge that their proposal is motivated by both PDP and cognitive neuroscience frameworks, though encodingism is equally present in their understanding of the latter as well: Munakata & Johnson (2005) define representation as

provide an adequate (non-foundationalist) model of representational content. The degree of differentiation of the input classes may occur gradually over time with increased robustness to noise, but that does not constitute a *graded representational* ontology. Further, despite the generally action-based attitude regarding the relevance of embodiment, PDP networks are typically non-interactive, passive systems that "have yet to fully meet the challenge of taking development seriously (Elman, 2005; p. 114)". Crucially, they do not take seriously the idea that action is essential and intrinsic to the ontology of representation itself and thus have few resources available to take seriously an emergent developmental perspective.

In sum, the APA constitutes an alternative interpretation of the seemly contradictory results regarding different measures of object permanence (i.e. looking vs reaching). It was a reaction to the principle-based (broadly nativist) approach and its attempt to use the ancillary-deficit-hypothesis to reconcile the conflicting result from these measures. More deeply, it was also a reaction to the characterization of knowledge that was argued to underlie principle-based proposals. In contrast, the APA attempted to take seriously the idea that knowledge itself develops gradually over time and, as with any non-nativist proposal, must ultimately answer the question of where representational content comes from throughout development. That is, rejecting nativist positions still requires some account of the source and development of representational knowledge.

Contemporary Empiricist models

As a consequence of the historical fact that nativist proposals in the 70s and 80s severely dampened the accepted legitimacy of Piaget's emergent constructivism, the most

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"… patterns of activation within the brain that correspond to aspect of the external environment (p. 154)".

common alternative to nativist positions were, and are, broadly empiricist: the world presents itself to us in experience and we somehow (typically through induction and or abstraction) derive knowledge from that presentation. Mandler (1992, 2000, 2004; Mandler & McDonough, 1993) offers one of the most comprehensive of such broadly empiricist proposals. Although for somewhat different reasons, Mandler shares with nativism in her rejection of Piaget's emergent constructivism. In its place she attempts to provide an account of how conceptual representational content is generated from an innate mechanism designated by Mandler as *perceptual analysis*. Muller and Overton (1998) provide a detailed critique of Mandler's position, that they contrast with their own action framework, in an effort "to illustrate the manner in which meta-theoretical contexts influence the theoretical, methodological and empirical debate (p. 104)". As such, the current article can be understood as an elaboration and extension of the same type of project.

Summary

For Mandler's model in particular and other empiricist proposals in general, the inherent tension with attempting to account for the origins and development of representational content derives from the fact that empiricist accounts are equally as committed to foundationalism as are nativist proposals. Whether representational content is present at birth and comes from the genes, or is imprinted over developmental time and comes from the environment, makes little fundamental difference to the issue of emergence. The differences that do occur derive from how foundationalism manifests itself in the specifics of different disciplines (psychology, philosophy, neuroscience, etc.),

domains (development, social cognition, language), research programs (developmental nativism, computational models, dynamical systems theory) and particular models.

Conclusion

The current article has attempted to detail the sense in which foundationalism matters to psychology. As a fundamental epistemological assumption it precludes emergence, and in so doing violates naturalism. A major portion of the current article has attempted to explore the specific implications of foundationalism for the primary methodology (looking paradigms) used by nativist researchers along with their interpretations of empirical data. Further, the arguments presented here have suggested that understanding foundationalism was one of the keys for liberation from the nativist-empiricist debate; however, developmental nativist assumptions and research has systematically precluded the only framework (Piaget's emergent constructivism) that was potentially able to offer it a "third" way. That is, the developmental nativist framework has begged the question against Piaget's action-based approach (rather than just influencing research as all frameworks do) precisely because it precluded relevant constructivist alternative possibilities a priori.

The specific senses in which developmental nativism begs the question crosses both conceptual and methodological boundaries, though of course these categories are not mutually exclusive. Conceptually, nativist proposals are problematic because of their necessary commitment to foundationalism. Methodologically they are problematic because of their supposedly a-theoretical construal of habituation and their failure to control for perceptual level variables. And cutting across both, is their biased application of the competence-performance distinction.

While the main alternative to nativism is empiricism, both are equally committed to foundationalism and simply suffer from different manifestations of the same fundamental issues. One of the main theses of this article has been to argue that the reason recent attempts to transcend the nativist-empiricist debate have failed is precisely because they do not recognized the deeper commitment to foundationalism and its reciprocal entailment relationship to encodingism. The difficulty in ever discovering that foundationalism and encodingism may underlie many of the debates in psychology is threefold: (1) foundationalism is auto-protective in the sense that it precludes the very framework that can help demonstrate its inadequacies; (2) attempting an analysis of foundationalism (encodingism) requires accepting that conceptual level explication is a legitimate part of scientific psychology; and (3) foundationalism and encodingism are related to "the issues" of psychology in non-obvious ways.

If correct, the ultimate conclusion of the current article is that psychologists should adopt an action-based emergent constructivism. Doing so would require a considerable amount of conceptual work, that on analogy with physics, would justify the creation of theoretical psychology as a proper sub-discipline of psychology itself.