Received: 9 January 2019 | Accepted: 6 February 2019

DOI: 10.1111/mila.12251

ORIGINAL ARTICLE

WILEY

# Cognitive instincts versus cognitive gadgets: A fallacy

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**Abstract**

The main thesis of Heyes' book is that all of the domain-specific learning mechanisms that make the human mind so different from the minds of other animals are culturally created and culturally acquired *gadgets*. The only innate differences are some motivational tweaks, enhanced capacities for associative learning, and enhanced executive function abilities. But Heyes' argument depends on contrasting cognitive *gadgets* with cognitive *instincts*, which are said to be innately specified. This ignores what has for some years been the mainstream nativist/anti-empiricist view, which commits only to partially specified learning systems that become elaborated and built through domain-specific learning.

**KEYWORDS**

culture, empiricism, imitation, learning, mindreading, nativism

## 1 | GRIST VERSUS MILL

Heyes' (2018)[1] is built around the distinction between cognitive *grist* and cognitive *mills*. *Grist* comprises behavior, items of information that an individual acquires over a lifetime, as well as specific skills, like the ability to tie one's shoelaces. The mind's *mills*, on the other hand, are the cognitive systems that enable one to acquire those skills, as well as to acquire and process information. Heyes' focus is on mills. She wants to know the origins of the learning, reasoning, and skill-acquisition systems that mark us off from other animals, in particular. Her overarching thesis is that all such systems are culturally constructed and culturally acquired cognitive *gadgets*. The parade example of such a gadget is reading, a cultural invention that only took hold a few thousand years ago, but is now an indispensable information-acquisition device. Another example might be capacities for exact number, together with culturally constructed principles for exact mathematical reasoning.

[1] All references to Heyes' work are to this book unless otherwise indicated.

  

It is not controversial that reading is a cognitive gadget, of course. But it provides the model for Heyes' handling of a number of human-specific learning capacities that have been the focus of a good deal of recent debate. These include capacities for selective social learning, imitation, mindreading, and language, all of which are said by Heyes to resemble reading in their emergence, both across historical time as cultural inventions, and within the life-time of an individual. Our focus in the discussion that follows will be on imitation and mindreading, in particular. But we should note at the outset that there is an important point of disanalogy between reading and exact number, on the one hand, and the other four capacities listed above. Learning to read is generally laborious, and takes many hours of explicit instruction and feedback over a number of years. The same is true of acquiring exact number concepts (Sarneka, 2015). In contrast, children spontaneously start to be attracted to prestigious people, to imitate, to attribute mental states to others, and to talk. They do so early, easily, and in the absence of explicit instruction. This is an important initial cue, we think, that something has gone badly wrong with Heyes' account.

The importance of culture for understanding human cognition is not new, of course. It has been emphasized also by cultural evolutionists like Boyd and Richerson (1988) and Henrich (2015). But these theorists are not strongly empiricist about the mind, in the way that Heyes is. Rather, they think that the distinctive "mills" that mark us off from other species emerged through a process of gene-culture co-evolution. While being specialized for cultural learning, they now have a significant innate component. One criticism that Heyes makes of this view is that human cognitive mechanisms are tracking cultural targets that are moving too fast for genetic evolution (p. 208). But it is "grist" that moves fast. Only since the invention of agriculture has there been fast development of new cognitive "mills" (exact number systems; writing and reading; probability-theory; scientific method; and so on). With respect to the "mills" that she deals with in her book (learning biases, imitation, mindreading, and language) there has been stasis for a very long time, at least in the learning-problem abstractly described. Although what one needs to learn from one's culture is continually shifting, copying the prestigious remains a good strategy; although the actions that need to be imitated change, a system that fast-maps vision and audition to action remains useful; although people's particular mental states change a lot, they still have beliefs, preferences, and intentions that one needs to learn about; and although languages vary a great deal in their properties, there might still be some features in common, advance knowledge of which would ease the learning process.

On Heyes' view, the innate cognitive differences between humans and chimpanzees are much more minimal than those postulated by cultural evolutionists like Henrich (2015). They comprise just some motivational tweaks (resulting from a sort of self-domestication that took place when increasingly cooperative humans started to prefer prosocial humans to cooperate and mate with), some attentional biases (to attend to social stimuli, including faces and voices in particular), enhanced associative learning abilities, together with enhanced executive function. On Heyes' account, humans learn so quickly about social phenomena because they are deeply interested in social phenomena, not because they have innate mindreading systems or innate language-acquisition devices. And they learn more deeply about these (and other) things because their general-learning capacities have been significantly ramped up in comparison with other primates.

While we are sympathetic toward the view that the genetic endowments that differentiate human cognition from that of chimpanzees are small (with most of the vast gulf that separates the two species resulting from cultural evolution and cultural learning), we disagree with Heyes' specific views. In part this is because we disagree with her characterization of *animal* minds as domain-general associative-learning engines. Rather, we are inclined to follow Gallistel (1990) and others in thinking that animal minds comprise multiple specializations and domain-specific learning mechanisms

(Carruthers, 2006), some of which may have been enhanced and extended (rather than built de novo) in the course of human evolution. But this will not be the main focus of our commentary.

## 2 | COGNITIVE INSTINCTS, COGNITIVE GADGETS, AND THE SPACE BETWEEN

Heyes' argument in support of her cognitive-gadgets account of the distinctively-human mind takes the form of an inference to the best explanation over the alternatives. Chief among these is the view that cognitive "mills" like language and mindreading are cognitive *instincts* that are either richly or fully innately specified. Given this contrast, Heyes argues that whenever we find evidence that the outcome of development is experience-dependent in a dose-sensitive way, then we have evidence supporting her cognitive-gadgets account. This strategy is laid out in chapter 2, and is then followed in connection with the particular cognitive gadgets that provide her focus, namely selective social learning, imitation, mindreading, and language, in chapters 5 through 8.

This general strategy of argument fails, however, because the contrast between cognitive instincts and cognitive gadgets by no means exhausts the theoretical options available. In fact, there is significant space between the two. One can claim that human-specific cognitive "mills" are built during learning and development, not using general-learning mechanisms, but rather through the operations of a set of thinly specified domain-specific learning systems that have been adapted for the purpose. Take the case of language, for example. Few anti-empiricists today think that the starting-state for language learning is a fully specified universal grammar representing all possible grammatical languages. Rather, there is a smaller set of conceptual primitives and priors (or expectations of what a language should look like), perhaps together with a computational principle like Merge, which bootstrap and speed the learning process (Lidz & Gagliardi, 2015). Given such a picture, one would *expect* the outcome of language-learning to depend upon the richness of the input in a dose-dependent way. Hence finding that it is so dependent cannot discriminate between Heyes' cognitive-gadgets account and one that postulates a thinly specified domain-specific learning-mechanism.

The obvious fallaciousness of Heyes' argumentative strategy is remarkable, because the kind of theory she overlooks is by no means novel or underrepresented in the literature. On the contrary, it has been central to main-stream anti-empiricist theorizing for many years (Carruthers, Laurence & Stich, 2005, 2006, 2007; Laurence & Margolis, 2015). It has long been recognized that the central debate about the origins of the mind's "mills" is not really about whether they are fully specified innately, on the one hand, or learned, on the other. (Everyone has always allowed that most of the mind's "grist" is acquired through learning, of course.) Rather, it is about the starting-state of development and the nature of the systems that enable learning. Empiricists maintain (as does Heyes) that the starting state comprises only general-learning mechanisms (perhaps together with some innate biases in affective systems). Anti-empiricists think that, in addition to domain-general statistical learning mechanisms, the starting state for development includes a suite of innately structured domain-specific learning systems, designed to build the mature "mills" of the human mind (language, mindreading, and the rest) through learning over the course of development. Anti-empiricists can then differ from one another in how richly structured they take the domain-specific mechanisms to be. This set of views is simply not addressed in Heyes' book, and remains untouched by her main line of argument.

While the overall structure of Heyes' argument is clearly fallacious (not just logically, but as a purported inference to the best explanation), it may still be the case that what she says about some or all of the particular cognitive gadgets she postulates is correct. We will examine her account of just

two of these abilities: imitation (in Section 3) and mindreading (in Section 4), leaving language and selective social learning for others to address.

## 3 | IMITATION

Exact imitation is an important cognitive "mill," enabling people to acquire skills that others have developed, and giving rise to cultural lineages in such things as speech, dance, artifact construction, and much more. Heyes argues that imitation is a cognitive gadget rather than an innate adaptation, and is constructed in development via associative "Hebbian" learning ("neurons that fire together wire together"). The process begins in infancy whenever the infant moves either its hands or its feet into its field of vision, enabling visual representations of body-movement to "wire together" with the motor representations that generated those movements. All that is required is domain-general associative learning, Heyes thinks.

There are two obvious problems with this account, both of which Heyes attempts to address in the same set of replies. The first arises from the fact that animals are good associative learners. For in that case, why is it that most animals do not engage in imitation? The second problem is that people can imitate actions that they cannot observe themselves perform. They can imitate other people's facial expressions, for example, as well as the movements people make with their arms above their heads or with their hands behind their backs. Heyes provides three lines of reply to this second difficulty. All involve cultural practices that insure that infants and children get experiences that other animals do not, thus at the same time responding to the first problem.

Heyes' first reply is that caregivers frequently imitate the infant's own spontaneous facial expressions in face-to-face encounters. If the infant protrudes his tongue, for example, and his mother immediately protrudes hers, then a visual representation of tongue protrusion will become associatively linked to the infant's own motor representation, enabling him later to reverse the process by copying his mother's tongue protrusions. Plainly this reply is fairly limited in scope, unless caregivers imitate the full range of behaviors that their infants and young children exhibit. It also makes a clear prediction regarding what one should observe in cultures where infants are carried on their mothers' backs all day, and in which there is much less face-to-face interaction with infants. Specifically, we should find that young children in such cultures are much less good at imitating the facial expressions of other people than are children in Western countries. Since we know of no data on the topic (and Heyes does not cite any) we simply note this prediction, and move on.

Heyes' second reply is that children can use mirrors to observe their own movements, including those that would otherwise remain invisible to them. They can use hand-mirrors to observe their own facial expressions, and they can use full-length mirrors to observe their own bodily postures and limb movements. Again there is a clear prediction made by this proposal. It is that children who grow up in traditional cultures that lack access to mirrors (the vast majority over the course of human history and pre-history) will be significantly weaker imitators than children raised in developed countries today. But again we are aware of no data on the topic, and again Heyes does not cite any.

Heyes' third reply appeals to coordinated cultural group-activities, like ritual dancing, soldiers marching in step, and so on. When participating in these activities one will observe others doing what one can feel oneself doing, and one can observe the very actions for which one is concurrently generating the motor instructions. The problem here, however, is to explain how people come to be able to participate in these activities in the first place. Unless they can *already* imitate the movements of other people, how could they ever get themselves into a situation where they are performing the same movements in time with others? Given that this third proposal appears to beg the question at issue,

that throws all the weight of imitation-acquisition onto the other two. The clear implication of this together with our earlier points, then, is that people in traditional cultures should be much worse at imitating others than are Westerners. Although we know of no direct evidence, we are doubtful of the truth of this prediction.

Heyes fails to discuss the now-extensive evidence of culture (in the sense of socially acquired information and patterns of behavior that persist across generations) found in numerous animal species (Whiten, Ayala, Feldman & Laland, 2017). Some of this behavior looks very much like imitation, with patterns of matching synchronous behavior across individuals. Perhaps much of this is not "true" imitation, of which a variety of different kinds can be distinguished (Byrne, 2009). But some seems quite close to the sort of imitation that forms Heyes' target, with an animal mapping to its own motor system a visual representation of behavior in others that it cannot observe in itself. Contagious yawning provides a good example, which has been demonstrated in dogs (Romero, Konno & Hasegawa, 2013), chimpanzees (Campbell & de Waal, 2011), and baboons (Palagi, Leone, Mancini & Ferrari, 2009), and seems to perform an affiliative function in all three species. The case of baboons is especially interesting, since they have three distinct types of yawn (teeth covered, teeth uncovered, and gums uncovered), and yet imitators reliably copy the type of yawn exhibited by the target—none of which can they observe in themselves.

Moreover, there is at least one carefully documented instance of infant chimpanzees copying the nut-cracking movements of an adult, pantomiming the adult's movements in the absence of a stone or anvil (Fuhrmann, Ravignani, Marshall-Pescini & Whiten, 2014). Even more striking, a large recent study of neonatal imitation of "lip-smacking" facial gestures in macaque monkeys found that individual variations in this ability predicted capacities to follow the gaze of another agent 7 months later (Simpson, Miller, Ferrari, Suomi & Paukner, 2016). When taken together with the yawning data, this suggests that innate differences in imitative capacities might play an important social role—capacities that could then have been greatly ramped up, rather than built de novo as a cultural gadget, in our own hyper-social species.

Is there positive evidence that human imitation is a cognitive gadget, nevertheless? Heyes emphasizes evidence (much of it collected by her own group) that mirroring behavior can easily be induced, reduced, and even reversed though temporally-contingent pairings of actions and perceptions. For instance, if people are repeatedly required to move their little finger in response to the sight of an index-finger movement, then they no longer automatically mirror the latter with muscle activity in the index finger. On the contrary, mirroring activity switches to the little finger.

Here, as elsewhere, Heyes contrasts her cognitive-gadget account with a cognitive-instinct view. She cites Meltzoff & Moore (Meltzoff & Moore, 1994, 1997) in the latter connection as claiming that people have a powerful genetically-inherited mechanism that can translate any motor instruction into a third-person-perspective representation of what the resulting action would look like, and vice versa. And she complains that such a view merely "black boxes" the capacity in question, without providing a computational account of its operations. In contrast, her own account has the advantage of providing a computational theory, utilizing known principles of general associative learning.

This is one of the numerous places where Heyes overlooks the possibility of an intermediate-strength theory. This would postulate an innate imitation-*learning* mechanism, which may start with some crude mappings between action and perception, but whose functioning needs to be trained by experience. Nor need the computational "innards" of this system be black-boxed. On the contrary, such an account could appeal to the very same general-learning computations that Heyes herself does. For notice that copying-and-repurposing is quite common in biological evolution (Barrett,

2012; Chakraborty & Jarvis, 2015; Kaas, 1989; Marcus, 2004). An adapted special-purpose mechanism can re-deploy computational processes that are utilized elsewhere for very different purposes.

Indeed, there are good indirect reasons for thinking that some version of this intermediate-strength theory is true. For imitation requires long-distance neural networks linking visual and auditory sensory cortices with motor cortex. And although synaptic connections can be developed with training, there is no reason to think that long-distance pathways can be. And in fact, imitation appears to utilize the same dorsal sensorimotor networks that are employed in visually-guided action and in the perception and control of speech, together with ventral networks that are used in visual and auditory recognition (Hickok, 2014; Hickok & Poeppel, 2007; Rauschecker, 2012). These networks link sensory regions of the brain with motor regions, and with control regions in ventrolateral prefrontal cortex. They appear to be homologous among primates, at least, although they are quantitatively greatly enhanced in humans, especially in prefrontal control regions (Rauschecker, 2018). So this may be one of those cases we mentioned in Section 1, where relatively small genetic changes enabled a novel capacity to emerge. Given its evident utility among cultural beings such as ourselves, it is reasonable to think that the resulting network amounts to an innate imitation-learning system that has been at least partly adapted for the purpose.

The existence of an innate cortical network that has been adapted in the human lineage to facilitate imitation leaves open the question of the starting-state of that system, of course. Do all perceptual-to-motor mappings enabled by the system need to be learned, as Heyes thinks? Or are some approximate mappings built in, needing to be fine-tuned but remaining open for flexible learning of novel associations across the life-span? One sort of evidence one might look for is facial imitation in neonates (Meltzoff & Moore, 1977). If imitation can occur in the absence of experience, then plainly empiricist accounts of the learning process must be mistaken. But the reliability of this evidence is a matter of dispute. Indeed, Heyes cites with delight a recent "decisive" study (p. 133) of "unprecedented power and rigor" (p. 128) showing that facial imitation is absent in neonates (Oostenbroek et al., 2016). However, Meltzoff et al. (2018) identify no less that 11 serious flaws in the experimental design of this study (which was never intended as a *test* of neonate imitation, in any case; Oostenbroek et al., 2018), chief among which are that the experimenters coded for imitation of actions that are not even in the neonate behavioral repertoire (like tongue clicks), and that they tested for no less than 11 distinct forms of imitation sequentially over a period of more than 10-min. Moreover, despite these flaws, Meltzoff and colleagues' re-analysis of the original data from the study found evidence of imitation of tongue-protrusion after all. When set alongside the data concerning facial imitation in infant macaques (Simpson et al., 2016), we think that the evidence of at least some limited innate imitation abilities is robust.

Especially striking evidence consistent with this conclusion is provided by Bruderer, Danielson, Kandhadai and Werker (2015). They tested 6-month-old infants' capacities to discriminate a non-native (Hindi) consonant contrast sound that they had never previously encountered. The tests took place under two conditions: with a pacifier that obstructed the infant's own tongue movements, or with a pacifier that did not. The capacity to discriminate the non-native contrast was present in the latter case, but not in the former. This suggests either a remarkably precise innate mapping from sound to motor abilities, or at least a capacity to generalize spontaneously on the basis of previous sound-to-motor experience.

We should stress, however, that even if neonates *do not* imitate, this would fail to rule out the existence of some set of innate perceptual-to-motor mappings. For all sorts of other factors might explain a null finding, ranging from neural maturation that is not experience-dependent to lack of motivation on the part of the infant. It would be by no means easy to discriminate between the

various possibilities experimentally. All we insist on here, however, is that Heyes' argument for a cognitive-gadget account of imitation is unsound, and that there are both experimental and general theoretical reasons to think that it is false.

## 4 | MINDREADING

Heyes claims that mindreading, too, is a cognitive gadget. Like reading, it is a cultural invention, and is acquired slowly and effortfully during childhood. She cites evidence of dose-dependent learning from experience in young children. (Note: the tasks appealed to here are verbal ones. We will consider Heyes' critique of the implicit-mindreading literature shortly.) She also cites evidence that the mindreading network is slow to mature, and that mindreading performance continues to improve even beyond adolescence. These finding are said to support her view that the capacity is acquired on the basis of experience using general-learning mechanisms alone. But she makes no attempt to explain *how* this can happen: how does one get from a state in which one can represent bodily movements, but lacks any mentalistic concepts, to a position in which unobservable mental states are richly represented and reasoned about? For unlike the case of reading, no formal or informal teaching takes place. Admittedly, people talk about mental states quite a bit. But if associative learning is one's only cognitive resource, how could one learn to map the sounds that one hears onto the concepts around which mental models of the mind are constructed?

Here as elsewhere Heyes contrasts her view with an innate-instincts account, according to which mindreading development is merely a matter of maturation, issuing in the same terminal state across a very broad range of developmental environments (p. 146). (She also rejects child-as-scientist accounts, but in this case without argument; presumably because they postulate innate learning mechanisms beyond mere associative learning.) But no one in the field actually denies that learning takes place. And at least one published view (which claimed to articulate the assumptions underlying the burgeoning implicit-mindreading literature) suggests that there is an innately structured domain-specific learning system, which is designed to build the mature system in response to both direct experience and cultural input (Carruthers, 2013). The starting state of the system might comprise a handful of conceptual primitives such as THINKS, WANTS, INTENDS, and SEES, together with a small number of initial attribution rules (e.g., "When something happens in someone's line of sight, they see it"; "When someone sees something, they come to think it"; and so on.) On that basis the system gradually adds new concepts and attribution rules in response to experience, included in which is people's talk about the mind.

An innate learning-system account can of course accommodate the evidence of learning that Heyes appeals to in support of her cognitive-gadgets view. It can explain why children's performance on verbal mindreading tasks should be a function of the kinds of verbal input they have received from their mothers, or from the surrounding culture. It is consistent with the finding that mindreading abilities continue to improve past adolescence. (Who ever said that mindreading should be easy? On the contrary, given the evident complexity of the human mind, one might expect that mindreading would be hard to master, even with the head-start provided by a domain-specific learning mechanism specialized for the purpose.) And it is consistent with the variations that we find in folk psychologies across cultures. For much of the input that the learning-system is designed to accommodate will comprise the diverse verbal practices of mentalizing description and explanation that develop within each culture.

As Heyes recognizes, the evidence of mindreading in infancy, with representation of false beliefs seemingly being present from 6 months of age (Southgate & Vernetti, 2014), causes a big problem

for her cognitive-gadgets account. Her strategy is to deny the probative value of the data. She claims that in every case the behavioral data can be explained by appeal to domain-general processes of some sort that do not involve mindreading, such as learned associations among low-level properties of the stimuli, distraction or forgetting at crucial moments in the test display, and so on. This is a strategy that can only be applied piecemeal, of course. She needs to study each experimental set-up after the fact and locate some property that might have biased the infants' behavior in the observed direction. Even supposing that she can succeed in every case, the result, as we now show, is a *worse* explanation than the mindreading-based one, not a better one. This ought to be troubling for anyone who (like Heyes herself) wants to support a cognitive-gadget account on the basis of an inference to the best explanation.

There are now well over 30 studies that provide evidence of false-belief understanding in infants and young children, using a variety of materials and methods, and coming out of a number of different labs (Scott & Baillargeon, 2017). Admittedly, there have been some failures to replicate individual findings (e.g., Dörrenberg, Rakoczy & Liszkowski, 2018; Kammermeier & Paulus, 2018). But Baillargeon, Buttelmann and Southgate (2018) point out the methodological weaknesses of many of these attempted replications, while also acknowledging that some methods (specifically anticipatory looking) might not be reliable. Yet in the meantime, new studies both replicating and extending previous findings continue to be published (Király, Oláh, Csibra & Kovács, 2018). And in any case, these disputes are about the reality of infants' representations of false beliefs. There are even more studies of infants' understanding of goals, preferences, and perceptual access. So taken all together, there are a *lot* of data that Heyes needs to explain away.

It is possible that all of these studies include a confound of some sort. But certainly they cannot all include the *same* confound. So Heyes would be forced to proceed piecemeal and post hoc. (She actually makes no attempt to consider the totality of the evidence.) This is already a severe weakness. One thing a good explanation should do is *unify* the data; another is that it should be *fruitful*, opening up new lines of research and leading to new discoveries. Neither is true of Heyes' deflationary approach. Both are true of the innate learning-system account.

Heyes may reply that at least her account is *simpler*, in that it only appeals to forms of associative learning that everyone acknowledges to exist. But in the first place, we do not really know whether the associative-learning account is simpler until we have been told how to get from mere observations of movement to representations of people's mental states. And in the second place, it is not clear that appeals to simplicity cut much ice in the biological domain, where one's background expectation should be for messy complexity and specialization (West-Eberhard, 2003). Indeed, this is one of the places where it becomes important how one should conceive of animal (that is, ancestral) minds: whether as mere general-purpose associative engines, as Heyes believes, or as collections of special-purpose systems, as we maintain.

Heyes seems to recognize the importance of the ancestral state, and specifically the mindreading capacities of other primates. For if they can represent others' minds, then it will become quite implausible to claim that the human capacity to represent mental states is a cultural gadget. In this connection she mentions her previously-published criticism of Krupenye, Kano, Hirata, Call and Tomasello (2016), who had claimed to find evidence of false-belief understanding in apes (Heyes, 2017). She had proposed that the apes' behavior may have been driven by an association between location and the color of the protagonist's shirt, rather than by a representation of the protagonist's false belief. She does not mention, however, that although they thought her proposal quite unlikely, Kano, Krupenye, Hirata, Call and Tomasello (2017) took up Heyes' challenge and ran a control experiment,

with null effects. And of course here (as in the infancy domain) false-belief is just one kind of mental state that we have evidence apes can represent, in any case.

Heyes' final argument attempting to undermine implicit mindreading (this time in adults) is to show that while people automatically encode what an avatar in an image can see (one dot or two), they will likewise automatically encode what an arrow is pointing at. She takes this to show that the effect is driven by low-level associative processes, rather than representations of mental states. But arrows are symbols endowed with meaning. An arrow generally expresses someone's *intention* that you should attend in the indicated direction. So a mindreading-based explanation of the finding is equally possible, as Heyes herself notes (p. 267–268). Moreover, in a related paradigm, Terrizzi and Beier (2016) have shown that "automatic" attention orienting is influenced by whether one believes (tacitly) that the target figure is an agent or not. If the figure interacts contingently with another agent prior to the orienting trials, attention-shifts result; whereas if the figure goes through exactly the same sequence of movements and sounds, but *not* while interacting contingently with another agent, it does not.

Overall, then, we think that Heyes' case for believing that mindreading is a cognitive gadget is weak. And it depends on just the same fallacious exclusive contrast with a cognitive-instinct account that permeates the rest of the book.

## 5 | CONCLUSION

Heyes' book is a missed opportunity. She could have tried to build her case for her cognitive-gadgets account of the learning mechanisms that enable the acquisition and transmission of culture by contrasting that account with the strongest available opposition. Instead, she chose to focus on her weakest set of opponents: those who think that culture-acquisition mechanisms are fully innately specified cognitive instincts. No doubt there are real proponents of such views. But in science, and in intellectual inquiry generally, theories can only be fully evaluated by being pitted against their most plausible opponents. None of her arguments challenge the view that human culture is undergirded by a set of domain-specific and weakly innately-specified learning mechanisms. As with the construction of real mills for grinding wheat, the "mills" of the mind may need to be built using specialized sets of tools, not general-purpose learning.

## ACKNOWLEDGEMENT

We are grateful to Gregory Currie for his comments on a penultimate version of this article.

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**How to cite this article:** Roige A, Carruthers P. Cognitive instincts versus cognitive gadgets: A fallacy. *Mind Lang*. 2019;1–11. https://doi.org/10.1111/mila.12251