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LONG ARTICLE

The Evolved Apprentice Model: Scope and Limits

Kim Sterelny

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Abstract Downes, Gerrans, and Sutton all raise important issues for the account of human social learning and cooperation developed in *The Evolved Apprentice*. Downes suggests that I have bought too uncritically into the view that hunting was economically critical to forager life; I remain unpersuaded, while conceding something to the alternative view that hunting was signaling. Downes also suggests that I consider extending the evolved apprentice model to contemporary issues in social epistemology; I wonder whether that might make the model so general that it loses explanatory force. Gerrans probes the model on the relationship between social learning and imitation; I respond by arguing that imitation became important relatively late in the human social learning career, probably via learning to communicate via gesture. Sutton wonders whether the model of social learning developed is too intellectualist and individualist; I respond by emphasizing the varied task demands in different domains, and the change over time of the different elements involved in social learning.

Keywords Cooperation · Cultural inheritance · Extended mind · Human social evolution · Imitation · Social foraging · Social learning

Colloquium on Kim Sterelny's *The Evolved Apprentice: How Evolution Made Humans Unique*.

K. Sterelny (✉)
Philosophy and Tempo and Mode, Australian National University, Canberra, ACT, Australia
e-mail: kim.sterelny@anu.edu.au

Overture

Let me begin by thanking all three commentators for their exceptionally generous response to *The Evolved Apprentice* (2012c; hereafter, EA), and to Ben Fraser for organizing the workshop from which this colloquium has grown. Gratifyingly, all three are on board with the core idea of the book: the distinctive forms of human social life, and of the cognitive systems that support it, evolved incrementally, through coevolutionary feedback between informational, reproductive, and ecological cooperation. A central aspect of this process was our increasing tendency to shape our environments, not just to respond to them. In particular, hominins increasingly engineered the developmental environment of the next generation, thus scaffolding an increasingly rich, reliable, and high-fidelity flow of information across the generations. But while there is agreement on the core picture, the commentators press me on a series of important points. I shall begin by briefly summarizing these probes, and shall then respond to them individually in somewhat more detail.[1]

My account of the evolution of human social life is committed to the claim that social learning is rich, and of high fidelity. For example, I treat the onset of "behavioral modernity"—the phase in human prehistory when Pleistocene foragers began to resemble ethnographically known forager societies in technology, ecological breadth, and cultural specificity—as a period in which innovations became more frequent and more reliably transmitted (Nowell 2010; Sterelny 2011). Philip Gerrans (2013, this

[1] This article is one of four in *Biological Theory's* Colloquium on Kim Sterelny's *The Evolved Apprentice: How Evolution Made Humans Unique* (2012c). See also Downes (2013, this issue); Gerrans (2013, this issue) and Sutton (2013, this issue).

issue) presses me on the role of imitation in this model of social learning. This question is important, because humans are inveterate imitators, and great apes are (at best) barely competent imitators. Imitation was once thought to be a specific cognitive adaptation, and one requiring advanced theory of mind capacities (Byrne 1995; Tomasello 1999b). If that were right, this difference and its correlation with the cumulative character of hominin culture would be no surprise. If, on the other hand, imitation is just a special case of a much more general and ancient learning capacity (as both Gerrans and I suspect), it is much more challenging to explain this contrast between humans and great apes, and to give a positive and incremental account of the role of imitation in social learning.

Stephen Downes (2013, this issue) pushes me on two important issues. One is an important aspect of the coevolutionary picture: the role of hunting. I see hunting as a cooperative, utilitarian activity, generating rich resources, and hence generating selection pressure in favor of the technical, social, and psychological capacities needed to hunt and share in concert with others. It was economically important in supporting long periods of juvenile learning, and it was cognitively important in selecting for cognitive skills of cooperation, coordination, and planning. Downes reminds us that this view is extremely controversial. His point is both substantive and methodological. How well supported is this view of Pleistocene foraging? More generally, how is an account of this kind to be tested?

Second, Downes takes up the scope of the model. EA sees skill acquisition in ancient societies as hybrid learning. Agents learn by doing, but in a safer and enriched environment, with social support in the forms of skilled models, encouragement, advice, and explicit teaching. Downes wonders whether this model can be extended to important aspects of contemporary environments.

John Sutton (2013, this issue) probes the EA model on two fronts. First, he wonders whether my view of information sharing is too optimistic. I portray information sharing as leading to adaptive responses to environmental challenges, and argue that in many contexts, the dangers of deception and manipulation have been oversold. Sutton points out that deception is not the only problem: the literature of social psychology documents pathologies of agreement, as collective decision can be collective confirmation bias, as agents reinforce their shared prejudices and false starts. Perhaps the picture of adaptive response in EA depends on an overly hopeful picture of information sharing. Second, Sutton presses on the interaction between explicit, introspectable off-line cognitive capacities and implicit, opaque, online capacities. Sutton's own views are sophisticated, and he certainly agrees that explicit, individual representations of task domains and procedures play an important role in skilled action. Even so, he wonders whether the view developed in EA is somewhat too intellectualist and internalist, seeing advanced skills as depending rather more than they really do on wholly internal resources and on explicit meta-representation of the task domain. I turn now to discuss these challenges in a little more detail.

Imitation: Why Us

A decade or so ago it was argued that hominins evolved the capacity for cumulative culture, and the great apes did not, because hominins evolved the capacity to learn by imitation, thus making high-fidelity cultural transmission possible (Tomasello 1999a). Imitation learning is rare because it is cognitively sophisticated, requiring agents to represent an action sequence as structured and goal-oriented, and to solve a correspondence problem, mapping a visual input of another agents' actions onto the self's motor output. That view has attracted skeptical response (Laland and Galef 2009), and the EA model is supposed to explain high-fidelity transmission without supposing that the agents in question have sophisticated capacities to learn by imitation. For neither an apprentice plumber nor an apprentice lawyer learns by imitation; they learn through sequences of trial and error, in highly organized and socially supported environments, with each new skill becoming a platform for further advance. But if imitation is not necessary for the social acquisition of demanding cognitive capacities, what explains our predilection for imitation learning? Why are great apes insensitive to opportunities to learn from skilled models? This challenge is especially confronting if, as Gerrans suggests, the machinery that makes imitation possible is an ancient feature of the primate clade: the mirror system, together with associative learning. Like me, Gerrans is impressed by Celia Heyes' (2005) associative model of imitation. But if Heyes is right, perhaps we should expect the great apes to be ready imitators.

Gerrans himself provides part of the answer. In many contexts, emulation learning is at least as efficient as imitation. Emulation is a form of social learning where the target of attention is the product of the action sequence, rather than the sequence itself. If a Pleistocene hominin were learning how to make a hand axe through emulation learning, he or she would be focused on the cobble, and the size and place from which chips were struck; not the speed and angles of the hands in motion. There are clear advantages in tracking the cobble. The cobble persists, so there are fewer demands on memory. That is especially helpful if models are making hand axes for their own purposes, rather than delivering a stylized, slowed-down training demo. Moreover (if the model is moderately cooperative), as well as having a second or third look at a strike scar, the cobble can be viewed from different angles, felt, and weighed in the hand. Finally, while the production sequence in tool making or extractive foraging constrains the motor sequence that generates the changes in the material substrate, it rarely specifies it uniquely. Different agents will carve a fishhook, grind ochre, or skin an animal in somewhat different ways. Their size, strength, dexterity, handedness, and personal idiosyncrasies will play some role in forming their technique. Trying to reproduce the exact motor sequence of a skilled agent may not be the best learning strategy. Knowing what you are aiming at, understanding the intermediate sequences through which you must go, perhaps with a very coarse-grained idea of how to get there, can guide trial-and-error learning efficiently. Recipes in cookbooks, for example, typically specify production sequences in the substrate, not motor patterns of the cook.

Imitation comes into its own when there is no substrate: when there is no physical world to guide choice and shape it through feedback (Heyes 2013). Social interactions are often guided by routines of acting and reacting that place agents in one another's social maps, and which display mutual knowledge of that placement. Sometimes these routines do not tolerate error. Precision is regularly required. For example, in dance and in other rituals, agents often coordinate or match one another's actions quite precisely. Some craft skills demand great precision in either production or execution, but error-intolerant social signaling probably preceded error-intolerant craftsmanship. While the craft skills of behavior modernity were impressive, they were probably not precision technologies. A few of these social signals are ritualized or transformed versions of utilitarian activity; a few others are constrained by their roles as costly signals (for example, mutual penis-holding as a sign of trust). But many are arbitrary. Form cannot be guessed from function. Famously, linguistic signals are arbitrary, too. There is independent reason to believe that language-like communicative capacities originally evolved as systems of gesture (Sterelny 2012b). If so, as with these other social signals, protolinguistic gesture could have been learned only by imitation (shaped and fine-tuned by trial and error) rather than emulation.

So great ape imitation skills are poor because selection for social learning (to the extent that such selection has shaped their learning capacities at all) would bias their attention to the products of action sequences, rather than those sequences themselves; for they never evolved the cooperative, collaborative, communication-hungry lives of (say) Middle Pleistocene hominins. Moreover, as Gerrans suggests, it is quite likely that efficient imitation learning depends on advanced executive capacities. In Sterelny (2012b), I argue that complex, transformative technical skills depend on internal representations of the target (Sutton demurs; we will come to that), and that this capacity is then exapted to power gestural communication about the elsewhere and the elsewhen. Thus a mime of a zebra bogged in a waterhole can be prompted by a mental representation of a zebra. Without this capacity, mime and gesture might be enough to coordinate a response to a zebra by a group lucky enough to stumble across one, but could not be used to solicit zebra catching from a band otherwise engaged. Great apes make tools, but not through complex transformations. So their imitation skills might be limited both by the fact that action sequences are less salient to them, and by the fact that they do not routinely form inner templates of the targets of action sequences that can then drive their execution.

Models and Meat

In EA I claim that reproductive, ecological, and informational capacities coevolve through positive feedback; for example, ecological cooperation generates the resources that supports long childhoods, powering extended social learning. Reproductive cooperation makes social learning more reliable by increasing the range of informational sources available to children. As Downes notes, the role of hunting on this view is highly controversial; he reads Hawkes as having an empiricists' skepticism ("show me the data") about coevolutionary models in general, and this view of hunting in particular. I am impressed by the general worry. EA builds a complex historical scenario tied (I claim) closely to the archaeological and paleoanthropological record, and appealing only to well-confirmed evolutionary and ecological mechanisms. But it is not supported by direct experiment, simulation, or formal theory. There is clearly a testability challenge to be met. That said, I am not persuaded that the picture is vulnerable to a critique based on Hawkes' challenge.

To begin, I see the empirical challenge somewhat differently. I think there is good and uncontested evidence that hunting generates enough food (and other resources like hide and tendon) to pay its way as an economic activity (Kaplan et al. 2005, 2009). Hawkes' challenge is based on distribution, not total value (Hawkes et al. 2010). As she and her collaborators see it, the successful hunter and his family do not get a disproportionate share of the kill, so hunting is not family provisioning. The hunter's rewards are individual and social. Those of the contrary view respond by seeing the distribution of the kill as insurance. Hunters provision their families indirectly by sharing, for they thus ensure that their families will eat when they are unlucky. On this reading of the debate, the empirical issue turns on whether sharing is targeted and contingent: portions of the kill are given only to those that share in return.

The evidence on sharing and reciprocation is meager and contested: there are very few forager societies for which we have a reliable quantitative profile of food distribution. But the show-off model seems to make two problematic predictions. First, it predicts that only expert hunters should hunt and share: the inexpert merely reveal their mediocrity (or alternatively, depending on the form of the costly signal hypothesis, that hunting effort should vary remarkably and in concert with skill). Second, it predicts that the preferred targets should be dangerous or difficult, rather than those that are high value resources, for these most effectively advertise skill. While there are a few cultures with hunting practices that fit this template (for example, those with rite-of-passage rituals involving hunting large predators), these seem to be outliers. The paleoanthropological record seems to show that the ideal targets of Pleistocene hunters were prime adult herbivores; typically from medium or large species (Stiner 2002). This is most naturally seen as resource-driven hunting. Of course there is wriggle room; perhaps show-offs are chasing rich packages to attract a larger audience for their generosity. But the show-off hypothesis hardly leaps out of the data.

So I very much doubt that there is a clear empirical signature of hunting-as-signaling in either the ethnographic or paleoanthropological record, though I agree that hunting, like many other utilitarian activities (wearing clothes, making shelters, making artifacts) has often acquired secondary signaling functions, and that these occasionally become an activity's dominant purpose. I am much more persuaded by Hawkes' positive proposal: hominin life history has been profoundly influenced by reproductive cooperation, and especially by grandmothers supporting their adult daughters. But I continue to resist the view that this is more than one factor amongst several, and this returns me to the difficult issue of the role of formal models in testing evolutionary hypotheses. Downes cites a recent modeling defense of grandmothering (Kim et al. 2012). But that model illustrates both the strengths and limits of model-led evolutionary social sciences. The strength: the model supports the idea that relatively modest amounts of grandmotherly support can result in major life history changes (in this case, longer life spans). The limits: the model suppresses the very features of mid-Pleistocene hominin life that indicate that grandmothering evolves in concert with other factors. It suppresses encephalization, and hence the specific nutritional demands of brain growth. Large, fast-growing brains need fats and proteins, not just the carbohydrates that grandmothers can plausibly provide (Kennedy 2005). And it suppresses the changes in the biological and social environment needed to make grandmothering survivable and profitable. Lone older women need to be able to forage in reasonable safety, and to have secure possession of their finds, as they travel back to home base. Formal models are not well suited to represent complex causal hypotheses of the kind defended in EA, though they are certainly important in demonstrating the causal potency of the individual components of the picture. EA would certainly be more convincing if it included potency proofs of this kind about, say, the effects of simple information sharing and social learning on foraging efficiency. So I see an important but limited role for formal models.

So much for testing. As I mentioned above, Downes is also interested in the scope of the model. He wonders whether the hybrid view of learning developed in EA can be extended to information creation and transmission in contemporary environments, and especially to science. There is, after all, a striking passage in S.J. Gould's *Wonderful Life* that explicitly sees the supervisor–graduate student relationship as one of master and apprentice (Gould 1989, pp. 139–140). He suggests that the EA picture can enrich the project of construing science as a practice rather than as a body of theory. This is an attractive idea, and one that had never occurred to me, but on reflection, I doubt whether the EA model can be productively extended to the sciences. (1) In comparison to the transmission of artisan skills or even the social and moral norms of a community, the transmission of scientific expertise depends heavily on explicit and highly structured teaching. (2) While scientific expertise depends importantly on tacit know-how and technique, explicit theory plays a very central role. This explicit theory is coded in public representations: in depictions and diagrams; linguistically; and in alphanumeric form. These representations exist as lectures, textbooks, and monographs; journal articles and data bases. In important ways, science does not exist inside the heads of scientists. It is best seen as an organized collection of public representations available to, accessed by, and added to by individuals and research teams. (3) Finally, purpose-built information technology plays a critical role. Science would be impossible without its measuring instruments, systems for recording and reporting data, and its tools for processing quantitative information. It is true that these factors are only differences in degree from the paradigms discussed in EA. A child absorbing the norms of her community will experience some explicit teaching, will acquire some explicit and general representations of normative principles, and these will typically be encoded in public representational systems. Her learning trajectory will be aided by purpose-designed props; stories, for example, that are particularly vivid and memorable, that are part of the common currency of her community, and that illustrate unmistakably a feature of her normative environment. But the differences in degree are large. Modeling typically faces trade-offs between generality and richness of detail, and this is true of conceptual models, not just formal models. Making the EA model general enough to capture the sciences, the transmission of artifact skills in the Paleolithic, and the transmission of social tools in the early Holocene seems likely to rob it of explanatory content.

Information and Individualism

John Sutton presses the view developed in EA on two fronts. First: he wonders whether my view of informational cooperation is too optimistic. Sutton's worry is not with deception, nor with the reliable transmission of collective opinion to the next generation, but with pathologies of agreement and disagreement in the formation of collective opinion. He reminds us that there is a good deal of social psychology on information sharing and group decision making, and the news is not always good. It can result in a form of collective confirmation bias: entrenching initial biases and first guesses. The EA model sees the accumulation and transmission of cognitive capital as a collective activity, often leading to an adaptive response to environmental challenges. But perhaps this picture depends on an idealized, perhaps even romanticized view of band-level exploration and decision making.

The challenge is important, and my main response will be to emphasize the modesty of my claims about Pleistocene social epistemology. I do not propose that the mechanism of innovation, collective assessment, and transmission is foolproof; that it always or almost always results in well-crafted, informationally sensitive, well-adapted responses to environmental threat. I have discussed systematic limits on culturally mediated adaption in Sterelny (2007, 2012a). These papers point out that some problem domains resist socially mediated trial-and-error solutions. Folk medicine is terrible because the local pathogen load is extraordinarily labile, and because it is difficult to determine whether a trial is a success or a failure. Likewise, it is hard to filter norms and social conventions by trial and error coupled to collective assessment. Natural experiments in which otherwise similar groups differ just in one aspect of their normative commitments are few and far between. Even when they do exist, if normative differences between groups have effects, these will be felt at spatial and temporal scales unfriendly to individual observation and assessment. There were relevant changes in social context as well. As societies became more complex and differentiated in the Holocene, many social heuristics for tracking real expertise became less reliable.

So the exploration–innovation–transmission cycle is not always reliable. When it is reliable, it is reliable because of the differences between Pleistocene forager bands and experimental subject groups. As Sutton reminds us, experimental groups are typically transient, and composed of agents not known to each other. Forager bands (1) have a real stake in the process, for they live with the consequences of their decision making; (2) they know one another well, typically over many years; (3) they are drawn from egalitarian environments with prestige differences but without formal authority, so one would not expect Milgram-like effects on decision making; (4) in many of the core cases—the assessment of local technology, natural history, foraging options—they are all experts. Collective thinking is more like a lab group deciding on their next experiment than like a group of strangers on a broken-down bus. That said, it is important to remember that the cognitive profile of ethnographically known forager groups is a puzzling mix of genuine expertise and cognitive illusion. The same people who have rich, detailed, and accurate knowledge of their local history and environment also fear sorcery (and execute supposed sorcerers (Boehm 2012)). Some accept norms of kinship and purity that leave sexual resources in the control of aging gerontocracies (Keen 2006). Forager cognitive profiles mix insight and credulity, and the task is to explain both aspects of human cognition. That was probably not explicit enough in EA.

Sutton's second challenge raises related issues: on the interaction of explicit, introspectable, off-line cognitive capacities with tacit, introspectively opaque, online capacities. One strand of EA and related work sees the hominin cognitive trajectory as evolving an increasing role for explicit representation in the control of complex action. Sutton's own views on this issue are nuanced and subtle, and he certainly accords a role to both in explaining human cognitive competences. Even so, he suspects that the EA picture is too internalist and individualistic, pointing out that some of the most skilled human practices are tightly coupled to substrate and embedded in teamwork: think, for example, of a team of surgeons and specialists in an operating theater.

The idea that over the course of hominin evolution, skill is brought under internal control, and partially explicit, introspectable control, is important to the overall picture defended in EA. First, it is important to a rich version of the apprentice-learning model, one according a significant role to demonstration, error diagnosis, and correction, explicit teaching and to self-generated practice. These are part of the social-learning toolkit of forager life (though doubtless their importance varies greatly from case to case). Second, it is central to the account I develop of the evolution of communication. Language is not stimulus bound. We can speak about the elsewhere, and even about fictions and mere possibilities. I see language as beginning with gesture, and gesture becomes decoupled from the immediate environment once agents have evolved this capacity to drive a complex action sequence through an internal representation of a goal, and of the sequence that takes the agent to that goal.

That said, I can concede something to Sutton's greater emphasis on the coupling of skill to substrate and to social context. For one thing, Sutton is clearly right that agents can develop extraordinary levels of skill without being able to explicitly represent their own capacity; certainly, without a representation that enables them to teach effectively. Garfield Sobers, perhaps the most expert all-round cricketer in the history of the game, apparently lacked that capacity. Moreover, some skills have remained sub-doxastic. Most of us have little top-down representation of our capacities to speak or understand, or of our capacities to read emotion and intent from facial expression and posture. So the transition I identify is not uniform or universal; there is variation from agent to agent and capacity to capacity. But some skills have been brought partially under top-down control. I argued in EA that this transfer of control to internal representation began with transformative technologies, where an artisan constructs a tool from a range of distinct source materials, and hence cannot organize his or her actions by imaging or planning changes in a real object in perceptual focus. But even if tacit skills begin with these transformative technologies, this does not explain current variation. That remains an open project. Finally, it is also true that top-down representations are not always trustworthy. Most of us learned to read supported by explicit rules about the alphabet, spelling, and sounding out letters. But our mature competence depends heavily on word recognition and sentential context.

The bottom line, then, is that I agree that there is plenty of current and recent variation about the role of individual and internal resources in skilled practice. The EA claim is that in the deep history of skilled practice, this variation has expanded. Hominins evolved from a baseline in which they were skilled extractive foragers, but where those skills involved little conscious, top-down representation of their own extractive skills. Over time, skill became a more heterogeneous category. The acquisition toolkit expanded, and so did the cognitive foundations of mature competence. Amongst this increased variation, there came to be skills for which top-down representation played a critical role. My best guess is this began in the erectines of the mid Pleistocene (Nowell and White 2010). Sutton knows the cognitive psychology and neuroscience of skill far more deeply than do I, but my impression is that this pluralist view of skill and its cognitive basis is reasonably consistent with that literature.

Let me finish by summarizing what I take to be the main challenges posed by the commentaries. (1) Skill is not a unitary category, in either its acquisition or cognitive basis. What explains the variation, and how did that variation evolve? (2) Under what circumstances does information sharing and transmission support adaptive response to, or intervention on, hominin environments? (3) How are complex historical scenarios to be tested; in particular, what are the scope and limits of testing by formal simulation?

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