Innateness as Closed Process Invariance*

Ron Mallon and Jonathan M. Weinberg†‡

Controversies over the innateness of cognitive structures play a persistent role in driving research in philosophy as well as cognitive science, but the appropriate way to understand the category of the innate remains in dispute. The invariantist approaches of Stich and Sober face counterexample cases of traits that, though developing invariantly across different environments, nonetheless are not held by nativism partisans to count as innate. Appeals to canalization (Ariew) or to psychological primitiveness (Samuels) fail to handle this liberalism problem. We suggest a novel approach to innateness: *closed process invariantism*.

Controversies over the innateness of cognitive processes, mechanisms, and structures play a persistent role in driving research in philosophy as well as the cognitive sciences, but the appropriate way to understand the category of the innate remains subject to dispute. One venerable approach in philosophy and cognitive science merely contrasts innate features with those that are learned. In fact, Jerry Fodor has recently suggested that this remains our best handle on innateness: "the polemical situation between rationalists and empiricists is really entirely symmetrical: nativism is merely the denial of empiricism insofar as we lack a way of saying what 'innate' comes to other than *not learned*. Likewise, empiricism is merely the denial of nativism insofar as we lack a way of saying what 'learned'

*Received August 2005; revised April 2006.

†To contact the authors, please write to: Ron Mallon, Department of Philosophy, University of Utah, 260 S. Central Campus Dr., Room 341, Salt Lake City, UT 84112; e-mail: rmallon@philosophy.utah.edu. Jonathan M. Weinberg, Department of Philosophy, Indiana University, 1033 E. Third St., Sycamore Hall 026, Bloomington, IN 47405-7005; e-mail: jmweinbe@indiana.edu.

‡We are grateful to Colin Allen, André Ariew, Fiona Cowie, Stephen Crowley, Stephen Downes, Seth Jones, David Landy, Dominic Murphy, Shaun Nichols, Anya Plutynksi, William Ramsey, Kelsey Rinella, Georg Theiner, and two anonymous referees for generous comments on earlier drafts. The order of the authors' names is arbitrary. This article is thoroughly collaborative.

Philosophy of Science, 73 (July 2006) pp. 323–344. 0031-8248/2006/7303-0001$10.00
Copyright 2006 by the Philosophy of Science Association. All rights reserved.

https://doi.org/10.1086/515414 Published online by Cambridge University Press

comes to other than *not innate*" (Fodor 2001, 101; emphases in original). If Fodor is right, the concept of the *innate* is used merely as a placeholder, offering little illumination.

Unsatisfied with this answer, recent discussions of innateness have pursued two alternative approaches to understanding the category of the innate. The first alternative (see Stich 1975; Ariew 1996, 1999; Sober 1998) identifies innateness with a sort of developmental invariance. The most basic form of *invariantism* holds that a trait will be innate to the extent that it would robustly develop in the face of wide variation in the environment. If this approach could be applied to human cognition, it would have the advantage of offering a single account of innateness applicable across the study of the mind and biological development (Sober 1998). Unfortunately, as we discuss below, simple versions of invariantism appear too liberal to make sense of disputes in the empirical study of mind, as the approach seems to count a variety of learned or otherwise environmentally acquired characteristics as innate (Ariew 1999; Samuels 2002). André Ariew (1999), however, has defended a *canalization* account of innateness that he claims successfully negotiates the problem of liberalism.

A second alternative to identifying the innate with the unlearned is provided by Richard Samuels's (2002) recent defense of *primitivism* as an account of innateness for the sciences of the mind. Primitivism, first suggested by Fiona Cowie (1999) as an interpretation of Fodor's nativism,[1] suggests that we understand as innate those structures or mechanisms lacking an explanation within psychology.[2] On this account, the claim that a trait *t* is innate is what Cowie calls "an expression of metatheoretical gloom" (1999, x) regarding the prospects of a psychological explanation of the acquisition of the trait.

One aim of this article is critique. We argue that Ariew's understanding of innateness as canalization and Cowie's and Samuels's primitivism are both fundamentally insufficient approaches that fail to articulate the distinctive theoretical role of innateness in the contemporary study of human cognition. Our second aim is constructive: we offer an alternative account of innateness that we think escapes the problems besetting these two accounts. Our aim in doing so is to identify what Richard Boyd has called a *programmatic definition* of innateness suited to the purposes of the cognitive sciences, one that specifies a certain "inductive or explanatory role"

1. It is Samuels who labels the view 'primitivism'. (Cowie offers several labels, including *nonnaturalism* [e.g., 1999, 64].)

2. Cowie suggests nonnaturalism as an *interpretation* of a number of historic and recent figures in philosophy and psychology, including Fodor. In contrast, Samuels's aim is to offer a host of independent reasons for employing primitivism as the correct account of innateness for the cognitive sciences (Cowie 1999, 111; Samuels 2002, 248–250).

https://doi.org/10.1086/515414 Published online by Cambridge University Press

that a term plays within a discipline (1999, 149). In this project, we join others we discuss, including Ariew (1996, 1999), Block (1981), Samuels (2002), Sober (1998), Stich (1975), and Wimsatt (1986, 1999). Just as they do, we draw upon a variety of resources—historical, conceptual, and empirical—to constrain and argue for a proposed account of innateness. Unlike Wimsatt (1999, 15–17), we do not feel particularly constrained by ordinary or folk intuitions about innateness. Such intuitions may be confused (cf. Griffiths 1997, 59ff.) and, even if clear, may be of little relevance to ongoing discussions in the sciences of the mind. We will begin by briefly setting forth the invariantist and primitivist accounts in order to suggest what their respective shortcomings are and, thereby, to motivate our own account.

1. Invariantism. The core idea of invariance accounts is to identify the innateness of a trait with its stable development in diverse environments.[3] We can think of development as guided by processes such that:

> (**D**$_\alpha$) Organisms of type O develop traits of type T in environment α.

Developmental generalizations like (**D**$_\alpha$) do not require that any organism of type O has ever actually been in an α environment but only that there be a fact to the matter about what such an organism would have developed in such an environment.

Invariance accounts begin by identifying, for a given trait type and a given organism type, the set A of environments, such that we consider developmental outcomes in these environments relevant to whether or not a trait is innate. They then hold that:

> (**I**) A trait of type T is innate for an organism type O to the extent that (**D**$_\alpha$) is true of T and O in a larger proportion of the relevant environments in A.

3. We mean for 'trait' to stand in for a broader class of properties that an organism might have than perhaps is customary within biology. For example, on our usage, particular beliefs will count as traits. We needed a simple catchall word to capture the wide range of things that one can argue about being innate, and 'trait' struck us as the best candidate. But we invite the reader to substitute 'characteristic' or 'property' for 'trait' throughout the remainder of the text, should this usage seem untoward. Nothing in our discussion hangs on the terminology. (We are grateful to Stephen Crowley for drawing our attention to this issue.)

https://doi.org/10.1086/515414 Published online by Cambridge University Press

As others have noted (e.g., Ariew 1996, S25; Sober 1998, Section 1), such an approach makes innateness a matter of degree.[4]

1.1. The Liberalism of Invariance Accounts. The most serious objection (advanced by Ariew [1999] and Samuels [2002]) against simple invariance accounts is that they are too liberal. In particular, there are traits that develop in a stable manner across a variety of environments that current partisans on both sides of the innateness debate would categorize as acquired.[5] Samuels focuses on a case of belief: "it is very plausible to maintain that pretty much every human being acquires . . . the belief that water is wet" (2002, 243). Ariew offers an alternative, nonpsychological example: "Humans typically possess an abundant supply of a particular species of bacteria *clostridium difficile* (*c.diff*.) in our intestines. Humans are not born with *c.diff*.; we typically acquire it by ingesting food and water . . . so, intuitively, the possession of *c.diff*. is not innate. However, in the normal course of events humans eat and drink water and hence acquire *c.diff*. . . . It follows on Stich's . . . [invariance] account, that *c. diff*. is an innate disease of the intestines. This is an unfortunate consequence of Stich's account" (Ariew 1999, 133).[6] In each of these cases, the trait in question develops invariantly across the range of typical environments, so simple invariance accounts are committed to counting each trait as innate. But Samuels and Ariew take it as obvious that the respective traits are acquired.[7] In joining them in this judgment, we reject the option of simply grouping such universally learned beliefs together with central examples of the innate (as in Wimsatt's [1986] "generative

4. The idea is that developing across all the environments in *A* would make *T* maximally innate. It is less clear how far away from that it needs to fall in order to count as maximally noninnate. Developing in none of the environments in *A* is clearly too strong, since that really amounts to its being innate that the organism lacks the trait.

5. Note that in our usage the *acquisition* of a trait specifically rules out that trait's innateness, whereas the mere *development* of a trait is meant to be agnostic on the trait's innate or noninnate status.

6. Ariew draws the example from Wendler (1996).

7. Relatedly, Samuels argues that simple invariance accounts misclassify the positions of paradigmatic psychological theorists, thereby failing to capture what is at stake in debates over the nature of mind. Samuels recalls Piaget's view that cognitive development occurs in a series of stable developmental stages that are highly invariant and concludes that, on a simple invariance account, "Piaget would appear to be a nativist!" (2002, 245). Notice that this is another manifestation of the problem of liberalism. Any too-liberal account of innateness will misclassify theorists, for any theorist who endorses the existence of noninnate traits that are stably acquired in the course of development (be it the trait *believing that water is wet* or some Piagetian cognitive structure) thereby endorses an account of a trait as *acquired* that a simple invariance view would count as innate.

https://doi.org/10.1086/515414 Published online by Cambridge University Press

entrenchment" approach). While such sangfroid may seem attractive to an invariantist, it renders the category of the innate too far removed from the concerns guiding research in cognitive science to be of interest. In what follows, we use both examples to fix our discussion, noting where they pull apart.[8]

1.2. Ariew, Canalization, and Liberalism. Ariew (1996, 1999) attempts to improve on the invariantist proposal by drawing on C. H. Waddington's account of developmental canalization.[9] He begins by offering the following diagnosis of invariantism's liberalism problem:

> Both Stich and Sober fail to recognize that there are two ways in which a trait emerges (invariantly) in a (e.g. normal) course of development, namely (both conditions come from Johnston 1988, 420):
>
> (1) By means of strict genetic control over development so that the outcome of development is *insensitive* to the conditions under which it occurs. Such outcomes are said to be strongly canalized against environmental perturbation.
> (2) By means of a developmental sensitivity *only* to environmental factors that are themselves invariant within the organism's (e.g. normal) developmental environment. (134)

This diagnosis of simple invariantism strikes us as basically right: the simple invariance account fails because it pays no attention to the etiology of the emergence of phenotypic traits. Any account that improves over the Stich and Sober accounts must attend not just to under what circumstances the trait develops but by what processes it develops—not just the

8. We take it that these examples are not folk intuitions about cases but, rather, expert judgments on the part of players in the innateness debates. We take it that both Samuels and Ariew agree with the constraint imposed by such cases, though both put their point in conceptual terms that do not illuminate the issue. As in the passage quoted in this paragraph, Ariew seems to hold an account of innateness responsible to our intuitions about such cases, though elsewhere he takes these to be constrained by certain theoretical desiderata (1996, S20–S21). Samuels pursues the more felicitous route of *stipulating* that the concept *innate* excludes learned traits, calling it the *fundamental conceptual constraint* (2002, 236).

9. Ariew (1996, 1999) speaks almost exclusively of canalization simpliciter, but one can distinguish two different kinds of canalization: environmental and genetic canalization. The former refers to developmental robustness in the face of environmental perturbations and seems to be what Ariew has in mind. (In his 1999 definition of innateness, he mentions "environmental canalization" only to drop reference to it a moment later [1999, 128]). 'Genetic canalization' refers to developmental robustness in the face of genetic mutation.

https://doi.org/10.1086/515414 Published online by Cambridge University Press

'when and where' but also the 'how' of acquisition. The question arises, then, as to what resources Ariew might bring to bear so as to avoid suffering the same fate as simple invariantism. He suggests that canalization will allow him to make the necessary distinction, as indicated by how he continues the above passage: "The second outcome is not canalized in Waddington's sense but is invariant under the normal conditions of development. . . . Although the outcome is invariant it is acquired, not innate and not canalized" (1999, 135). Ariew's strategy is thus to identify innateness with canalization, where "the degree to which a developmental pathway is canalized is the degree to which development of a particular endstate (phenotype) is insensitive to a range of environmental conditions under which the endstate emerges" (128). Ariew's preceding discussion introduces the idea of development, which is sensitive to various environmental parameters over the course of development. He concludes that "the more sensitive to the environmental conditions the developmental system is, the less inclined we are to say that the developmental endstate is canalized" (134). His idea, we take it, is that the development of a trait is more or less insensitive to environmental perturbation, and this degree of insensitivity *just is* canalization or innateness.

Yet, so far, this adds little to the simple invariance account. While he claims that traits produced by an invariant environment are not "canalized in Waddington's sense," it is hard to see how the account of canalization Ariew provides is any improvement over, for example, Sober's simple invariance thesis that identifies phenotypic traits developing "in all of a range of developmental environments" as innate (1998, 795). For the traits that provoke the concern that invariance accounts are too liberal, it is true to say that they develop in a way that is "insensitive to a range of environmental conditions under which the endstate emerges," apparently satisfying Ariew's account of canalization. Ariew has correctly diagnosed the source of the liberalism problem, but he has not solved it.[10]

While Ariew is not clear about how he thinks Waddington's account of canalization might address the problem of liberalism, we can think of two lines of solution he might pursue, turning on different ways of understanding Ariew's phrase "strict genetic control."[11] First, one might think that a solution lies in the idea that the development of some traits are under more direct genetic control than that of other traits; the former, but not the latter, are innate. However, there are at least two problems

10. Because our claim here is that Ariew's (1996, 1999) account is incomplete, it is open to him to develop his account in ways that address the liberalism problem more explicitly, perhaps in line with the options we suggest here or alternatively in the spirit of our own proposal below.

11. This language comes from Johnston (1988).

https://doi.org/10.1086/515414 Published online by Cambridge University Press

with this approach. First, the obvious way to make sense of such genetic control is via the notion of causal invariance.[12] This is, for example, the way Johnston (1988, 419–420) seems to have understood the idea, distinguishing characteristics that are strictly under control from those that are not by reference to whether the trait develops in the face of environmental perturbation.[13] On construals such as those, however, the notion of a genetically controlled trait simply collapses back into a simple version of an invariantly produced trait, without providing any help in distinguishing invariant outcomes that are canalized from those that are not. We thus have no solution to the problems facing simple invariance accounts advocated by Stich and Sober. There is also reason to be skeptical that any account of strict genetic control is forthcoming, since every developmental process exploits multiple genetic and nongenetic resources. In addition, it remains unclear what genes are such that they might figure in genetic control. To choose just one complication, a single DNA coding sequence can (via alternative splicing) produce multiple proteins, problematizing the gene concept itself (Downes 2004; see also Stotz, Griffiths, and Knight 2004) and, along with it, the notion of 'genetic control'. These reasons look to us to be reason enough to think that invoking genetic control offers no improvement over simple invariance accounts.

If the idea of genetic control cannot do the work Ariew needs, then perhaps it is the idea of the *strictness* of the invariance that he thinks can distinguish the truly canalized from the merely invariant. So perhaps another candidate solution for invariance approaches is to *extend the range* of environments under consideration. That is, perhaps we can extend our analysis of development beyond normal developmental environments and consider possible environments across the course of development in which traits would not occur.

Expanding the set A of relevant environments will typically result in a *shrinking* of the number of innate traits (or in a shrinking of the degree of innateness of most traits) because fewer traits will develop across the range of relevant environments (or will develop in a smaller number of them). So, for example, we might evaluate the innateness of the trait *believing that water is wet* by whether it develops in environments in which a person is never exposed to water but is fed entirely on, say, orange juice. If such an environment is considered relevant (and thereby included in

12. There *may* be a direct notion of "genetic control" if one takes genes to simply be segments of DNA (though see below) and restricts oneself to talk of, e.g., the synthesis of proteins. But clearly that notion will not be able to underwrite speaking of genes causing, say, such traits as *having a language acquisition device*.

13. Sober (2000), in a similar but slightly different vein, construes genetic causation in terms of norms of reaction.

https://doi.org/10.1086/515414 Published online by Cambridge University Press

A), then the belief will be relatively less innate than if we evaluate its development only in actual environments. (Presumably, the person would not develop the trait believing that water is wet because that person would fail to develop the concept *water*.) Call a theory that expands the relevant range of environments from relatively normal environments to possible-even-if-wildly-unlikely ones 'wide invariantism'.

Unfortunately, wide invariantism faces severe difficulties of its own. For starters, remember that, on an invariantist account, innateness is a matter of degree. So adding in the orange juice environment might succeed in rendering the trait believing that water is wet not 100% innate—but it still turns out to be very, very, very innate. Yet we expect that partisans on both sides would take such traits to be very noninnate on the grounds that, ex hypothesi, they arise from a learning process. Indeed, we would have thought that it is as noninnate as such similarly acquired but clearly variant traits as *believing that the number five bus goes to Parley's Way* or *believing that it is a good idea to keep six cats in a studio apartment in the East Village*. If this is right, merely expanding the range of relevant environments does not succeed in solving the problem posed by traits like believing that water is wet.

Wide invariantism faces a baby-and-bathwater problem too: even if a wider range of relevant environments could get rid of paradigm-acquired traits like believing that water is wet, it might also thereby get rid of paradigm-*innate* traits like *having 10 fingers and 10 toes*. This latter trait, while paradigmatically innate in humans, does not develop in circumstances that include thalidomide in the prenatal environment. And we do not see any principled way for wide invariantists to specify a range of environments relevant to assessing innateness that would allow them to include, for example, the orange juice environment but not a prenatal-thalidomide environment. The former, after all, is purely fictional, as is any no-water environment consistent with the organism's developing at all; the latter is an environment that unfortunately has been actually instantiated.

There may be other possible fixes for invariantism, but both genetic control and wide invariantism are not up to the job. Following Ariew's own diagnosis, we can see that neither proposed fix successfully makes use of the *etiology* of traits in order to help separate the innate from the invariantly acquired. The diagnosis thus motivates turning our attention to a recent and very etiology-oriented account of innateness: primitivism.

2. Primitivism. As we saw above, Samuels is sharply critical of invariance accounts. In their stead, he offers a radically different account of innateness. The core of Samuels's alternative view converges with a strand of Cowie's (1998) exegesis of the history of the innateness concept: the view

https://doi.org/10.1086/515414 Published online by Cambridge University Press

that a psychological structure is innate just in case it is a *psychological primitive*. To a first approximation, this means that innate traits are those for which there is no psychological account of their development, and acquired traits are those whose acquisition is explicable in psychological terms. Cowie and Samuels both think that their view captures an important concept of innateness used in contemporary cognitive science by Jerry Fodor, among others. And to the extent that Cowie and Samuels are correct in their exegetical claims that innateness understood as primitivism is an important, operative concept at work in contemporary (and perhaps historical) cognitive science, then Samuels's defense of primitivism is also a defense of this tradition. To the extent their exegetical claims are correct, our critique of primitivism is an attack on a certain way of using and thinking about innateness in contemporary cognitive science. But we also think that those claims are false—our account of innateness gives a better understanding of how that term functions in the literature. We begin by setting out Samuels's defense of primitivism and highlighting several features. We then go on to critique Samuels's account.

2.1. Defining Primitivism. So, what is it for a psychological structure to be a psychological primitive? A psychological structure *S* is a psychological primitive, according to Samuels, if and only if two conditions are met: *S* is a structure posited by some correct scientific psychological theory and there is no correct scientific psychological theory that explains the development of *S* (2002, 246). On Samuels's view, therefore, being a psychological primitive is an *explanatory* notion: "to say that a cognitive structure S is primitive is to claim that, *from the perspective of scientific psychology*, S needs to be treated as one whose [development] has no explanation" (246). Samuels emphasizes the failure of explanation within scientific psychology because he wants to leave open the possibility that explanations of the relevant structure might be possible within other (lower-level) sciences, "e.g. neurobiology or molecular biology" (246). Primitivism allows us to understand disputes regarding nativism in the cognitive sciences as disputes over whether the development of particular psychological mechanisms, a set of such mechanisms, or the preponderance of such mechanisms can be explained within scientific psychology.

This account of innateness employs the notion of an explanation within scientific psychology to do the work of distinguishing between the innate and the acquired, a point to which we return shortly. For now, let us note that Samuels is clear that he does not mean for the category of innate to be coextensive with those structures lacking a developmental explanation within *current* scientific psychology. Rather, he means that a structure is innate for which "there is no such theory to be discovered" (2002, 246, note 20) or, to put it another way, that even a completed scientific psy-

https://doi.org/10.1086/515414 Published online by Cambridge University Press

chology—a psychological theory that has discovered all the psychological acquisition explanations there are to be discovered—will not explain the development of the structure.

2.2. Why Primitivism? Why might primitivism be an attractive account of innateness? Samuels suggests a variety of advantages to his approach, but we will emphasize two. First, it both explains and advances the venerable philosophical approach that closely associates the innate with the unlearned. It explains the intuitions governing this approach, since for *t* to be learned is, on a primitivist account, for us to have a learning—and thus psychological—explanation of *t*'s development. By contrast, it advances the approach by allowing that there may be psychological processes of acquisition beyond those identified by traditional empiricist learning models.

A second advantage is that, as noted, primitivism focuses on etiology in a way that may allow it to avoid the invariantist's problem of liberalism. Beliefs like *water is wet* acquired through learning processes will have explanations (namely, learning explanations) within a complete scientific psychology, so primitivism counts them as acquired, not innate.[14]

2.3. The Need for Normal Development. So far, primitivism clearly looks radically different from invariantism. But this radical difference is soon moderated as Samuels supplements his account of innateness in order to deal with a certain class of problematic cases. These cases all involve the acquisition of a psychological structure via a patently nonpsychological process (i.e., one without a psychological explanation). For example, Samuels supposes that one could acquire knowledge of Latin by taking a 'Latin pill'.[15] "By hypothesis . . . the causal cascade initiated by ingesting the pill does not have a psychological description, but works by reorganizing the underlying neurochemistry of the brain" (Samuels 2002, 257). The problem posed by such a case (and other more realistic examples [257ff.]) is clear: the primitivism we have been considering clas-

14. For similar reasons, psychologists like Piaget who endorse an account of the psychological acquisition of a psychological structure thereby count as nonnativist on the primitivist account (see note 7). But note that primitivism's victory here is not complete. For while it handles the most troubling cases of liberalism for the cognitive sciences, it does not even try to handle nonpsychological cases like Ariew's example of *C. diff.* This bacterium, as noted earlier, is widely acquired by normal consumption of food and water. Possession of *C. diff.* is a paradigm acquired trait, but it does not have a *psychological* acquisition explanation. Samuels's account, for better or for worse, abandons the prospect of a unified account of innateness across biology and the cognitive sciences.

15. The example is from Fodor (1975, 34).

https://doi.org/10.1086/515414 Published online by Cambridge University Press

sifies such knowledge of Latin as innate, and such a result is problematic since it is common ground that in such a case the knowledge of Latin is acquired. Primitivism therefore faces a liberalism problem of its own.

One might think, given that invariantism's problems arose from insufficient attention to etiology, that a primitivist like Samuels would want to fix this problem by tweaking his own appeal to etiology, for example, by broadening the set of processes such that, if they produce a trait, the trait cannot be considered innate. But instead, surprisingly, Samuels tacks in the direction of invariantism! He attempts to handle such cases by adding a separate necessary condition on innateness that requires that an innate structure appear in the *normal course of development*. He suggests a *Normalcy Condition*:

> A (token) cognitive structure possessed by an organism O is innate only if O would acquire S . . . in the normal course of events. (2002, 259)[16]

Such a condition excludes cases like the Latin Pill, for such knowledge acquisition is not part of the normal course of events. Samuels does not offer us much guidance here about how to understand the notion of normal development, apparently because he realizes the issue is complicated and does not think the details matter much. He later suggests that the idea is related to "species typical patterns of development" (262) but says little else.

Like Samuels, we do not much want to wrangle over the details of normal development. However, without any further explanation, the condition looks ad hoc, designed only to address counterexamples to primitivism. Samuels offers little independent motivation for why such a condition is part of innateness or how it coheres with primitivism. We suspect that Samuels is relying on the intuitive plausibility of relating normal development and innateness. But notice that this same plausibility drives invariance accounts of innateness as well. Invariance accounts hold that a trait is innate only if it arises in a (perhaps pragmatically identified) broad range of environments. Invariance accounts do not have trouble with Latin Pills precisely because such pills do not figure in the production of invariant psychological structures across a broad range of environments. Very few environments have such pills in them! Requiring a 'normal course of development' condition addresses such problems in precisely the same way, by simply specifying the range of environments in which development counts in determining the degree of innateness.

To summarize, Samuels's account, which is offered as an alternative to

16. It seems clear that Samuels does not want to put much weight on the details of this condition, writing that it is an "instance" of the sort of condition one might add.

https://doi.org/10.1086/515414 Published online by Cambridge University Press

invariance accounts, is actually something of a hybrid, holding that a token psychological structure *S* is innate if and only if

(i) S is a structure posited by some correct scientific psychological theory, and there is no correct scientific psychological theory that explains the acquisition of S (*explanatory primitivism condition*); and
(ii) S would arise in the course of normal development (*normalcy condivation*).

In Section 3, we argue that despite its considerable charms, primitivism is not the right account of innateness for the cognitive sciences.

3. Critiquing Primitivism. Primitivism suffers from at least two critical flaws. First, primitivism needlessly confuses the epistemology of psychological theorizing with the metaphysics of mechanisms of acquisition. Second, the distinction between psychological and nonpsychological acquisition explanations is obscure, only recreating the problems with the innate/learned distinction we left behind.

The primitivism condition is a peculiar condition to impose on innate structures, precisely because, as Samuels notes, it is an *explanatory* constraint. It is an epistemological rather than a metaphysical condition on a psychological structure. We are as optimistic as Samuels that correct theories are a wonderful guide to how the world is (what better guide could there be?), but it is odd to *define* putatively objective aspects of the world in terms of explanation. Paradigmatic scientific properties such as *charge*, *spin*, *mass*, *cell*, *respiration*, and so forth are not understood in terms of explanations, and it strikes us as implausible that the concept *innate* should be so understood. We would have thought that innateness was a property of structures, traits, processes, or mechanisms that could be characterized independently of the varieties and levels of explanation that theorists do or could apply to them.

It is possible to recast Samuels's proposal in objective terms. We can do this by applying the predicates "psychological" and "nonpsychological" to the *processes of development themselves* rather than of our *explanations* of those processes. Primitivism thus recast would hold that innate psychological structures are structures that are psychological (where this means figuring in lawlike psychological regularities), but which are not *developed* by a psychological process. Thus, we can distinguish between primitivism$_e$ (Samuels's original proposal) and primitivism$_m$ (our amendment to that proposal):

A token psychological structure *S* is innate if and only if

https://doi.org/10.1086/515414 Published online by Cambridge University Press

(i) *S* is not acquired by a psychological process (*metaphysical primitivism condition*); and
(ii) *S* arises in the course of normal development (*normalcy condition*).

Primitivism$_m$ abandons one of the distinctive features of Samuels's formulation, but if we are right, it is better off abandoned.

This way of presenting primitivism also makes very clear that it (either in its original or metaphysicalized form) rests squarely on the shoulders of a distinction between psychological and nonpsychological explanations, processes, or mechanisms. But dialectically, it looks as though this reliance simply replaces the obscure learned/innate distinction with the equally obscure psychological/nonpsychological distinction. We have been happy to follow Samuels (and, e.g., Fodor 1975, 1981) in identifying the category of *learning* with *specific sorts* of psychological acquisition processes and in recognizing a superordinate category of *psychological acquisition* to pick out a larger group of relevant acquisition processes. But this will amount to substantial progress in understanding innateness only if we can give the psychological/nonpsychological distinction some determinate content. We cannot simply allow the terms to figure as bare unanalyzed placeholders, for that is precisely the circumstance, with respect to the innate/learned distinction, that we are hoping to leave behind.

How could we give the distinction between the psychological and the nonpsychological more content? What is it that a finished psychological science will be about? Put semantically, what is the meaning of "psychological"? There are some well-established pathways through the forest here, but we are not sure that they end up where we want to be. For example, one tradition in the philosophy of mind identifies psychological entities with the referents of folk psychological terms, where we understand these referents as being determined by the common beliefs comprising a *folk psychological theory* (e.g., Braddon-Mitchell and Jackson 1996). In line with this tradition, we could simply identify finished scientific psychological theories as being complete theories regarding the entities of folk psychology.

Unfortunately, there are serious problems with such a reliance on folk psychology. First, many people, and perhaps most famously Daniel Dennett (1987), argue that folk psychology is a predictive and explanatory instrument without commitments to the specific metaphysical facts of the brain in virtue of which it is successful. According to this instrumentalist view, the apparent objects of folk psychology—beliefs, desires, intentions, and so forth—are "*abstracta* rather than part of the 'furniture of the physical world'" (Dennett 1987, 72). If this view is right, then it is not

https://doi.org/10.1086/515414 Published online by Cambridge University Press

clear that folk psychology identifies entities that can then form the subject matter of a finished scientific psychology.

Second, a problem with this proposal is that, as Paul Churchland (1981) has argued, the boundaries of psychology as established by folk psychology also look to exclude a great many phenomena that are both interesting and within the purview of the scientific study of the mind/brain as it is currently understood. And one need not share Churchland's skepticism about the value of folk psychology to agree that there are a great many phenomena of interest to the sciences of the mind that do not figure in our folk psychology of beliefs, desires, and the like.

Instead of relying on folk psychology to identify the entities that interest us, we could shift from folk psychology to scientific psychology and use scientific theories to identify the entities about which a completed science of psychology is needed. Unfortunately, this simply pushes the bump in the carpet. Whereas before we hoped to look at folk psychology to identify the subject matter of scientific psychology, we now need to identify those scientific theories or researchers that are genuinely psychological. And how are we to do that? It might be thought that we can use contemporary disciplinary boundaries as institutionalized in academic and professional psychology to draw the relevant distinctions, but we are skeptical. We see no reason to think that those researchers and theories identified as psychological by contemporary institutional boundaries of scholarship and industry share a common subject matter. Rather, it is obvious that disciplinary and institutional boundaries are evolving and intellectually porous and cannot ground the kind of distinction that we need. The openness of the category of the psychological is revealed by the rapid increase in the number of centers for cognitive science, representing a confederacy that includes not just psychology but also such fields as linguistics, philosophy, neurology, computer science, and anthropology. The recently developing field of cognitive neuroscience lies right on the vague border between the psychological and the biological. Moreover, disciplinary boundaries can expand to chase their explananda, so what can appear to be a chapter of purely biological neurology one day can become part and parcel of psychology the next, should the structures involved prove of psychological—and, in particular for our purposes here, developmental psychological—interest. The psychological/nonpsychological distinction is simply too weak a branch upon which to rest the concept of innateness.[17]

17. It is worth noting that to the extent Samuels simply stipulates borders for psychology, to that extent his proposal will fail to have the kind of programmatic neutrality he claims for it. Samuels writes that "disputes between behaviorists and cognitivists and between connectionists and classical computational theorists have, in large part, concerned" the "correct framework within which to formulate scientific psychological

https://doi.org/10.1086/515414 Published online by Cambridge University Press

A more promising option for Samuels is to identify the psychological with a certain sort of natural kind or set of kinds. All Samuels needs to ground the distinction is that the boundary between the psychological and nonpsychological be objective, not that we know currently what the boundary is. Samuels could take the referent of the psychological to be the natural kind, *whatever it is*, that underlies key phenomena associated with the use of the term 'psychological'. As contemporary semantic externalists have argued, perhaps we can carve nature at its joints without knowing exactly what those joints are.

But is it at all plausible to think that such a kind underlies psychology? Call membership in such a kind a *natural criterion* for psychology. What might such a criterion look like? The only existing proposal that seems remotely plausible to us for such a kind is the possibility, suggested in the works of classical cognitive scientists (e.g., Fodor 1975, 1981; Newell and Simon 1976; Haugeland 1981; Pylyshyn 1986), that psychological phenomena may be explained by manipulations of internal symbols occurring at a distinct level of explanation. For example, Newell and Simon famously defend the "Physical Symbol System Hypothesis," holding that "a physical symbol system has the necessary and sufficient means for general intelligent action" (1976, 116). We are very sympathetic to this classical approach to the study of cognition, as one significant piece of the overall psychological story; however, we do not think that it can underwrite a distinction between psychological and nonpsychological phenomena of the sort primitivism needs. Moreover, we think its inability even to begin to define the cognitive suggests that no natural criterion is available to do the work Samuels requires.

The *cogency* of the classical approach to cognition has been sharply attacked on numerous grounds, but we will not address those here, focusing instead on attacks on the *completeness* of the classical approach. Such attacks suggest that even if some cognitive processes are underwritten by a symbolic architecture, it is wildly implausible to assume that they all are. Rather, a variety of explanatory paradigms, including connectionism (e.g., McClelland and Rumelhart 1986; Rumelhart and McClelland 1986; Sejnowski and Rosenberg 1986, 1987), dynamic systems (van Gelder 1995), and situated/embedded cognition (Hutchins 1995; Clark 2003), offer explanations of some seemingly intelligent behaviors

theories" (2002, 252). But, Samuels argues, "it would be a mistake to incorporate a substantive account of scientific psychology into the primitivist account of nativism" (252) because this would make determination of the right programmatic definition of innateness hostage to far more sweeping research programs instead of allowing it to play a role in fixing and contrasting the empirical claims of such programs. Insofar as primitivism remains silent on the specific character of psychology, it offers this kind of programmatic neutrality.

https://doi.org/10.1086/515414 Published online by Cambridge University Press

that do not appear to require an internalized symbolic language of thought for their production. The moral of these research programs, and the others that will no doubt follow, is that there are numerous, heterogeneous ways to solve cognitive/behavioral problems, of which a symbolic architecture is only one. If the mind/brain ultimately employs heterogeneous mechanisms, then classical approaches to cognition will fail for the same reason Churchland attacked folk psychology: there would be important mental phenomena that fall outside the scope of the theory.

The possible incompleteness of the symbolic story suggests a more general failure as well. *Any* heterogeneous approach to explanatory mechanisms (and such an approach is accepted even by many sympathetic to symbolic representations [e.g., Sterelny 1990, Chapter 8]) undermines our chances of finding the needed natural criterion. So, the strategy of attempting to find a natural criterion to ground the needed psychological/nonpsychological distinction turns out to rely on a substantial (and we think implausible) empirical assumption: that there is such a criterion to be had.

We thus see that primitivism, despite its initial attractions, is forced to rest too much theoretical weight on the distinction between the psychological and the nonpsychological. One might be able to draw such a distinction in institutional terms, though we doubt it. But it is clear that one will not be able to draw the distinction firmly where really one would have to for primitivism to succeed—out in the world.

4. Beyond Invariantism and Primitivism. Thus far, we have argued against the two main extant accounts of innateness. Invariantist accounts are too liberal, and this results from their indifference to the actual processes or mechanisms that give rise to the traits in question. Ariew's (1999) defense of canalization recognizes this but fails to formulate an account of innateness that rectifies the problem. And Samuels's primitivism is wrongheaded, relying as it does on an epistemological rather than metaphysical conception of innateness and requiring an unsustainably robust psychological/nonpsychological distinction. Despite their apparent (and real) differences, Ariew's invariantism and Samuels's primitivism share a deeper set of similarities. Both theories are at heart 'invariance-plus' accounts that try to fix simple invariantism's liberalism problem by appealing to *something* about the etiology of traits: canalization for Ariew and psychological primitiveness for Samuels. But in both cases the additional condition—the 'plus' factor—failed the task set for it. Nonetheless, we think that the attention Ariew and Samuels pay to etiology is entirely warranted, and we follow them in pursuing the invariance-plus strategy. Our claim is that for a trait to be innate, it must normally be invariantly acquired and *must not have been acquired by means of a process that*

https://doi.org/10.1086/515414 Published online by Cambridge University Press

normally produces variant traits. What distinguishes the cases that cause trouble for the invariance account is that they involve *open developmental processes* in systematically invariant environments. So we want to exclude these cases from the innateness category.

First, let us give a more precise account of the open/closed distinction. Any developmental process has some range of traits in which it can eventuate, and the openness or closedness of the process corresponds to how diverse that range is. It is not just that the process eventuates in a large number of traits, since presumably the process that gives us 10 fingers and 10 toes also gives us lots of other more particular traits about the particular arrangement and structure of each of those digits, inter probably many alia. It is important also that the process is producing different traits in accord with which environments in which the organism is developing.

For example, if we assume that Noam Chomsky is basically correct, then the human language acquisition device (LAD) can produce any of an extremely diverse set of linguistic competences, depending on what linguistic community in which the organism finds itself. The LAD thus is a fairly open process. But the process, for any given person, by which they develop their basic bodily map is much more highly constrained. These developmental processes are difficult to disrupt, but disrupting these processes has a much narrower range of outcomes. If one goes to an environment that is sufficiently extreme, one may find that it does not support the development of two arms, two legs, two kidneys, and so forth, in their usual locations. The reason is almost always because it will not support the development of arms or legs or kidneys *at all*—and not unusual cardinalities of organs, or organs in problematic locations. If the environment supports the functioning of such processes at all, the process is extremely fixed in its outcomes. We thus say that these are very closed processes. Many processes may lie in between such extremes; the process by which we have a certain degree of muscle in our arms, for example, can give significantly varying outcomes depending on such environmental factors as nutrition and exercise, but only within a fairly specific range.

Our account thus holds that a trait *t* is innate in an organism *O* to the extent that

(i) *O* would develop *t* across the range of normal environments (*invariance condition*);[18] and

18. As Samuels (2002) notes, it is not clear how to interpret *normal*, and different theorists may wish to interpret this condition in different ways. While we view the exact specification of the condition as indeterminate, we know of no cases in which a theorist has insisted that a trait is innate that would not develop across a broad range of actual environments.

https://doi.org/10.1086/515414 Published online by Cambridge University Press

(ii) The proximal cause of *O*'s development of *t* is by a closed process or processes (*closed process condition*).

To illustrate the account, consider how it applies to sex determination. In humans, the development of phenotypic sexual features is typically determined by the chromosomes each of us receives. It then typically develops invariantly via closed processes. It therefore counts as innate. But in other species, notably many reptiles, phenotypic sexual characteristics are determined in part by the temperature at which the eggs are incubated (see, e.g., Ayling and Griffin 2002). In these species, the process that gives rise to phenotypic sex may well be closed given that it results in relatively few phenotypic outcomes across a range of environments, but phenotypic sex would not count as innate because the sex of each organism does not develop invariantly across a wide range of environments—in those environments in which the temperature is different, the sex is different.

5. Closed Process Invariance and Liberalism. Where Ariew has invoked a distinction between canalized and noncanalized development, and Samuels a distinction between psychological and nonpsychological explanations, we rely on a distinction between open and closed processes. We thus need to take some time here to argue both that the resulting account is immune to the charge of liberalism (and thus succeeds in improving over simple invariantism) and that our preferred distinction has no undischarged or intractable theoretical commitments (and thus succeeds in improving over both Ariew and Samuels).

We begin with the charge of liberalism. Importantly for our account, traits like believing that water is wet are (by hypothesis) produced by an extremely open process, namely, general learning.[19] Learning can produce an indefinitely large set of traits, for example, all of the various beliefs that any person might develop given some set of inputs from the environment. Thus learning is a decidedly open process, and any trait it produces will not be counted as innate by our account.[20] What about the

19. It is unproblematic for us if 'general learning' turns out to name more a family of processes than any one process; see below.

20. It is worth noting that our account provides exactly the sort of distinction needed to handle certain examples from classical philosophical debates over innateness. Empiricists like John Stuart Mill argue that the appearance of necessity could arise as a result of experience. For example, he writes "that the same proposition cannot at the same time be false and true. . . . I consider it to be, like other axioms, one of our first and most familiar generalizations from experience" (*Logic*, bk. II. c. vii. § 4). The idea, as in the water is wet case, is that a belief could be acquired invariantly from experience, but the empiricist explains the belief by reference to an open process of acquisition

https://doi.org/10.1086/515414 Published online by Cambridge University Press

case of *c. diff*? It seems that the process by which the trait of being inhabited by *c. diff.* is simply the process of ingestion, which can produce a large variety of traits. Just consider all the different things—from the benign to the pathogenic—that one might ingest! Distinguishing between open and closed processes allows us to fend off liberalism, but what of the notion of a process itself? Our account is committed to the existence of real, distinct processes operating in development, some of which can be identified as the proximal causes of a given trait. Without such a commitment, the notion of a process could undermine our account from two directions: by being either overly narrow or overly broad.

Consider first the possibility of overly narrow construals of processes. Suppose that someone simply describes a process as follows: "the process that produced *t*, but no other traits." For example, suppose that someone describes the process that happened to produce believing that water is wet and nothing else in an organism. If such a description is not empty, then the process so picked out would be very closed. And the trait would thus, on our account, be innate. Indeed, since every trait would enable such a description of a process, it would follow on our account that all traits are innate! But we deny that the description is satisfied. We deny that there is a process that produces the trait of believing that water is wet, but no other trait. Any substantive ontology of processes may acknowledge the existence of learning in general,[21] and important subtypes of learning, typed according to underlying sense modalities; or different mechanisms of conscious inference, tacit learning, and conditioning; or different storage architectures such as long-term memory, the lexicon, and so on. But none of these will turn out to be sufficiently belief specific as to count as a closed process whose sole output is believing that water is wet. And if it did turn out on close inspection that there was such a process that was dedicated to the production of that trait, then we would contend that the initial judgment that the trait is not innate should be revised.

One might worry that, in addition to ruling out overly specific 'processes' that would make all traits the product of closed processes, we also must rule out the overly general construals of 'process' that would make all traits the product of open processes. Suppose that one could take all

(experience of the world) while the rationalist holds that such beliefs arise in some way from the mind itself. It is clear that this debate can be construed as depending on the openness of the process by which the trait is acquired.

21. Though we do not mean to wed our account to any particular story about processes, we note that the recent and popular Machamer, Darden, and Craver theory of mechanism is sufficiently robust to rule out these problem cases, since that theory includes an emphasis on the importance of regularity, nonaccidentality, and counterfactual support in the positing of activities (e.g., Machamer, Darden, and Craver 2000, 7).

https://doi.org/10.1086/515414 Published online by Cambridge University Press

the processes that have been involved in Chomsky's development and call that whole assemblage "Chomsky's developmental process." This will turn out to be an open process, since it would have given Chomsky different traits had he grown up with a different environment. And it might seem to follow, then, that none of Chomsky's traits are innate, since there is some open process that has produced all of his traits! Call this the *overly general process* problem.

This worry is closely related to a worry commonly raised by opponents of the innateness concept. Opponents of the innateness concept, especially those influenced by Lehrman's (1953) influential critique, are wont to emphasize that development is an enormously complex cascade of processes, many of which may be open to environmental perturbations. Consider one of the examples of an open process with which we have been working: ingestion. While we have suggested that ingestion is an open process that may result in wide range of traits, it is important to note that ingestion is also a causal prerequisite for the operation of virtually any paradigmatically innate process in humans. Similarly, the processes by which DNA causes the synthesis of particular proteins that in turn take part in other complex interactions giving rise to, for example, tissues and organs may have an enormous number of outcomes under environmental perturbation, and so these processes would be open. What these examples seem to show is that if *any* open process in the developmental history of a trait makes that trait noninnate, almost no traits will be innate. Call this the *openness in the causal history* problem.[22]

While some might think that the solution here is to give up on the innateness concept, we think that the answer to both the overly general process problem and the openness in the causal history problem is to be found in insisting again on a robust ontology of processes. In response to overly general construals of processes, we need only insist that our condition ii be interpreted such that for t to be innate in O, it does not matter if *some* open process has produced t in O, as long as there exists *some* proximal cause of t that is a closed process. And similarly, openness in the causal chain leading to the development of t does not count against t's being innate as long as the proximal cause of t is closed.

So, it seems that we are hanging our account on there being an objective way of individuating processes. As a general rule, for a process to count as a "real" process, there must be some criteria of individuation independent of the trait or traits the process results in. For example, processes might be individuated by their evolutionary function or by the distinct

22. Indeed, DNA synthesis also raises the overly general process problem as it produces a rather large number of traits, many of which, *like having ATP synthase in its cells*, would be categorized as very innate in most organisms.

https://doi.org/10.1086/515414 Published online by Cambridge University Press

physiological mechanism that underwrites them. In any case, we take it that scientific practice justifies our rejection of overly narrow and overly broad processes—they just are not the sort of things in which actual developmental biologists or psychologists have much interest. Moreover, at least Samuels relies on a robust distinction between processes to ground his account of innateness, so we are in good company in making this assumption. At a minimum, we are willing to grant that if, as a matter of fact and at the end of the day, the notion of a real process fails to be scientifically respectable, then so too will the notion of innateness prove disreputable as well. But, in the meantime, in which the sciences are up to their Krebs cycles in processes, we take our 'closed process invariantism' account to provide a framework for discussing innateness that resolves the problem of liberalism.

BIBLIOGRAPHY

Ariew, A. (1996), "Innateness and Canalization," *Philosophy of Science* 63, suppl.: S19–S27.
——— (1999), "Innateness Is Canalization: In Defense of a Developmental Account of Innateness," in Valerie Hardcastle (ed.), *Where Biology Meets Psychology: Philosophical Essays*. Cambridge, MA: MIT Press, 117–139.
Ayling, L.-J., and D. K. Griffin (2002), "The Evolution of Sex Chromosomes," *Cytogenetic Genome Research* 99: 125–140.
Block, Ned (1981), "Introduction: What Is Innateness?" in Ned Block (ed.), *Readings in Philosophy of Psychology*. Vol. 2. Cambridge, MA: Harvard University Press, 279–281.
Boyd, Richard (1999), "Homeostasis, Species, and Higher Taxa," in Robert A. Wilson (ed.), *Species: New Interdisciplinary Essays*. Cambridge, MA: MIT Press, 141–186.
Braddon-Mitchell, D., and F. Jackson (1996), *Philosophy of Mind and Cognition*. Cambridge, MA: Blackwell.
Churchland, Paul (1981), "Eliminative Materialism and the Propositional Attitudes," *Journal of Philosophy* 77 (2): 67–90.
Clark, Andy (2003), *Natural Born Cyborgs: Minds, Technologies, and the Future of Human Intelligence*. Oxford: Oxford University Press.
Cowie, Fiona (1999), *What's Within? Nativism Reconsidered*. New York: Oxford University Press.
Dennett, Daniel (1987), *The Intentional Stance*. Cambridge, MA: MIT Press.
Downes, Stephen (2004), "Alternative Splicing, the Gene Concept, and Evolution," *History and Philosophy of the Life Sciences* 26: 91–104.
Fodor, Jerry (1975), *The Language of Thought*. Cambridge, MA: Harvard University Press.
——— (1981), "The Present Status of the Innateness Controversy," in Jerry Fodor (ed.), *Representations: Philosophical Essays on the Foundations of Cognitive Science*. Cambridge, MA: MIT Press, 257–316.
——— (2001), "Doing Without What's Within: Fiona Cowie's Critique of Nativism," *Mind* 110 (437): 99–148.
Griffiths, Paul (1997), *What Emotions Really Are*. Chicago: University of Chicago Press.
Haugeland, J. (1981), "Semantic Engines: An Introduction to Mind Design," in J. Haugeland (ed.), *Mind Design*. Cambridge, MA: MIT Press, 1–34.
Hutchins, E. (1995), *Cognition in the Wild*. Cambridge, MA: MIT Press.
Johnston, T. D. (1988), "Developmental Explanation and the Ontogeny of Birdsong: Nature/Nurture Redux," *Behavioral and Brain Sciences* 11: 617–663.
Lehrman, Daniel S. (1953), "A Critique of Konrad Lorenz's Theory of Instinctive Behavior," *Quarterly Review of Biology* 28: 337–363. Reprinted in Susan Oyama, Paul Griffiths,

https://doi.org/10.1086/515414 Published online by Cambridge University Press

and Russell Gray (eds.), *Cycles of Contingency: Developmental Systems and Evolution*. Cambridge, MA: MIT Press, 2001.
Machamer, Peter, Lindley Darden, and Carl Craver (2000), "Thinking about Mechanisms," *Philosophy of Science* 67: 1–25.
McClelland, J., and D. Rumelhart (1986), "The Appeal of Parallel Distributed Processing," in D. Rumelhart, J. McClelland, and the PDP Research Group (eds.), *Parallel Distributed Processing: Explorations of the Microstructure of Cognition*. Vol. 2. Cambridge, MA: MIT Press, 3–44.
Newell, A., and H. Simon (1976), "Computer Science as Empirical Inquiry: Symbols and Search," *Communications of the Association for Computing Machinery* 19 (3): 113–126.
Pylyshyn, Zenon (1986), *Computation and Cognition*. Cambridge, MA: MIT Press.
Rumelhart, D., and J. McClelland, "On Learning the Past Tenses of English Verbs," in D. Rumelhart, J. McClelland, and the PDP Research Group (eds.), *Parallel Distributed Processing: Explorations of the Microstructure of Cognition*. Vol. 2. Cambridge, MA: MIT Press, 216–271.
Samuels, Richard (2002), "Innateness in Cognitive Science," *Mind and Language* 17 (3): 233–265.
Sejnowski, T., and C. Rosenberg (1986), "NETtalk: A Parallel Network That Learns to Read Aloud," technical report JHU/EEC-86/01. Baltimore: Johns Hopkins University.
——— (1987), "Parallel Networks That Learn to Pronouce English Text," *Complex Systems* 1: 145–148.
Sober, E. (1998), "Innate Knowledge," in E. Craig (ed.), *The Routledge Encyclopedia of Philosophy*. Vol. 4. London: Routledge, 794–797.
——— (2000), "The Meaning of Genetic Causation," in A. Buchanan et al. (eds.), *From Chance to Choice*. Cambridge: Cambridge University Press.
Sterelny, Kim (1990), *The Representational Theory of Mind*. Cambridge, MA: Blackwell.
Stich, S. (1975), "Introduction: The Idea of Innateness," in *Innate Ideas*. Berkeley: University of California Press, 1–22.
Stotz, K., P. E. Griffiths, and R. D. Knight (2004), "How Scientists Conceptualize Genes: An Empirical Study," *Studies in History and Philosophy of Biological and Biomedical Sciences* 35 (4): 647–673.
Van Gelder, T. (1995), "What Might Cognition Be, If Not Computation?" *Journal of Philosophy* 92 (7): 345–381.
Wendler, D. (1996), "Locke's Acceptance of Innate Concepts," *Australasian Journal of Philosophy* 74 (3): 467–483.
Wimsatt, William C. (1986), "Developmental Constraints, Generative Entrenchment, and the Innate-Acquired Distinction," in W. Bechtel (ed.), *Integrating Scientific Disciplines: Case Studies from the Life Sciences*. Dordrecht: Martinus-Nijhoff, 185–208.
——— (1999), "Generativity, Entrenchment, Evolution, and Innateness: Philosophy, Evolutionary Biology, and Conceptual Foundations of Science," in Valerie Hardcastle (ed.), *Where Biology Meets Psychology: Philosophical Essays*. Cambridge, MA: MIT Press, 139–180.

https://doi.org/10.1086/515414 Published online by Cambridge University Press