Received: 9 June 2016 | Revised: 11 November 2017 | Accepted: 15 December 2017

DOI: 10.1111/phc3.12492

ARTICLE

WILEY

The conceptual critique of innateness

Stefan Linquist

University of Guelph

Correspondence
Stefan Linquist, Department of Philosophy, University of Guelph, 50 Stone Road East, Guelph, Ontario N1G 2W1, Canada.
Email: linquist@uoguelph.ca

Funding information
Social Sciences and Humanities Research Council of Canada; Social Sciences and Humanities Research Council of Canada

Abstract

It is widely recognized that the innate versus acquired distinction is a false dichotomy. Yet many scientists continue to describe certain traits as "innate" and take this to imply that those traits are not acquired, or "unlearned." This article asks what cognitive role, if any, the concept of innateness should play in the psychological and behavioural sciences. I consider three arguments for eliminating innateness from scientific discourse. First, the classification of a trait as innate is thought to discourage empirical research into its developmental origin. Second, this concept lumps together a number of different biological properties that ought to be treated as distinct. Third, innateness is associated with the outmoded folk biological theory of essentialism. In response to these objections, I consider two attempts to revise the concept of innateness which aim to make it more suitable for scientific explanation and research. One proposal is that innateness can be defined in terms of the biological property of environmental canalization. On this view, a trait is innate to the extent that it is developmentally buffered against a range of different environments. Another proposal is that innateness serves as an explanatory primitive for cognitive science. This view holds that there exist a sharp boundary between psychological and biological explanations and that to identify a trait as innate means that it falls into the latter explanatory domain. This essay ends with some questions for future research.

1 | INTRODUCTION

It is widely recognized that the innate versus acquired distinction is a false dichotomy. Our most basic understanding of developmental biology tells us that every trait results from an *interaction* between genetic and environmental factors. Yet despite this "interactionist consensus" as it is called (Kitcher, 2001), talk of innateness remains commonplace in both academic and colloquial settings. Even sophisticated researchers seem comfortable asking whether complex psychological traits—such as moral judgment (Prinz, 2008), primitive concepts (Carey, 2009), or aesthetic values (Dutton, 2009)—are innate. What is going on here? How can so many researchers acknowledge, on the one hand, that innateness-talk is out of synch with basic biology, while simultaneously pursuing questions about innateness as if they were perfectly sensible?

© 2018 The Author(s) Philosophy Compass © 2018 John Wiley & Sons Ltd

This article reviews some recent philosophical arguments surrounding this question. On one side of the debate are “eliminativist” philosophers and scientists who recommend abolishing the category of innateness from scientific discourse. They argue that classifying a trait as innate encourages researchers to neglect environmental influences on its development (Lehrman, 1953). This concept also lumps together a number of biological properties that ought to be treated as distinct (Bateson, 1991; Griffiths, 2002; Mameli & Bateson, 2006, 2011). Critics of innateness further present evidence that the concept is associated with a discredited folk biological theory of essentialism (Griffiths, Machery, & Linquist, 2009; Linquist, Machery, Griffiths, & Stotz, 2011). Since scientists have at their disposal more precise terms for describing developmental and evolutionary processes, innateness is seen as a defunct category that ought to be abandoned by the behavioural and cognitive sciences.

On the other side of this debate are “revisionist” philosophers who admit that our commonsense concept of innateness is problematic, but maintain that—with some tweaking—it can be rendered scientifically useful. Revisionists look to particular scientific disciplines for suggestions about how to redefine innateness. Although a number of revisionist proposals have emerged in recent years (Birch, 2009; Collins, 2005; Khalidi, 2007; Maclaurin, 2002; Mallon & Weinberg, 2006; O'Neill, 2015; Sober, 1998; Stich, 1975; Wendler, 1996; Wimsatt, 1999), I will focus this essay on two particularly influential suggestions. The first is André Ariew's (1996, 1999, 2007) proposal to define innateness in terms of the biological property of environmental canalization. The basic idea is that a trait can be defined as innate to the extent that it is developmentally buffered against some normal range of environmental changes. The second is Richard Samuels' (1998, 2002, 2007) proposal that the innate versus learned distinction marks the boundary between biological and psychological modes of explanation. On this view, innateness functions as an explanatory primitive for cognitive science. This essay concludes with some questions for future research.

Before proceeding, it is important to clarify what this essay is not about. My focus is on whether our folk concept of innateness, or some revised version of it, should feature in the scientific study of cognition and behaviour. This conceptual critique of innateness should not be confused with the epistemic question of how to empirically demonstrate whether some trait is innate. Much has been written, for instance, about “poverty of the stimulus” arguments and whether they provide adequate support for innateness (e.g., Collins, 2003; Cowie, 1999; Fodor, 2001; Schoneberger, 2005). The topic of this essay is in a sense more basic. If innateness turns out to be a scientifically bogus category, then we shouldn't waste time debating possible methods for detecting it.

2 | WHY ELIMINATE “INNATE”?

At first, the question of whether scientists should jettison innateness from their stock of theoretical terms might seem entirely empirical. Shouldn't it be up to *scientists* to determine what the term means and whether it is useful? Why would it take a contribution from philosophy to decide the issue?

Part of the answer is that innateness is not exclusively a scientific term. The dichotomy between innate versus acquired traits is rooted in a prescientific worldview dating at least to Plato. It is partly a philosophical challenge to explicate this vernacular or “folk” concept of innateness and to determine whether some revised version has scientific utility (Machery, 2017).

Another part of the answer is that scientists themselves disagree about whether innateness is a useful construct. Daniel Lehrman (1953) presented one of the earliest and most influential critiques of innateness in the field of ethology. He cites numerous examples of traits which, though initially labelled “innate,” turned out to be environmentally influenced. For example, Lehrman pointed to researchers who identified a trait as “innate” if it was present at birth. According to Lehrman, this label discouraged them from investigating whether the trait was sensitive to environmental factors. Indeed, many of those same traits turned out on closer inspection to undergo *prenatal conditioning*—i.e., learning that occurs in the womb or egg (Wells & Hepper, 2006, provide a contemporary example). Lehrman concluded that innateness functions as a pseudo-explanation for how traits develop, serving only to inhibit experimental research. As he put it, “The use of ‘explanatory’ categories such as ‘innate’ and ‘genetically fixed’ obscure the necessity of investigating developmental processes in order to gain insight into the actual mechanisms of behaviour and their interactions" (1953, p. 345).

Another influential critique of innateness can be found in the work of behavioural scientist Patrick Bateson (1985, 1991; Bateson & Martin, 1999). In 1991, he observed that "At least six meanings are attached to the term: present at birth; a behavioural difference caused by a genetic difference; adapted over the course of evolution; unchanging throughout development; shared by all members of a species; and not learned" (p. 21). His objection was not merely that the term is ambiguous. The problem is that it encourages researchers to unreflectively jump from one property to another without adequate evidence. For example, if a trait is common across some species (or "species typical"), it is often regarded as an adaptation. Similarly, if a trait is stable over development, it is often presumed to have a specific genetic basis. Such inferences are unjustified, Bateson argued, because the relevant properties often come apart. For example, a trait can be species typical without being an adaptation. This occurs, for example, when a trait is fixed by genetic drift, or when it is genetically linked to an adaptation, or because it is maintained by phylogenetic inertia, or because it is phenotypically plastic (Gould & Lewontin, 1979). Likewise, various non-genetic mechanisms contribute to developmental stability. For example, the phenomenon of imprinting in birds (Bateson, 1966) shows how biologically important information (i.e., which object to follow during first few weeks of life) is supplied by the environment. Such considerations led Bateson to recommend eliminating the concept of innateness because it seemed inextricably tied to such questionable inferences.

This objection has been developed in Bateson's more recent work with philosopher Matteo Mameli (2006, 2007, 2011). Expanding on Bateson's (1991) original list, they identify a total of *twenty six* candidate definitions of innateness (Mameli & Bateson, 2006). Many of these proposals are deemed unsatisfactory because "they appeal to unclear, unexplained, or controversial notions such as 'genetic information,' 'learning,' 'developmental induction,' and 'normal development'" (2006, p. 176). Other candidate definitions are rejected by Mamelli and Bateson because they fail to pick out any trait whatsoever. For instance, no trait is (strictly speaking) genetically determined or unacquired. Other candidates are rejected because they apply to all possible traits. For example, every trait is (to some degree) genetically influenced or insensitive to the environment. By the end of their analysis, Mameli and Bateson arrive at a shortlist of just eight candidates. These *i-properties*, as they call them, are relatively promising definitions in that they suffer from no obvious conceptual or empirical shortcoming. Hence, a trait can potentially be described as "innate" if it satisfies at least one of the following:

1. It appears reliably at a particular stage of the lifecycle.
2. It develops abnormally in response to environmental manipulations which were not encountered in the organism's evolutionary history.
3. It is not produced by a developmental mechanism that evolved to generate different phenotypes in response to particular environmental conditions (i.e., adaptive phenotypic plasticity), and it is also not the result of exposure to abnormal environmental conditions.
4. Any modification to the trait would have a negative impact on the development of other traits (i.e., generatively entrenched).
5. It is insensitive to certain environmental changes during development.
6. It is insensitive to certain environmental changes after development.
7. The trait is species typical.
8. The trait is a standard Darwinian adaptation.

The question, however, is whether one of these properties, or perhaps some combination, provides a useful scientific definition of innateness. An easy solution would be to just stipulate one or more of the i-properties as the referent of "innate." But this would raise a practical problem. This term is already a fixture of our common vernacular.

As we shall see momentarily, this vernacular concept of innateness recognizes several different i-properties as individually sufficient for innateness (Griffiths et al., 2009). To stipulate a narrow definition for innateness would place it in tension with the folk concept. This is a problem, not because the folk concept is somehow justified or correct. On the contrary, this concept is highly problematic and the danger is that researchers will continue to lapse into using it (Griffiths, 2002). Cognitively, it is more economical to abandon innateness in favour of more precise concepts.

Another possible strategy for defining innateness would be if certain i-properties naturally come packaged together. For example, suppose that traits exhibiting (1) developmental regularity also tend to be (7) species typical and (8) an adaptation. Then a case could be made for defining innateness as the cluster of these three co-occurring i-properties.

Mameli and Bateson claim that this possibility has not been addressed in the empirical literature. We simply do not know which (if any) of the i-properties tend to co-occur (but see Shea, 2012). At the same time, they present theoretical reasons for suspecting that the overlap among i-properties is only partial (see also Griffiths, 1997, 2002). Part of their argument points to the growing body of scientific literature. No longer do biologists view development as the simple execution of a genetic code that was programmed by natural selection. Molecular biology now recognizes various epigenetic mechanisms involved in cognitive and behavioural development (Day & Sweatt, 2011; McGowan & Roth, 2015). Some developmental biologists further encourage us to view the organism as the inhabitant of a “developmental niche” consisting of recurring environmental factors that control the expression of genes (Stotz, 2017; West & King, 1987). Others suggest that phenotypic plasticity in conjunction with a shared environment is sufficient to maintain species-typical traits (West-Eberhard, 2003). Social learning provides yet another mechanism that can cause traits to become species typical without a specific genetic basis (Mameli, 2004; Sterelny, 2003; Wilson & Kniffin, 1999). Various mechanisms such as these can explain how certain i-properties are maintained, often in the absence of other i-properties. Hence, according to the *clutter hypothesis*, as Mameli (2008) calls it, the presence of any one i-property does not reliably increase the likelihood that a trait will possess another.

At this stage, we might stop and ask why the concept of innateness retains its currency in certain scientific circles. After all, Bateson and others have been making the point for decades that this term encourages the conflation of potentially distinct i-properties. Why do some researchers cling to innateness in the face of mounting theoretical opposition and a lack of empirical support?

Paul Griffiths (2002) offers one of the more sophisticated answers to this question. He points to psychological evidence that most people operate with a fundamentally flawed conception of the biological world (Atran, Medin, & Ross, 2002). According to this “folk biological” picture, organisms can be classified into groupings because they share an inner nature or essence. This idea of an underlying essence is thought to explain why members of a species look similar to each other, develop in the same way, and have traits that serve particular “purposes” or functions. Of course, this essentialist picture was overturned with the rise of Darwinism (Sober, 1980). Nonetheless, it remains an influential fixture of popular thinking. As Atran et al. (2002) put it, “to understand modern biological science, people must *unlearn* universal dispositions to view species essentialistically” (p. 32).

Griffiths (2002) hypothesized that the vernacular concept of innateness is grounded in three particular features that are shared by the essentialist framework:

- Fixity: Innate traits are insensitive to environmental influences during development.
- Typicality: Innate traits are shared by most adult members of the same species or sex.
- Function: Innate traits serve some purpose or end for the organism.

In recent experimental work, Griffiths et al. (2009) used a questionnaire survey to test this Three Feature theory. They presented subjects with a series of vignettes describing examples of birdsong that differed in Fixity, Typicality, and Function. The aim was to determine the extent to which each factor *independently* contributes to folk judgments about innateness. As predicted, evidence of Fixity was enough for subjects to agree that a trait is innate, even if the trait lacks Typicality and Function. However, evidence of Typicality also independently drove innateness judgments, as did

Functionality (though to a lesser degree). The authors take these results to provide a fine grained analysis of the vernacular concept of innateness, revealing just how easily we are prompted to classify a given trait as “innate.” In a following study, Linquist et al. (2011) expanded the survey to test whether Fixity, Typicality, and Function also influence judgments about whether a trait is “in the DNA.” They found that the two phrases (“innate” and “in the DNA”) draw upon the same underlying folk concept. Linquist et al. (2011) also cite examples where researchers in psychology and biology—who presumably “know better”—nonetheless employ the folk concept in drawing unjustified inferences among i-properties. It would appear that folk essentialism is a stubborn theoretical construct that requires a concerted effort to avoid.

The magnitude of this worry has been explored using a slightly different questionnaire that was administered, this time, both to lay-people and to professional cognitive scientists (Knobe & Samuels, 2013). The authors report that scientific training had no effect on subjects' judgments about innateness—in response to some questions, both groups relied on the folk concept. As the authors explain,

> *whatever differences in knowledge there might be between a person who goes to popular science websites [i.e. a layperson] and a person who has a PhD in cognitive science, these differences do not have a substantial impact in responses to the questions posed here (Knobe & Samuels, 2013, p. 82).*

However, the authors detected an effect of context on innateness judgments:

> *When people are placed in a situation that encourages them to think in a principled way about considerations that are influencing their judgments, they show a systematic tendency to shift towards a pattern of judgment described by existing scholarship on the scientific concept of innateness (Knobe & Samuels, 2013, p. 82).*

The question, however, is whether people who routinely think about innateness tend to engage in the right sort of principled reflection. Knobe and Samules clearly show that subjects can be prompted to avoid certain errors in their innateness judgments when the question is presented in just the right way. But they do not address whether this is a robust pattern of response, or, alternatively, whether it might be an artefact of their wording in their questionnaire.[1] Nor do they demonstrate that responses to their questionnaire are representative of scientific reasoning “in the field” as it were.

To summarize, the classification of a trait as “innate” encourages researchers to ignore environmental influences on its development while also suggesting that the trait refers to a theoretically and empirically unwarranted cluster of properties. Psychological evidence suggests that the vernacular concept of innateness is associated with an outmoded theory of folk biological essentialism. These considerations suggest, to some, that scientists ought to make a concerted effort to avoid classifying traits as “innate.” As Griffiths (2002) advises:

> *Substituting what you actually mean whenever you feel tempted to use the word “innate” is an excellent way to resist the slippage of meaning. If a trait is found in all healthy individuals or is pancultural, then say so. If a trait has an adaptive-historical explanation, then say that. If it is developmentally canalized with respect to some set of inputs or is developmentally entrenched, then say that it is. If the best explanation of a certain trait difference in a population is genetic, then call this a genetic difference. If you mean that the trait is present early in development, what could be simpler than to say so. If, finally, you want to ‘blackbox’ the development of a trait for the purposes of your current investigation then saying so will prevent your less methodologically reflective colleagues from supposing that you think the trait is … innate (p. 82).*

3 | INNATENESS AS CANALIZATION

Philosopher André Ariew (1996, 1999, 2007) proposes that instead of eliminating innateness, we should define it more precisely in terms of *environmental canalization*—a term coined by biologists C. H. Waddington to describe the interaction between genetic and environmental influences on development. Ariew acknowledges that the vernacular concept of innateness is vague and misleading: "Commonly," he explains, "people associate innateness with a process that is 'in the genes' or 'present at birth.' But such associations when taken out of scientific context are at best hopelessly vague and at worst reminiscent of 18th century preformationism" (Ariew, 1996, p. S19). However, Ariew maintains that innateness plays a legitimate explanatory role within certain scientific disciplines. He cites as successful examples Lorenz's (1935) work on sexual display in mallards, Gould and Marler's (1991) research on birdsong, and Chomsky's (1993) theory of universal grammar. In all of these cases, Ariew observes, the concept of innateness is invoked to explain the *environmental stability* of some trait. That is, innateness is invoked to explain why a trait develops "normally" across a certain range of environments. We can think of this explanatory requirement as the primary job which any adequate definition of innateness must perform.[2] For example, Ariew (1996) argues that it would be a mistake to define innateness in terms of heritability, because heritability is highly sensitive to environmental changes (i.e., unstable).

A similar objection is raised against attempts to define innateness in terms of the possession of a "gene for" a given trait. Genes (i.e., specific alleles) are not the sole factors determining which phenotype is expressed by an organism. This point is vividly exhibited in studies of genetically identical organisms exhibiting markedly different phenotypes (Miko, 2008). Molecular biologists are beginning to understand how epigenetic factors regulate gene expression to produce a stable phenotype across some (often limited) environmental range (Nijhout, Sadre-Marandi, Best, & Reed, 2017). One clear point of consensus is that the mere possession of a "gene for" some trait is insufficient for ensuring the environmentally stability of that trait.

For a more promising definition of innateness, Ariew invokes Waddington's (1940, 1957) notion of environmental canalization. Simply put, an environmentally canalized trait is buffered against certain types and amounts of environmental variation over stages of its development. Waddington described this phenomenon using the metaphor of an "epigenetic landscape" which consists of a ball rolling down a contoured surface. The surface is sloped like a pinball machine. The path traced by a ball as it rolls through one of the valleys is supposed to represent the trajectory of a developing trait. The amount of "shaking" required to make the ball jump out of one valley and into another depends on the overall shape of the landscape. The deeper (more canalized) the valley, the more buffered the trait is against environmental or genetic changes. Waddington distinguished genetically canalized from environmentally canalized traits, depending on which type of factor was primarily responsible for influencing the hills and valleys (see Griffiths & Machery, 2008, for a helpful discussion). However, Ariew focuses just on environmental canalization in his definition of innateness. His proposal is simply that a trait should be considered innate to the extent that it is environmentally canalized in Waddington's sense. One advantage of this definition, he adds, is that it represents innateness as a matter of degree rather than as an all-or-nothing property.

Putting these ideas together, we can reconstruct Ariew's argument as follows:

Primary job criterion:	Any adequate definition of innateness must be able to explain the environmental stability of a trait across a range of environments.
Assumption that canalization is explanatory:	Waddington's notion of environmental canalization along with his metaphor of an epigenetic landscape can explain environmental stability.

Therefore, Waddington's notion of environmental canalization along with his metaphor of an epigenetic landscape provides an adequate definition of innateness.[3]

Of particular interest are the possible senses of "explain" that appear in his two premises. Upon close inspection, we see Ariew invoking two different accounts of scientific explanation when defending his primary job assumption. In his discussion of Konrad Lorenz's work, Ariew (1996, 1999) suggests that innateness provides a *proximate causal* explanation of developmental stability. However, in his discussion of Gould and Marler's work on birdsong, Ariew (2007) suggests that innateness provides a *unifying* explanation of environmental stability. This makes an important difference to our analysis of his second premise. In what sense does Waddington's metaphor *explain* environmental stability? I shall argue momentarily that it fails to provide a proximate causal explanation of this phenomenon. Others have argued that canalization cannot support the kind of unifying explanation that Ariew proposes (Griffiths & Machery, 2008). Thus, Ariew's argument encounters obstacles on either the causal or the unificationist reading of "explain."

Especially in his earlier work, Ariew (1996, 1999) suggests that the concept of innateness should provide a proximate causal explanation of environmental stability. This suggestion is implicit in his qualified acceptance of Lorenz's theory of innateness. Ariew's discussion of Lorenz focuses on his (1935) study of courtship behaviour in mallard ducks (*Anas platyhynchos*). Although female mallards were reared in the exclusive company of pintail ducks (*Anas platyhynchos*), they exhibited courtship behaviour typical of their own species when first encountering a male mallard, at maturity. Lorenz inferred that this behaviour is "innate" rather than "learned." In his later writings, Lorenz (1957) went on to explain that "innate" refers to a trait that is under the developmental control of genetic information. Lorenz further proposed that this information becomes genetically encoded by natural selection. For instance, in the case of mallards, he hypothesized that the preference for species-typical courtship behaviour is selected to prevent outbreeding.

Ariew (1999) emphasizes that although Lorenz associated innateness with natural selection, he primarily understood it as a proximate cause of development. In explicating Lorenz's position, he writes,

> *For adaptive behavior to develop in an individual, the organism requires* information *about its environment …. Either it is provided by the individual's interaction with its environment, or it is provided by the evolutionary process of natural selection, in which case it is encoded in the organism's genes. When the source of information is provided by natural selection, Lorenz argued, the trait is said to be "innate." So the category of innateness is important not only to evolutionary biology, but also to the study of* development … *the key point is that knowing that a trait is a product of natural selection helps explain its development in an individual (Ariew, 1999, p. 121–122* original italics*).*

To many of us, this passage serves as a reminder of why the concept of innateness is so slippery. Lorenz assumed that if a trait is genetically encoded, then it is both environmentally stable and maintained by selection. Whereas we now recognize that these three properties are not necessarily coextensive. Setting aside that issue, however, it is fairly clear that Ariew wants to preserve Lorenz's idea that "innateness should denote an environmentally stable trait," adding that the evolutionary part of his project, "seems promising although it is an open question whether all such traits are explainable via natural selection" (1999, p. 126). Ariew's endorsement of Lorenz's position is further qualified by the fact that (for reasons already mentioned) he does not think that genetic information can adequately explain environmental stability. This is where Waddington's notion of canalization is—supposedly—a better substitute: "According to Waddington, canalization explains why developing organisms tend to produce a number of distinct and well-defined body parts despite environmental perturbation" (1999, p. 128).

Ariew's understanding of canalization as a proximate cause of development is also revealed in his discussion of some contemporary thinkers. For example, in contrasting his own position from Elliot Sober's "invariance" account[4] of innateness, Ariew writes,

> *It is this sense of [environmental] canalization that accounts for the invariance effect that is central to Sober's account of biological innateness. But better than a mere invariance account the effect is* grounded in a real developmental pathway *(2007, p. 579* italics added*).*

Again, the description of canalization as a "real" developmental pathway suggests that he views it as a proximate cause of environmental stability.

By contrast, elsewhere in his writing, Ariew (2007) proposes a very different explanatory role for innateness. He suggests that innateness provides a general framework for comparing the varying degrees of environmental sensitivity among organisms or species. This comes through in his discussion of birdsong acquisition, where Ariew contrasts three different types of learning exhibited by different species of songbird:

> *Type 1 songbirds produce their characteristic song even if the bird is reared in silence. Type 2 birds produce their song only after sessions of call and response with a "tutor": they attempt to mimic the song of any tutor even if the tutor happens to be a member of another species. As for Type 3 songbirds, all that is required to produce their song is contact with some song or other (2007, p. 569).*

These cases are presented as if they vary along a continuum of sensitivity to environmental cues. On Ariew's presentation, Type 1 learning seems the least sensitive. Type 3 learning is only slightly more sensitive since the trait is "triggered" by exposure to a range of songs. Type 2 learning is the most sensitive to the specific songs that a bird encounters. Although Ariew falls short of putting it this way, the suggestion is that Type 1 is "more innate" than Type 3, which is in turn "more innate" than Type 2. Ariew then provides the following account of how this framework is supposed to be explanatory:

> *At this point, one might wonder, what is the value of distinguishing the three types of birds in terms of innateness/triggering/acquired? Some critics have pointed out… that innate descriptions are dispensable in the light of deep causal analysis of the developmental process that each bird undergoes. I agree. If the question is "how do these birds acquire their song?" then we ought to prefer a detailed causal story rather than a rough innate/triggered/acquired distinction. But to sometimes prefer a detailed causal story is not to undermine the value of innateness descriptions that identify distinct developmental patterns (Ariew, 2007, p. 571–572).*

The last sentence is somewhat vague. If he now rejects the idea that innateness ought to provide a proximate causal explanation of environmental stability, what alternative account of scientific explanation might Ariew be proposing? He goes on to state that "In this sense innate ascriptions serve a similar explanatory role as do fitness ascriptions in evolutionary biology," adding that "some commentators claim that fitness explanations unify disparate evolutionary phenomena under one description" (Ariew, 2007, p. 573). Ariew's use of "unify" here is suggestive. I take him to be proposing a form of explanatory unification in the sense developed by Kitcher (1989). In rough outline, an explanatory unification shows how disparate phenomena, previously thought to be unrelated, can be derived using a single, limited vocabulary for describing them. We can apply this to Ariew's birdsong example. Upon first encountering the three types of learning (Types 1, 2, and 3), one might assume that they are entirely distinct mechanisms. However, to further discover that they vary along a single parameter—i.e., the "innateness/triggering/acquired" dimension—would seem to unify them in Kitcher's sense.

We now have two possible interpretations of the Primary job criterion on the table. The first claims that innateness ought to provide a proximate causal explanation of environmental stability. The second claims that innateness should provide a unifying explanation of the differences among organisms or species in their sensitivity to environmental cues. Let us turn to the key question of whether Waddington's account of canalization can fulfil either of these explanatory roles.

When we think carefully about Waddington's landscape metaphor, it is difficult to see how it could be regarded as a "real" causal process. Instead, most molecular biologists view canalization as a phenomenon to be explained rather than as a mechanism that does any explaining. For example, Greg Gibson and Günter Wagner note that "There are likely to be many molecular mechanisms involved [in canalization], including genetic redundancy, feedback regulation, and cooperative biochemical interactions" (2000, p. 373–374). They go on to state that "These are more phenomenological descriptions, however, than quantitative explanations of buffering." If these proposals are merely phenomenological descriptions and thus fall short of explaining canalization, what are we to make of Waddington's landscape metaphor? In what sense does envisioning development as a ball rolling down a hill causally *explain* the environmental stability of some trait? My own sense is that, at best, Waddington's metaphor provides a useful heuristic for imagining, in exceedingly simple terms, how genetic and environmental factors might interact. Gibson and Wagner (2000) are even less charitable, describing Waddington's original notion as an "elusive concept" that requires clarification before it can adequately describe the explanadum of environmental stability, let alone serve as the explanans (see also Debat & David, 2001).

The alternative reading is that canalization offers a unifying explanation of environmental stability. At first blush Waddington's landscape metaphor seems better suited for this purpose. However, in order to provide a unifying explanation of different traits, there must be some limited vocabulary for describing them. This is apparently what Ariew has in mind when depicting canalization as a single dimension of environmental buffering (i.e., his "innate/triggered/acquired" dimension). The question is whether it is acceptable to view development in such simple terms.

In their criticism of Ariew's proposals, Griffiths and Machery (2008) portray canalization as a *multidimensional* phenomenon. On their view, there is no such thing as the degree to which a trait is simply buffered against "the" environment. Rather, for every trait, there are different types of environmental factor capable of influencing its development, each to varying degrees. On this view, whether some trait is classified as relatively "innate" or "acquired" depends on which factors a researcher focuses on. To take one of their examples, the songs of certain species of male cowbird are relatively insensitive to other male members of their species. This is in contrast to many species of American sparrow which are more sensitive to conspecific male songs. So, when we focus on just this factor—male conspecific song—the cowbirds seem relatively canalized or "innate" compared to the sparrows. However, other researchers focus on the ways that courtship interactions (with females) influence song development. When it comes to this environmental factor, the song of a male cowbirds is much more sensitive (less canalized) than most sparrows. Hence, song development will appear more or less canalized or "innate" in these taxa depending on which environmental factor we focus. Note also that these are just two of indefinitely many environmental factors that might influence its development. We might also consider the number of nest mates, background noise, or a host of less obvious ecological variables. Hence, Griffiths and Machery conclude:

> *The strategy of placing traits along a continuum of independence of the environment founders on the fact that there is no continuum, but rather a high-dimensional space whose axes are often specific to one or a few species. Even when the same axes can be applied to different species, so that canalization of a character in two species can be meaningfully compared, there is no systematic tendency for traits that are at the "innate" end of one axis to be at the "innate" end of the other. (2008, p. 402)*

Let us accept that canalization is a multidimensional property. Does this undermine Ariew's attempt to use it as a unifying concept? In order to provide any sort of unifying explanation of environmental stability there would have to be some limited set of parameters that characterize the degree of stability exhibited by a given trait. Griffiths and Machery clearly show that there is no single parameter of environmental sensitivity for describing birdsong. But could some wider (but not open ended) set of parameters perhaps explain a diverse range of stabilizing mechanisms? This is a topic for future research. However one thing seems fairly clear, that the more dimensions of environmental stability/sensitivity that are discovered, the less likely it is that Waddington's simple landscape metaphor can capture them.

Griffiths and Machery (2008) suggest a weaker reading of Ariew's position than the two we have been entertaining so far. Instead of proposing that the primary job for innateness is to explain (in some sense) environmental stability, they take Ariew to be defending a *heuristic* role for this concept. This heuristic interpretation seems to be based on the fact that Ariew often points to the use of isolation experiments as a strategy for investigating innateness. These experiments involve the removal of some "normal" environmental factor during development in order to determine its influence on some focal trait. Both Lorenz and Marler used this technique to investigate animal behaviour. Chomsky applies a similar logic in his poverty of the stimulus argument. Ariew can be interpreted as arguing that the success of this strategy in the fields of ethology and linguistics recommend it as a good heuristic for investigating cognition and behaviour more generally.

It is notoriously difficult to critically assess the heuristic value of a conceptual framework. Doing so would require a theory-neutral measure of progress against which the success of alternative frameworks can be measured. To my knowledge, no one has undertaken a rigorous assessment of whether the concept of innateness has positive heuristic value. However, Griffiths and Machery (2008) offer a fairly compelling prima facie reason for expecting otherwise. Drawing again from the scientific literature, they point out that environmental influences on development are often epistemically opaque. That is, it is often impossible to predict from a pretheoretical perspective whether changes to a given environmental factor will influence the development of a focal trait. The developmental literature abounds with examples of unexpected influences on development. To cite one of Griffiths' favourite examples, the development of aggression in adult rats is directly influenced by the posture adopted by their mother while suckling (Meaney, 2001). Who would have guessed? Now, suppose that every trait is subject to such unexpected environmental influences. As Lehrman (1953) also noted, researchers who operate with a concept of innateness are likely to overlook such non-obvious environmental influences. Griffiths and Machery conclude that “The concept of innateness is an anti-heuristic which encourages researchers to check the obvious sources of environmental input, and then to stop looking” (Griffiths & Machery, 2008, p. 403).

In sum, there are several challenges facing Ariew's revisionary proposal. The argument that canalization explains stability requires either an unjustified reification of the landscape metaphor or it fails to appreciate the multidimensional nature of environmental sensitivity. Nor does canalization provide an obviously good heuristic for studying the causes of stability. Yet Ariew's proposal has been extremely instructive for showing just how difficult it is to develop a scientifically legitimate definition of innateness. Let us now turn to a very different attempt to do so.

4 | INNATENESS AS AN EXPLANATORY PRIMITIVE

Samuels (1998, 2002, 2007) offers a very different account of the role for innateness in scientific discourse. He is particularly interested in the tendency for cognitive scientists (more so than biologists) to employ the innate/learned distinction. For Samuels, this practice suggests that the innateness concept is doing important theoretical work in this discipline (see also Khalidi, 2007, 2009). Specifically, he proposes that innate capacities serve as the explanatory primitives for cognitive science. On this view, to say that some trait is innate means that cognitive science cannot, in principle, account for its acquisition. Instead, innate traits fall under the jurisdiction of some other discipline, such as developmental biology or genetics.

A similar suggestion was proposed by Fiona Cowie (1999), who observed that researchers who appeal to innateness are often passing the explanatory buck to some other discipline. While some thinkers disparage such buck-passing behaviour (e.g., Lehrman, 1953), Samuels sees it as a virtue. On his view, cognitive science has, by its nature, a limited set of explanatory resources at its disposal. When it comes to explaining how traits are acquired, cognitive scientists can appeal to various sorts of learning and perhaps a few other cognitive processes—and nothing else. As soon as they venture into the territory of “biological” processes (e.g., neurotransmitters or evolution), they are engaged in a fundamentally different mode of explanation. The role of the innate/learned distinction, for Samuels, is to mark this disciplinary boundary.

Importantly, Samuels allows that innate capacities can themselves play an explanatory role in psychology. I think that the role played by the concept of valence in traditional learning theory illustrates this idea. It is taken for granted that certain stimuli have positive valence, while others are experienced as negative. Valence can be transferred from an unconditioned to a conditioned stimulus. Hence, valence is a theoretical concept that explains how certain behaviours are acquired. However, the capacity to experience certain stimuli as valenced is presumably explained in biological (not cognitive) terms. This would make valence an innate capacity on Samuels' account.

Samuels anticipates a potential problem: Some psychological traits that (intuitively) do not qualify as innate are, nonetheless, explainable only in biological terms. For example, one type of cognitive impairment in decision making is caused by damage to the prefrontal cortex (Fellows & Farrah, 2005). Samuels maintains that it would be a mistake to classify such cases of "acquired sociopathy" as innate. This motivates him to qualify his proposal: A trait is innate just in case it functions as an explanatory primitive for some correct psychological theory and it is *acquired by the organism as a part of its "normal" course of development*.

This appeal to normalcy has been criticized by Mameli and Bateson (2006), who argue that the very idea of a standard developmental environment is vague and misleading. Over the evolutionary history of a given trait, indefinitely many environmental factors will have influenced its development. Contemporary environments will resemble ancestral conditions in certain respects and not others. Samuels assumes that there is some non-arbitrary way to dichotomize developmental environments into normal vs. abnormal. It is unclear how this would work in practice.

Khalidi (2007) raises a similar objection to Samuels' normalcy condition. His chief example is the phenomenon of adolescent depression. Khalidi argues that a complete explanation of this trait would appeal to neurotransmitters in the brain, thus rendering it a form of biological explanation on Samuels' view. The problem for Samuels is that this trait is sufficiently common to be considered within the normal phenotypic range. Hence, it is not the product of some "abnormal" developmental process. "Clearly," he adds, "not all such forms of depression are innate: indeed, adolescent depression is thought to be less heritable than other forms of mental depression" (Samuels, 2007, p. 97). How, then, are such traits to be classified according to Samuels' framework?

Even if this issue can be resolved, there is potentially a deeper problem with Samuels' proposal. As mentioned, he assumes a fairly sharp division among scientific disciplines. Some philosophers and (I suspect) most scientists would resist this idea. One sort of problem is that it does not allow for the incorporation of biological concepts and models into cognitive science. Yet there are many examples in which these disciplines overlap. A classic example is Garcia and Koelling's (1966) work on taste aversion learning. Darwinian models have also been profitably applied to a variety of cultural phenomena (e.g., Boyd & Richerson, 1988; Sterelny, 2003). It has also proven insightful to think of learning as a form of phenotypic plasticity (English, Fawcett, Higginson, Trimmer, & Uller, 2016). In addition to these specific examples, I could also point to the entire disciplines of evolutionary psychology and cognitive neuroscience. All of these cases suggest that the boundaries between cognitive science and biology are more porous than Samuels would have us imagine.

Of course, Samuels would probably remind us that he is interested in disciplinary boundaries that exist *in principle*, not the ones that are occasionally transcended in practice. The idea, then, is that innate capacities are those that would be settled upon by some more ideal version of cognitive science. But Samuels needs to be careful here. This move threatens to make his position immune to any sort of empirical evidence to the contrary. Besides, we started out with the observation that cognitive science *as it is currently practiced* appeals to the innate/learned distinction. To be told that this distinction might be useful to some future (and potentially very different) version of cognitive science is hardly a satisfying analysis of this discipline as we know it.

I have been pushing against the idea of a sharp division between cognitive science and biology. The canonical statement of this view is Fodor's (1974) "Special Sciences" paper. On his view, each scientific discipline is constituted by a set of ceteris paribus laws that describe the behaviour of certain higher-level entities. Fodor argued that the laws of cognitive science are multiply realizable and, therefore, autonomous from lower-level sciences like biology.
However, this picture is difficult to reconcile with the way that psychological phenomena are actually explained (Bechtel & Mundale, 1999). In recent years, an alternative, *mechanistic* model of psychological explanation has become popular (see Andersen, 2014a, 2014b, for a review). According to this picture, the explanation of a given psychological phenomenon involves describing the entities and activities that reliably produce it. Importantly, those entities and activities are often drawn from both psychology and biology (Bechtel, 2008). In fact, Khalidi's example of adolescent depression offers an illustration of such a mechanistic explanation. This phenomenon is partly explained in terms of the psychological states that it involves, and partly in terms of conventional biological entities, like neurotransmitters (e.g., Sapolsky, 2004). A mechanistic explanation of adolescent depression is not restricted to a single level, and any explanation that ignores either cognitive or biological elements would be incomplete. Such examples challenge the idea that cognitive science would benefit from a concept that draws a territorial line between psychological and biological phenomena.

5 | SUMMARY AND FUTURE DIRECTIONS

We have considered three reasons for eliminating the concept of innateness from science. To label a trait "innate" tends to discourage researchers from exploring all but the most obvious environmental influences on its development. This is why Lehrman and others view the concept as (to quote Griffiths and Machery) an "anti-heuristic." Samuels suggests that there is a basic ontological distinction between the domains of psychology and biology and that the concept of innateness marks the boundary. I have suggested that this does not accord with the mechanistic model of explanation found in these disciplines. A second reason for discarding innateness from science is because it facilitates questionable inferences among i-properties. We have explored one of the more laudable attempts to define innateness in terms of one particular i-property, environmental canalization. However, this concept fails to provide either a mechanistic or a unificationist explanation of developmental stability. The third objection to innateness stems from its association with folk essentialism. So long as traits continue to be labelled "innate," "in the DNA" or with some other locution that evokes our folk-essentialist framework, there will remain a tendency to lapse into outmoded ways of thinking about development. I would suggest that a more practical strategy is to self-consciously avoid the use of these terms in cognitive science and thereby err on the side of caution.

Our investigation points towards several questions for future research:

- *Is innateness associated with a productive research program?* Philosophers have developed strategies for distinguishing stagnant versus productive research programs (e.g., Laudan, 1977). Some research programs in cognitive science are more wedded to innateness-talk than others. It would be interesting to rigorously compare the productivity of these traditions.
- *In which contexts do scientists employ particular conceptions of innateness?* Looking closely at the precise contexts in which innateness is invoked by cognitive scientists would help illuminate whether the term does any distinctive work. In particular, Knobe and Samuels' (2013) finding that innateness judgments are context sensitive requires further exploration.
- *Do i-properties cluster?* This empirical question could potentially be addressed using meta-analysis or some similar technique that systematically reviews the psychological and biological literature.
- *Are there general types of buffering mechanism?* A growing literature in molecular and developmental biology investigates the mechanisms that buffer traits against certain environmental influences (e.g., Siegal & Bergman, 2002). It would be a valuable exercise to determine whether there is some set of dimensions that might unify these mechanisms, regardless of whether those dimensions get associated with innateness.

ACKNOWLEDGEMENTS

Thanks to Don Dedrick, Edouard Machery, and an anonymous reviewer for comments on a previous draft. This research was supported by the Social Sciences and Humanities Research Council of Canada.

ENDNOTES

[1] In their experimental condition, Knobe and Samuels' (2013) claim to be "encouraging [participants] to think in a principled way about the considerations that are influencing their judgments" about innateness (p. 82). However, the authors describe their method for encouraging principled reflection as follows:

> *Participants in the principled condition were told at the outset that the two versions differed only in a few words (which were underlined for easy identification) and that we specifically wanted to know whether they thought that this difference was relevant to whether the trait was innate (2013, p. 80).*

This method of priming participants to focus on specific underlined words is a rather heavy-handed manipulation. As such, it runs the risk of an observer-expectancy effect (Rosenthal, 1964). It would thus be prudent, before accepting Knobe and Samuels' conclusion that principled reasoning about innateness has an influence on people's judgments, to validate this result with additional studies. Ideally, they would employ subtler encouragements for participants to reason in a principled fashion.

[2] In his earlier work, Ariew (1996, 1999) identified two other jobs for innateness besides the explanation of environmental stability. One is the general requirement that innateness is a property of a developing trait. The other is that innateness "should make clear how natural selection can effect the prevalence of some adaptive traits" (1999, p. 126–7). With the latter criterion, Ariew seems to suggest that innateness must be tied to an "ultimate" explanation of how a trait evolved. However, in his more recent work, Ariew (2007) backs off the third criterion and focuses just on environmental stability as his primary desideratum.

[3] Ariew fails to address the practicality of his revisionist proposal, simply stating that "rather than eliminating folk usage, we ought to clarify it" (Ariew, 2007, p. 571).

[4] Sober's (1998) invariance account claims that a trait is innate just in case the genetic factors influencing it exhibit a flat norm of reaction (i.e., no phenotypical variation) across some range of normal environments.

ORCID

Stefan Linquist http://orcid.org/0000-0003-4654-0419

WORKS CITED

Andersen, H. (2014a). A field guide to mechanisms: Part I. *Philosophy Compass*, (4), 274–283.

Andersen, H. (2014b). A field guide to mechanisms: Part II. *Philosophy Compass*, *4*, 283–297.

Ariew, A. (1996). Innateness and canalization. *Philosophy of Science*, *63*, S19–S27.

Ariew, A. (1999). Innateness is canalization: A defense of a developmental account of innateness. In V. Hardcastle (Ed.), *Biology meets psychology: conjectures, connections, constraints* (pp. 117–138). Cambridge, MA: MIT Press.

Ariew, A. (2007). Innateness. In M. Matthen, & C. Stephens (Eds.), *Philosophy of biology* (pp. 567–584). Amsterdam: Elsevier.

Atran, S., Medin, D. L., & Ross, N. (2002). Thinking about biology: Modular constraints on categorization and reasoning in the everyday life of Americans, Maya, and scientists. *Mind & Society*, *6*, 31–63.

Bateson, P. (1966). The characteristics and context of imprinting. *Biological Reviews*, *41*, 177–220.

Bateson, P. (1985). Problems and possibilities in fusing developmental and evolutionary thought. In G. Butterworth, J. Rutowska, & M. Scaife (Eds.), *Evolution and developmental psychology* (pp. 3–12). London: Harvester Press.

Bateson, P. (1991). Are there principles of behavioural development? In P. Bateson (Ed.), *The development and integration of behavior* (pp. 19–39). Cambridge, MA: Cambridge University Press.

Bateson, P., & Mameli, M. (2007). The innate and the acquired: Useful clusters or a residual distinction from folk biology? *Developmental Psychobiology*, *49*, 818–831.

Bateson, P., & Martin, P. (1999). *Design for a life: how behaviour and physiology develop*. London: Jonathan Cape.

Bechtel, W. (2008). Mechanisms in cognitive psychology: What are the operations? *Philosophy of Science*, *75*, 983–994.

Bechtel, W., & Mundale, J. (1999). Multiple realizability revisited: Linking cognitive and neural states. *Philosophy of Science*, *66*, 175–207.

Birch, J. (2009). Irretrievably confused? Innateness in explanatory context. *Studies in History and Philosophy of Biological and Biomedical Sciences*, *40*, 296–310.

Boyd, R., & Richerson, P. J. (1988). *Culture and the evolutionary process*. Chicago: Chicago U Press.

Carey, S. (2009). *The origin of concepts*. Oxford: New York.

Chomsky, N. (1993). On the nature, use, and acquisition of language. In A. Goldman (Ed.), *Readings in philosophy and cognitive science* (pp. 511–534). Cambrdige MA: MIT Press.

Collins, J. (2003). Cowie on the poverty of the stimulus. *Synthese*, *136*, 159–190.

Collins, J. (2005). Nativism: In defense of a biological understanding. *Philosophical Psychology*, *18*, 157–177.

Cowie, F. (1999). *What's within? Nativism reconsidered*. Oxford: Oxford University Press.

Day, J. J., & Sweatt, J. D. (2011). Epigenetic mechanisms in cognition. *Neuron*, *70*, 813–829.

Debat, V., & David, P. (2001). Mapping phenotypes: Canalization, plasticity, and developmental stability. *Trends in Ecology & Evolution*, *16*, 555–561.

Dutton, D. (2009). *The art instinct: Beauty, pleasure, and human evolution*. Oxford: New York.

English, S., Fawcett, T. W., Higginson, A. D., Trimmer, P. C., & Uller, T. (2016). Adaptive use of information during growth can explain long term effects of early life experiences. *The American Naturalist*, *187*, 620–632.

Fellows, L. K., & Farrah, M. J. (2005). Different underlying impairments in decision-making following ventromedial and dorsolateral frontal lobe damage in humans. *Cerebral Cortex*, *15*, 58–63.

Fodor, J. (1974). Special sciences (or: The disunity of science as a working hypothesis). *Synthese*, *28*, 97–115.

Fodor, J. (2001). Doing without what's within: Fiona Cowie's critique of nativism. *Mind*, *110*, 99–148.

Garcia, J., & Koelling, R. A. (1966). Relation of cue to consequences in avoidance learning. *Psychonomic Science*, *4*, 123–124.

Gibson, G., & Wagner, G. (2000). Canalization in evolutionary genetics: A stabilizing theory? *BioEssays*, *22*, 372–380.

Gould, J., & Marler, P. (1991). Learning by instinct. In D. Mock (Ed.), *Behaviour and evolution of birds* (pp. 4–19). San Francisco: Freeman.

Gould, S. J., & Lewontin, R. (1979). The spandrels of San Marco and the Panglossian paradigm: A critique of the adaptationist programme. *Proceedings of the Royal Society of London Series B*, *205*, 581–598.

Griffiths, P. E. (1997). *What emotions really are: the problem of psychological categories*. Chicago: University of Chicago Press.

Griffiths, P. E. (2002). What is innateness? *The Monist*, *85*, 70–85.

Griffiths, P. E., & Machery, E. (2008). Innateness, canalization, and “biologicizing the mind.”. *Philosophical Psychology*, *21*, 397–414.

Griffiths, P. E., Machery, E., & Linquist, S. (2009). The vernacular concept of innateness. *Mind & Language*, *24*, 605–630.

Khalidi, M. A. (2007). Innate cognitive capacities. *Mind & Language*, *22*, 92–115.

Khalidi, M. A. (2009). Should we eliminate the innate? Reply to Griffiths and Machery. *Philosophical Psychology*, *22*, 505–519.

Kitcher, P. (1989). Explanatory unification and the causal structure of the world. In P. Kitcher, & W. Salmon (Eds.), *Scientific explanation* (pp. 410–505). Minneapolis: University of Minnesota Press.

Kitcher, P. (2001). Battling the undead: How (and how not) to resist genetic determinism. In R. Singh, K. Krimbas, D. Paul, & J. Beatty (Eds.), *Thinking about evolution: Historical, philosophical and political perspectives (festchrifft for Richard Lewontin)* (pp. 396–414). Cambridge: Cambridge University Press.

Knobe, J., & Samuels, R. (2013). Thinking like a scientists: Innateness as a case study. *Cognition*, *126*, 72–86.

Laudan, L. (1977). *Progress and its problems: towards a theory of scientific growth*. U. California Press.

Lehrman, D. S. (1953). A critique of Konrad Lorenz's theory of instinctive behavior. *Quarterly Review of Biology*, *28*, 337–363.

Linquist, S., Machery, E., Griffiths, P. E., & Stotz, K. (2011). Exploring the folkbiological conception of innateness. *Philosophical Transactions of the Royal Society B*, *366*, 444–453.

Lorenz, K. (1935). Der kumpen in der umwelt des vogels. *Journal for Ornithologie*, *83*, 137–213.

Lorenz, K. (1957). The nature of instincts. In C. F. Schiller (Ed.), *Instinctive behavior* (pp. 129–175). New York: International University Press.

Machery, E. (2017). *Philosophy within its proper bounds*. Oxford: Oxford University Press.

Maclaurin, J. (2002). The resurrection of innateness. *The Monist*, *85*, 105–130.

Mallon, R., & Weinberg, J. (2006). Innateness as closed-process invariantism. *Philosophy of Science*, *73*, 323–344.

Mameli, M. (2004). Nongenic selection and nongenic inheritance. *The British Journal for the Philosophy of Science*, *55*, 35–71.

Mameli, M. (2008). On innateness: The clutter hypothesis. *The Journal of Philosophy*, *105*, 719–736.

Mameli, M., & Bateson, P. (2006). Innateness and the sciences. *Biology and Philosophy*, *21*, 155–188.

Mameli, M., & Bateson, P. (2011). An evaluation of the concept of innateness. *Philosophical Transactions of the Royal Society B*, *366*, 436–443.

McGowan, P. O., & Roth, T. L. (2015). Epigenetic pathways through which experiences become linked with biology. *Development and Psychopathology*, *27*, 637–648.

Meaney, M. J. (2001). Maternal care, gene expression, and the transmission of individual differences in stress reactivity across generations. *Annual Review of Neuroscience*, *24*, 1161–1192.

Miko, I. (2008). Phenotype variability: Penetrance and expressivity. *Nature Education*, *1*, 137.

Nijhout, H. F., Sadre-Marandi, F., Best, J., & Reed, M. C. (2017). Systems biology of phenotypic robustness and plasticity. *Integrative and Comparative Biology*, *57*, 171–184.

O'Neill, E. (2015). Relativizing innateness: Innateness as the insensitivity to the appearance of a trait with respect to specified environmental variation. *Biology and Philosophy*, *30*, 211–225.

Prinz, J. (2008). Is morality innate. In W. Sinnott-Armstrong (Ed.) *Moral psychology: The evolution of morality: Adaptations and innateness* (pp. 367-406). Cambridge MA; MIT Press 2008.

Rosenthal, R. (1964). Experimenter outcome-orientation and the results of the psychological experiment. *Psychological Bulletin*, *61*, 405–412.

Samuels, R. (1998). What brains won't tell us about the mind: A critique of the neurobiological argument against representational nativism. *Mind & Language*, *13*, 548–570.

Samuels, R. (2002). Nativism in cognitive science. *Mind & Language*, *17*, 233–265.

Samuels, R. (2007). Is innateness a confused concept? In P. Carruthers, S. Laurence, & S. P. Stich (Eds.), *The innate mind*, *volume 3* (pp. 17–36). Oxford: Oxford University Press.

Sapolsky, R. M. (2004). *Why zebras don't get ulcers: An updated guide to stress, stress-related diseases, and coping*. New York: Henry Holt and Co.

Schoneberger, T. (2005). A philosopher's war on poverty of the stimulus arguments: A review of Fiona Cowie's What's within? Nativism reconsidered. *The Analysis of Verbal Behaviour*, *21*, 191–207.

Shea, N. (2012). Genetic representation explains the cluster of innateness-related properties. *Mind & Language*, *27*, 466–493.

Siegal, M. L., & Bergman, A. (2002). Waddington's canalization revisited: Developmental stability and evolution. *Proceedings of the National Academy of Science*, *99*, 10528–10532.

Sober, E. (1980). Evolution, population thinking, and essentialism. *Philosophy of Science*, *47*, 350–383.

Sober, E. (1998). Innate knowledge. In E. Craig, & L. Floridi (Eds.), *Routledge encyclopedia of philosophy* (pp. 794–797). London: Routledge.

Sterelny, K. (2003). Thought in a hostile world. *Malden MA*. Blackwell.

Stich, S. P. (1975). I*nnate* ideas. Los Angeles: University of California Press.

Stotz, K. (2017). Why developmental niche construction is not selective niche construction: And why it matters. *Interface Focus*, *7*, 20160157.

Waddington, C. H. (1940). *Organizers and Genes*. Cambridge: Cambridge University Press.

Waddington, C. H. (1957). *The strategy of the genes*. London: Allen and Irwin.

Wells, D. L., & Hepper, P. G. (2006). Prenatal olfactory learning in the domestic dog. *Animal Behaviour*, *72*, 681–686.

Wendler, D. (1996). Innateness as an explanatory concept. *Biology and Philosophy*, *11*, 89–116.

West, M. J., & King, A. P. (1987). Settling nature and nurture into an ontogenetic niche. *Developmental Psychobiology*, *20*, 549–562.

West-Eberhard, M. J. (2003). *Developmental plasticity and evolution*. New York: Oxford University Press.

Wilson, D. S., & Kniffin, K. M. (1999). Multilevel selection and the social transmission of behavior. *Human Nature*, *10*, 291–310.

Wimsatt, W. C. (1999). Generativity, entrenchment, evolution and innateness: Philosophy, evolutionary biology, and conceptual foundations of science. In V. Hardcastle (Ed.), *Where biology meets psychology: Philosophical essays* (pp. 139–179). Cambridge MA: MIT Press.

Stefan Linquist works at the intersection between genomics, ecology, and cognitive science. He has written and co-authored papers in Mind and Language, Philosophical Transactions of the Royal Society, Philosophy of Science, The British Journal for the Philosophy of Science, The Quarterly Review of Biology, Bioessays, Molecular Ecology, The American Naturalist, and Biology and Philosophy. Currently, he is engaged in three active projects. One is an exploration of the culture of honour hypothesis and its significance as a case study for cultural evolution. The second is an examination of the function/junk distinction in genomics. The third is an investigation of generality in ecology, focusing in particular on unorthodox ecological systems like micro-organisms and mobile genetic elements. Stefan is Associate Professor of Philosophy and Adjunct Professor of Biology at the University of Guelph. He teaches courses in the philosophy of science (biology and psychology). He did a PhD in philosophy at Duke University (2005) and an MSc in biology at Binghamton University (2000).

How to cite this article: Linquist S. The conceptual critique of innateness. *Philosophy Compass*. 2018;13:e12492. https://doi.org/10.1111/phc3.12492