Cognitive Gadgets: A provocative but flawed manifesto.

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Cognitive Gadgets: A Provocative but Flawed Manifesto

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Review of *Cognitive gadgets: The cultural evolution of thinking*, by Cecilia Heyes (2018).

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Abstract. The argument against innatism at the heart of *Cognitive Gadgets* is provocative but premature, and is vitiated by dichotomous thinking, interpretive double standards, and evidence cherry-picking. I illustrate my criticism by addressing the heritability of imitation and mindreading, the relevance of twin studies, and the meaning of cross-cultural differences in theory of mind development. Reaching an integrative understanding of genetic inheritance, plasticity, and learning is a formidable task that demands a more nuanced evolutionary approach.

The provocative thesis of *Cognitive Gadgets* (Heyes, 2018) is that human abilities such as imitation, mindreading, and language—the traits that allow our species' extensive cultural transmission—are not adaptations produced by biological evolution or, as repeated throughout the book, "in our genes." Instead, these abilities are themselves "gadgets" that have been created and refined by cultural group selection. Although they give the illusion of innateness, they are taught to children through social practices, and learned with the support of enhanced domain-general mechanisms such as attention, social motivation, working memory, and—most importantly—associative learning. Except for potentiating these general-purpose cognitive tools, genetic evolution has had virtually no role in shaping the distinctive traits that define human nature.

Why evolution should have followed this route in our species is a mystery, and Heyes does not offer any rationale or theoretical model to make sense of it. In fact, she stresses that genetic evolution *could* have played a role—the evidence from cognitive science just happens to say otherwise. The first question, then, is whether the book makes a compelling empirical case for its almost-blank-slate argument. *Cognitive Gadgets* presents a wealth of interesting findings and useful criticism of previous research; but as a refutation of innatism I found it surprisingly weak. Consider Heyes' treatment of genetic assimilation. In a nutshell, genetic assimilation occurs when traits that initially develop through learning (or other types of plasticity) get increasingly under genetic control, as selection favors variants that make the learning process faster and more reliable. In principle, assimilation can proceed so far that the trait develops entirely under genetic guidance, with no environmental input.

Heyes claims that she found no evidence of genetic assimilation for abilities like imitation and mindreading. Granting the premise for now, Heyes assumes that the heritability of a trait estimated from twin studies is an indicator of whether the trait develops with minimal environmental input ("poverty of the stimulus," high heritability) or with considerable input from the social environment ("wealth of the stimulus," low heritability). For imitation, the book cites one study of 2-year-olds by McEwen et al. (2007) as showing that "identical twins are no more alike in their imitative ability than fraternal twins." But this is not what the study found. The correlation was significantly higher in identical twins, and the authors estimated the heritability of imitation at 30%. This figure is well within expectations: the heritability of cognitive traits is small in infancy, but increases to about 30-40% in childhood and reaches 50-60% by late adolescence (Briley & Tucker-Drob, 2017). Heyes fails to cite another study of imitation in 2-year-olds (Fenstermacher & Saudino, 2007), which also found a higher correlation in identical twins and estimated heritability at 45%. For mindreading, Heyes cites one study by Hughes et al. (2005), which found the same correlation between identical and fraternal twins, indicating negligible genetic influences on individual differences in children's theory of mind. She omits to mention that, while the authors found no specific genetic contributions to theory of mind, there was a significant influence of nonspecific genetic factors shared with verbal ability (accounting for about 15% of variance). Other twin studies of theory of mind in children and adults have found heritabilities in the 15%-35% range (McEwen et al., 2007; Ronald et al., 2006; Warrier et al., 2018). Thus, contrary to Heyes' claim, both imitation and mindreading skills show a non-trivial proportion of genetic variance. Moreover, the apparent heritability of mindreading is most likely deflated by the rather noisy measures employed in these studies.

A deeper question is whether twin correlations and heritabilities are germane to the book's argument. In contrast with Heyes' assumptions, the proportion of genetic versus environmental variance says very little about the nature of environmental inputs and the trait's history of genetic assimilation. Consider a genetically assimilated trait that has become fixed in a population, and shows little or no genetic variation among individuals. By necessity, most of the variance of such a trait would be environmental. Or consider a hypothetical developmental process in which an environmental variable triggers the expression of alternative, genetically specified behaviors that are the same for all the individuals in a species. The resulting trait would show low heritability and high environmental variance; but the role of the "stimulus" would be limited to selecting from a menu of pre-specified alternatives. To further complicate things, nonshared environmental variance in a trait may reflect random events and insults (e.g., infections) rather than learning or organized plasticity; and genetic variance may capture the effects of deleterious mutations besides those of functional alleles. In general, the factors that drive the development of a trait may not be the same factors that produce individual differences in that trait. Moreover, a particular skill can be both evolutionarily novel and socially learned, but depend for its acquisition on traits that show substantial genetic variation. To illustrate: playing chess is a cultural gadget if there ever was one, and yet interest and aptitude for chess are about 40-50% heritable (de Moor et al., 2013; Olson et al., 2001; Vinkhuyzen et al., 2009). By Heyes' criteria, one should conclude that playing chess is more likely to be a "cognitive instinct" than imitation or mindreading. In sum, the book's argument for rejecting genetic assimilation is conceptually flawed and supported with cherry-picked data.

To remain on the topic of mindreading, Heyes cites interesting cross-cultural evidence that the stages of theory of mind acquisition differ between individualistic countries like the United States and Australia and collectivistic countries like China and Iran. But these findings are damning only if one holds an inflexible model in which the various components of mindreading (Schaafsma et al., 2015) can only interact in one pre-specified way, with no meaningful input from the social environment. Of note, the observed sequence changes typically involve two particular tasks out of five ("diverse beliefs" and "knowledge access;" see Duh et al., 2016; Kuntoro et al., 2017; Shahaeian et al., 2011, 2014; for a puzzling exception see Dixson et al., 2018). The overall picture, then, is one of patterned variation on a background of stability. Heyes also cites evidence that theory of mind development is markedly delayed in Samoan children (Mayer & Träuble, 2013). However, this literature contains several inconsistent findings that cannot be explained by cultural differences (see Liu et al., 2008; Mayer & Träuble, 2015). Some apparent delays may reflect culture-specific issues with task demands, as Mayer and Träuble (2015) noted in their follow-up to the original Samoan study. At the same time, theory of mind skills are not independent from other cognitive traits, and are significantly associated with IQ (e.g., Baker et al., 2014; Rajkumar et al., 2008). It may be impossible to fully make sense of the cross-cultural data on developmental trajectories without addressing the thorny issue of national differences in cognitive ability (e.g., Rindermann, 2018).

These examples serve to illustrate a double standard that is applied throughout the book: whenever the data do not support a rigidly “preformist” view of development, they are implicitly or explicitly counted as positive evidence for an associative account. But in several of the examples discussed in *Cognitve Gadgets*, associative learning is little more than a hypothetical mechanism (or a plausible contributing factor), and it is unclear if the models proposed by Heyes are able to explain the totality of the evidence. Moreover, the apparent simplicity of associative accounts often hides a lot of complexity (and inefficiency), which is only revealed by careful unpacking (e.g., Dickinson, 2012; Hanus, 2016). For all these reasons, Heyes’ rejection of innateness in favor of almost-blank-slate associationism seems highly premature.

I will not discuss the book’s case for cultural group selection in any detail, except to note that the argument is fully—and admittedly—speculative. To be clear, I see nothing wrong with bold speculation; but there is some irony in the sudden shift away from the hard-nosed empiricism of the rest of the book, precisely at the point where Heyes needs to explain *how* all the distinctive content of human nature can be outsourced to culture-mediated learning. For example, it is unclear if the selection process envisioned in the book could provide enough robustness and reliability to enable adaptive evolution; if it could work on a realistic timescale, given the long “life cycle” of groups compared with that of individuals; how it would respond to conflicts of interests between different social actors, and between group and individual fitness; and how it would prevent genetic adaptation from catching up with cultural transmission.

Even though my review of *Cognitive Gadgets* is critical, I strongly recommend the book to other evolutionary-developmental psychologists. It will stimulate them, challenge them to think more deeply about their assumptions, and prompt the field to open the developmental “black box” and become more explicit about computational processes. I see a clear parallel with much recent work in artificial intelligence (including neural networks), which shares the book’s empiricist attitude and faith in the power of domain-general learning (Marcus, 2018; see also Lake et al., 2017). This new wave of research is a fantastic opportunity for evolutionary-developmental psychology. Understanding how learning is instantiated in the mind/brain, guided by evolved developmental programs, and integrated with innate information is a daunting task, which has been made even harder by a scarcity of explicit models (Frankenhuis & Tiokhin, 2018). Computational tools like reinforcement learning can help understand what (and how much) pre-existing information is needed to perform efficiently and reliably in the real world (Frankenhuis et al., 2018), and how evolved developmental programs may respond to novelties in the environment, from optical mirrors to online interactions.

These questions can be approached in a spirit of synergy and integration (e.g., Frankenhuis et al., 2018; Lake et al., 2017; Versace et al., 2019), or—less productively—as a zero-sum competition between genetic inheritance and learning. Back to *Cognitive Gadgets*, it is unfortunate that Heyes sets up her main argument as a dichotomy between two extremes. Psychological mechanisms are either genetically encoded, domain-specific “instincts” that develop with minimal environmental input; or culturally transmitted “gadgets” that are learned through domain-general processes, with minimal or no contribution from genetic factors. The only middle-ground option entertained in the book—and quickly dismissed—is genetic assimilation (see above). This black-and-white contrast leaves out a world of more plausible possibilities. For example, psychological mechanisms may reliably develop a basic level of functionality with minimal input, but depend on learning (often directed and canalized) in order to reach full competence. While basic preferences for sweet versus bitter flavors are present at birth, food preferences are expanded and fine-tuned through years of intensive but non-random learning, which yields cultural similarities as well as differences (Rozin, 1990a, 1990b). Furthermore, even established preferences for or against certain foods can be adaptively overturned by conditions such as pregnancy and nutrient deficiency (Berthoud, 2011; Flaxman & Sherman, 2000; Rozin, 1990b). By tuning their operating parameters, general processes such as associative sensory-motor learning can be canalized to reliably yield specific, adaptive outcomes. My colleagues and I have proposed such a canalization hypothesis for the development of mirror neurons (Del Giudice et al., 2009). Also, distinct mechanisms specialized for different tasks may reuse some basic information-processing algorithms—for example reinforcement learning—while adapting them to the particular nature of each task. Modularity, functional specialization, and the difficulty of distinguishing between domain-general and domain-specific processes have been addressed in considerable depth in the work by Clark Barrett and colleagues (e.g., Barrett, 2012, 2015, 2017; Barrett et al., 2016), which reconciles the notion of specialized adaptations with a sophisticated view of learning and plasticity. A powerful idea stemming from this approach is that cognitive mechanisms may develop hierarchically, through "module spawning" and progressive specialization induced by different categories of inputs (Barrett, 2012, 2015). Heyes never considers these possibilities, which have been discussed for years in mainstream evolutionary psychology (e.g., Buss, 2015). It remains to be seen if *Cognitive Gadgets* will herald a genuine paradigm change, or succeed mainly as a timely provocation.

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