

Understanding the evolution of psychometric g requires understanding how it comes about. As early as 1916, Thomson (1916) showed that it is sufficient to postulate underlying group factors that influence several tests to obtain a positive manifold without a general factor (see also Bartholomew et al. 2009). Reframed in modern psychological terms, an elementary analysis of tests shows that no test is a pure measure of a cognitive function (or construct). The relationship between cognitive functions and test scores is many-to-many: Each test score is influenced by several cognitive functions, and each cognitive function influences several test scores (in the same direction). The latter observation suffices to explain that test scores are positively correlated. We submit that the logic of Thomson's bonds model is much more general, as it also applies to factors underlying cognitive functions. Indeed, each brain function or property (e.g., frontal gray matter volume, nerve conductance velocity, dopamine synthesis, etc.) influences several cognitive functions, thereby inducing intrinsic positive correlations between cognitive functions. One step further back, each gene expressed in the brain (e.g., genes that code for neurotrophic factors, transcription factors, and any molecule involved in neurotransmission) typically influences several brain functions and properties, thereby inducing positive correlations between them. In parallel, many environmental factors (e.g., nutrition, socioeconomic status, education, diseases, and so on) influence more than one brain or cognitive function, thereby inducing further correlations. Finally, van der Maas et al. (2006) have shown that positive correlations between cognitive functions may emerge through mutual interactions in the course of cognitive development, even in the absence of intrinsic correlations. Thus, all of the factors underlying test performance are pleiotropic and conspire to produce positive correlations at all levels of description, hence the emergence of the positive manifold.

Note that, according to the explanation given previously, the positive manifold can arise in an entirely modular mind (because modules selected for different purposes nevertheless have to share underlying factors), and therefore there is no antagonism between modularity and general intelligence. Furthermore, the very same pleiotropic mechanisms are at work in other species and, therefore, readily explain that a g factor can be measured in nonhuman primates, rodents, and probably all organisms with a nervous system. Finally, in the speciation process, genes that progressively diverge between two populations influence more than one brain and cognitive function; therefore, the two populations are bound to eventually differ in more than one brain and cognitive function. This directly predicts that performance in different tests should covary across species, or what the authors term G . Thus, all of the evidence that the authors gather in support of a reified notion of general intelligence is more parsimoniously explained by the pleiotropy of the underlying factors, within and across species. The "independent evolution of large numbers of modules instead of general intelligence" is not "particularly difficult to reconcile with interspecific findings of G " (sect. 2.5, para. 5); it directly follows from an understanding of what modules are made of: the same building blocks, shared between species.

There is, therefore, no need to postulate that the positive manifold reflects one particular cognitive function or one brain function, whose evolution would require a special explanation. The positive manifold emerges spontaneously from the pleiotropy of all of the underlying factors. Only these underlying factors require an evolutionary explanation. It is indeed very interesting to inquire about the evolution of genes involved in brain development and function, the evolution of brain functions and properties, and the evolution of cognitive functions. If there is any brain or cognitive function whose evolution is a major puzzle, then it should be identified and studied as such. However, this is not the case for general intelligence, which does not reduce to a single brain or cognitive function, and whose evolution follows directly from that of the underlying biological, cognitive, and environmental factors.

General intelligence does not help us understand cognitive evolution

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David M. Shuker,^a Louise Barrett,^b Thomas E. Dickins,^c Thom C. Scott-Phillips,^d and Robert A. Barton^d

^aSchool of Biology, University of St Andrews, St Andrews KY16 9TH, United Kingdom; ^bDepartment of Psychology, University of Lethbridge, Lethbridge, Alberta T1K 3M4, Canada; ^cSchool of Science & Technology, University of Middlesex, London NW4 4BT, United Kingdom; ^dEvolutionary Anthropology Research Group, Durham University, Durham DH1 3LE, United Kingdom.

dms14@st-andrews.ac.uk <https://insects.st-andrews.ac.uk/>
 louise.barrett@uleth.ca <http://directory.uleth.ca/users/louise.barrett>
 T.Dickins@mdx.ac.uk <https://www.mdx.ac.uk/about-us/our-people/staff-directory/profile/dickins-tom>
 t.c.scott-phillips@durham.ac.uk <https://thomscottphillips.wordpress.com>
 r.a.barton@durham.ac.uk <https://www.dur.ac.uk/anthropology/staff/academic/?id=122>

Abstract: Burkart et al. conflate the domain-specificity of cognitive processes with the statistical pattern of variance in behavioural measures that partly reflect those processes. General intelligence is a statistical abstraction, not a cognitive trait, and we argue that the former does not warrant inferences about the nature or evolution of the latter.

Is "the presence of general intelligence" the "major evolutionary puzzle" that Burkart et al. claim? Like much of the literature on general intelligence in animals, the target article draws inferences about the nature and evolution of cognitive traits from the correlations among measures of performance, both within and between species. The "positive manifold" (sect. 1.1, para. 1) is thus taken to be a nontrivial finding, and g is treated as being – or reflecting – a trait with causal effects (a mechanism). g , however, is of course a statistical construct: When the authors refer to "the structure of cognition" (sect. 1.1.1, para. 1), what they actually describe is the statistical structure of variance in performance on behavioural tests. What can this statistical structure tell us about cognitive traits? We suggest that it tells us very little, or possibly nothing, because of the multiple plausible ways in which it might arise. Moreover, the analysis of g fails to provide a clear framework for empirical research, because the putative underlying mechanism, general intelligence, cannot be meaningfully defined in the absence of the correlations that are used as evidence for its existence.

More specifically, the reification of g involves a conflation of the proposed domain-generality of cognitive processes with the statistical pattern of variance in the behavioural output of those processes. Thus, "Massive modularity would appear to be irreconcilable with general intelligence" (sect. 1.2.1, para. 4) – well, only in the sense that apples are irreconcilable with oranges. Burkart et al. follow many in assuming that the positive manifold can be explained "by positing a dominant latent variable, the g factor, associated with a single cognitive or biological process or capacity" (van der Maas et al. 2006, p. 842). As pointed out by the latter authors, other explanations, which account for not only the presence of g but also its heritability and neuro-anatomical correlates, are not only possible, but also plausible. In citing van der Maas et al. (2006), Burkart et al. explicitly "equate general intelligence with the positive manifold" (sect. 1.1.1, para. 3), implying that their position and that of van der Maas et al. are in harmony. The point emphasised by van der Maas et al., however, and the point we also emphasise, is that the positive manifold provides little or no constraint on the possible architectures of cognition.

To labour the point, correlated variance does not imply any particular kind of cognitive process. That said, we might still want an explanation for why performance or behaviours are correlated across domains. Here, in brief, are some possibilities.

(1) They are not really different domains. For example, Reader et al. (2011) and Fernandes et al. (2014) found positive correlations among the rates of social deception, social learning, innovation,

extractive foraging, dietary breadth, percentage of fruit in the diet, and tool use across primate species, leading both sets of authors to conclusions about the domain-generalness of cognitive processes. Neither these authors nor Burkart et al. explain how a domain is to be identified, and therefore how these behavioural measures can, in principle, be used to test for domain-generalness. We can envisage plausible arguments to the effect that at least some of these behaviours draw on the same domain-specific processes. It is a question of natural ontologies: How do we carve nature at her joints? The only way that makes sense to us is in an evolutionary context where we identify a domain with a selection pressure. Deciding that “social” and “non-social” are distinct domains is, therefore, a hypothesis about what selection pressures have operated, not necessarily a fact about the world. Burkart et al. recognise this problem (“The issue of task selection is thus closely linked to the identification of domains in animal cognition” [sect. 2.4.2, para. 5]) but do not offer a convincing solution.

(2) Related to (1), it may well be that the behaviours measured are influenced by cognitive processes they share in common, but this does not mean it is helpful to describe those processes as “general processes,” or that together they comprise “general intelligence.” For example, primate species vary in their sensory-motor adaptations – in particular, in their stereo visual acuity and manual manipulative abilities – and these differences correlate with the evolution of binocular convergence supporting stereo vision, the size of visuomotor structures in the brain, and consequently overall brain size (Barton 2012; Heldstab et al. 2016). Clearly, such sensory-motor specializations may influence performance of a range of behaviours and/or experimental test procedures. Yet, describing them as “domain general” tells us nothing about how they work or how they evolved. We also do not share the optimism of Burkart et al. that reversal learning is free of such problems.

(3) Niche dimensions tend to be correlated (Clutton-Brock & Harvey 1977). For example, folivorous primates generally live in smaller social groups, have smaller home ranges, and engage less in extractive foraging and tool use than more omnivorous primates. Cognitive adaptations for specific niche dimensions could therefore theoretically be completely informationally encapsulated and yet performance across domains would still be correlated.

(4) The rates of naturally occurring behaviours in the wild (Reader et al. 2011; Fernandes et al. 2014, cited by Burkart et al.), may be systematically biased, leading to spurious correlations. Although these studies attempt to control for observation effort, they don't control for the number of individuals under observation. Rates of all behaviours will, other things being equal, correlate positively with group size and therefore with each other, because more individuals are under observation per unit time in larger groups. Variation in observability due to habitat will only exacerbate the problem. The implications are obvious.

For a theory to be useful, it has to be well defined in such a way as to generate testable predictions that differentiate it from other theories. Burkart et al., along with the wider literature on general intelligence and *g*, fail to achieve this. If we are to make progress in our efforts to understand the evolution and structure of cognition, we need to stop confusing the map for the territory.

It's time to move beyond the “Great Chain of Being”

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Robert J. Sternberg

Department of Human Development, Cornell University, Ithaca, NY 14853.

robert.sternberg@gmail.com www.robertjsternberg.com
robert.sternberg@cornell.edu

Abstract: The target article provides an anthropocentric model of understanding intelligence in nonhuman animals. Such an idea dates

back to Plato and, more recently, Lovejoy: On Earth, humans are at the top and other animals at successively lower levels. We then evaluate these other animals by our anthropocentric folk theories of their intelligence rather than by their own adaptive requirements.

Burkart et al. have written a very interesting, erudite, and anthropocentric account in the target article of how principles discovered for human intelligence might be generalized to animals other than humans. The presuppositions behind this article are captured well by Lovejoy (1936) in his book, *The Great Chain of Being*. The general idea, which goes back to Plato and Aristotle, is that there is a Great Chain of Being containing, among other entities, God at the top, then humankind, and then successively lower animals. At the top of the Earthly beings are humans. So if we want to understand other organisms, according to this view, we can do so by comparing them to humans and seeing in what ways they are similar and in what ways they are different and lacking. Much of early comparative psychology was based on this idea (e.g., Bitterman 1960).

Other areas of psychology and other behavioral sciences have not been immune from the logic of the Great Chain of Being, except that, in some cases, they viewed different cultures or races of people as occupying different positions on the Great Chain (Sternberg 2004; Sternberg et al. 2005). Many eminent behavioral scientists, such as Sir Francis Galton and Raymond Cattell, believed in some version of the Great Chain (see <https://www.splcenter.org/fighting-hate/extremist-files/individual/raymond-cattell>). Moreover, traditional cross-cultural psychological studies of intelligence involved (and still involve) translating Euro-centric intelligence tests, such as the Wechsler, and then administering them to people in other cultures (e.g., Georgas et al. 2003).

But in the field of cultural studies of intelligence, progress has been made, largely due to the pioneering work of Luria (1976). Luria, in testing individuals in non-European cultures, found that the problems that were alleged to measure intelligence in European populations did not do so in other cultures because the individuals did not accept the presuppositions of the problems they were given. For example, when Uzbekistan peasants were given a syllogisms problem, such as, “There are no camels in Germany. The city of B. is in Germany. Are there camels there or not?”, subjects could repeat the problem precisely and then answer “I don't know. I've never seen German villages ...” The subjects did not accept the problems in the abstract modality for which they were intended. Of course, one could argue that they could not do so. But then, Cole et al. (1971) found that Kpelle tribesmen seemed not to be able to sort items categorically but rather sorted only functionally, until they were told to sort the way a stupid person would, at which point they had no trouble sorting categorically. In our own research (see Sternberg 2004), we found that rural Kenyan children and rural Alaskan Yup'ik Eskimo children could do tasks that were extremely important for adaptation and even survival in their own cultures (e.g., treating malaria with natural herbal medicines, finding their way across the frozen tundra from one village to another with no obvious landmarks) that their White teachers never could do, but were considered stupid by their teachers because they underperformed in school and on standard Euro-centric cognitive tests. Who was lacking intelligence: the children or the psychologists who gave them tests inappropriate to the demands of their everyday adaptation?

The tests we used for the Kenyan and Alaskan children cut to the heart of what intelligence is – ability to adapt to the environment. That is the core of intelligence, according to surveys of experts in the field of intelligence (“Intelligence and Its Measurement” 1921; Sternberg & Detterman 1986). But the tests that Burkart and her colleagues have devised are not tests highly relevant to animal adaptation; at best, and even then questionably, they are tests of folk conceptions of what animal intelligence should be from a human viewpoint.

An appropriate way to look broadly at the intelligence of any organism is to look at how well it adapts to the range of