to appear in *The Complex Mind: An interdisciplinary approach*, McFarland, D., Stenning, K. and McGonigle-Chalmers, M. (Eds), Palgrave-Macmillan

Private Codes and Public Structures

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> ... with new paradigms we can create conditions enabling monkeys to view their cognitive procedures as externalized arrays, without requiring a prior lexical process ... (a collection of icons is formed at the base of the touchscreen contingent on icon selection). With these techniques, we are now in a position to evaluate whether a new cycle of causality might be created ... whereby cognitive systems are scaffolded to new heights of achievement, through externalization.
>
> -- McGonigle & Chalmers, 2006, p.263

> *There was a Macaca mulatta*
> *Who learned how to use a computer.*
> *With no need to use ink*
> *She was able to think*
> *And hence she became a lot smarter.*
>
> -- this author

Humans externalize cognition in myriad ways. Our tools, marks, trails, speech, writing, and dwellings pepper the landscape. The cognitive droppings of our ancestors go back over a million years. More recent cognitive achievements of our species are “scaffolded” (to repeat the term used by McGonigle and Chalmers in the quotation above) upon those earlier structures. The journey from tallies of grain to the Schrödinger equation was not inevitable, but it would have been impossible without externalized cognition. Our capacity to understand quantum mechanics, such as it is, depends on access to symbols that lie outside the head. And yet we barely understand the process by which such cognitive achievements are scaffolded. It is some part genomic, some part epigenetic, and entirely a product of complex developmental processes, “cycles of causality”, whose complex causal strands cannot be simply or linearly separated into genetic factors and environmental factors (Stotz & Allen, forthcoming).

My starting point in this paper is the pioneering work of Brendan McGonigle and Margaret Chalmers trying to unravel some of that complexity. As a philosopher of cognitive science with a special history in animal cognition, I am particularly interested in the longstanding debate between “associationists” and “cognitivists” about the best way to understand the cognition of the more sophisticated nonhuman animals, such as in transitive inference (Allen 2006). As such, I was intrigued by an email message from McGonigle (pers. comm., 2006) in which he wrote, “I don't hold out much scope for associative mechanisms on their own. ... Instead, I favour a ‘multiple types’ of learning approach which targets relationally based mechanisms as qualitatively different and more powerful than those derived from what the late Harvey Carr once described as ‘the educated salivations of a Russian dog’.” With their research on the seriation abilities of monkeys, and into how earlier training experiences supported the development of more sophisticated relational abilities, McGonigle & Chalmers challenge the canonical impulse to constrain scientific understanding of animal cognition within the limits of basic forms of associative learning. It is these relationally-based mechanisms – concerned with tracking higher-order and abstract relationships among stimuli – that they probed with the assiduous use of touchscreens.

In the the opening sentences of “The Growth of Cognitive Structure in Monkeys and Men”, McGonigle & Chalmers (2002, 287) put the importance of understanding development through life history like this:

> There is a widespread view that the sorts of animal learning mechanisms most frequently studied in the laboratory are inductively too weak and unproductive to generate the kinds of behaviours expressed in higher order forms of human cognitive and linguistic adaptation (Chomsky, 1980; Fodor & Pylyshyn, 1988; Piaget, 1971). One reason for this (Harlow, 1949) is that investigations are rarely followed through from one learning episode to another to assess the cumulative benefits (if any) as a function of the agent's task and life history.

The same theme reappears in their 2006 paper, where they claim that, as a result of the prevailing methodology, there is an "overdependence on a relatively weak inductive mechanism, rejected by cognitive and linguistic researchers alike as one that cannot scale up and deliver teachable cognitive or linguistic skills" and this has created, they say, "a conceptual vacuum in which language looms as a "magic bullet" invested with new capabilities of its own and putatively causal to the cognitive abilities unique to humans" (2006, 242).

Their stance against strict behaviorism on the one hand, and against language-centered accounts of cognition on the other, would appear to suit the many people already convinced by their reading of "cognitive ethology" that the gap between animal minds and human minds has long been exaggerated. Indeed, the implied critique of behavioristic methodology may seem like old news. But these same people are also likely to believe in the importance of studying animals in ecologically valid contexts. Here, however, McGonigle and Chalmers wield a double-edged sword, for on the reverse swing they argue (2006, 248) that:

> our learning-based assessments, which could be viewed from one perspective as exposing (i.e., bringing out) basic cognitive competences, are better viewed as bringing them on by use .... *it is only in a laboratory context* that these nurturing conditions can be provided in a principled way. Under natural conditions, by contrast, there is no guarantee that the ecology furnishes systematic challenges, let alone affords opportunities for supervised long-term learning of the sort we describe here, teaching that could match the "relentless" instruction accorded the child recipient of by adult caretakers" [emphasis added]

Thus they wade straight into the long and contentious history that divides comparative psychology from ethology, in which the situation of the animals that are the targets of investigation has always been at stake. Ethologists, steeped in a tradition of natural history and ecological validity, have often regarded laboratory animals as artifacts, bearing little resemblance to their uncaged counterparts. Psychologists, having learned the importance of controlled experimentation, have typically regarded wild animals as magnets for anthropomorphically over-interpreted anecdotes, and hardly the proper domain for rigorous scientific work.

My aim in this paper is not to reheat this long-simmering dispute. In actuality, McGonigle and Chalmers steer an interesting middle course of skepticism about both; questioning, on the one hand, the power of ethological investigation to answer essential questions about animal cognition (without necessarily implying that cognitive ethologists are wrong about what they think they see) and questioning, on the other hand, the tendency of many comparative psychologists to overgeneralize from the limitations of their experiments to limitations in the animals themselves. I mention their stance towards animals living under natural conditions because it once again points up the complex causality of development and the difficulties inherent in studying it.

Why might touch screens be an important tool for the comparative developmental psychologist? McGonigle & Chalmers mention the ability of touch screens to circumvent the limitations of monkeys due to the "serious manipulative restrictions imposed by their motor control systems" (2002, p.320). Although they don't use the jargon, the idea that manipulation skills play a significant role in scaffolding cognitive systems aligns with current interest in "embodied cognition" and "extended mind". Human intelligence is partly grounded in our ability to make precise and repeatable notches in materials that include wood, bone, stone, clay, and metal. Beyond the jargon lie important questions about the extent to which cognition that operates on external structures is not simply a reflection of pre-existing inner thought, but part of a set of feedback loops between brain, body, and environment that have complex combinatoric effects during development.

But if this perspective is correct, how are we to make sense of the further claim by McGonigle and Chalmers that cognitive meaning, both for language and non-linguistic forms of abstract cognition, is grounded in "private codes"? The claim is made in the context of a discussion of experiments they conducted within a Piagetian framework, investigating the ability of monkeys to sort objects by size. Instead of requiring the monkeys to physically manipulate the actual objects, as Piaget required of the children in his experiments, they allow the monkeys to indicate the correct order by sequentially touching different sized icons presented on a computer screen. McGonigle and Chalmers maintain that humans externalize these private codes in language but, "In simians, however, we conclude that these remain as private codes, until their externalization into a public domain is made possible though the vastly improved manipulation skills of humans" (2006, 243). The idea, I take it, is that because human dexterity allows us to build touch screens that monkeys can use, they too can finally externalize their private codes. Of course, one can reasonably go on to point out that even if monkeys can be scaffolded thus far, humans take yet another step of externalization by using their physically externalized codes on a larger, social scale as media for communication and social cooperation. One might well wonder about the potential for monkeys to do likewise. But I want to take a step in the other direction, towards asking what monkeys bring to the task of using a touch screen, and considering the sense in which the "private codes" being externalized on-screen would exist at all without the "supervised long-term learning" that is the hallmark of McGonigle & Chalmers' methodology. Can we say more about the "(nonarbitrary) relationships between physical objects" (2006, 243) that provides the "objective grounding" for these codes?

One of my objectives in this paper is to offer some ideas that might help clarify these questions. But I have a larger objective, too, which is to help promote a much more developmental approach to animal cognition, particularly to my philosopher-colleagues, but also among the comparative psychologists and cognitive ethologists whose work has captured so much more of the recent mindspace of public and academic awareness of animal cognition research than the more difficult path taken by McGonigle & Chalmers. So, before unpacking the notion of private code, a little context.

State of the Nation

We are in the midst of a boom of interest in the scientific study of animal cognition. Barely a week goes by without a new study, and accompanying coverage by science journalists, concerning attempts to show that animals can succeed at various "high-level" tasks such as self-recognition, imitation, deception, “theory of mind”, tool use, and referential communication. Studies of these capacities span a wide range of species, for example primates, cetaceans, dogs, elephants, parrots, and various members of the corvid family. These pursuits have something of the character of trophy hunting by scientists eager to show that their favorite species can (too!) do what another species can do.

The specific tasks investigated usually take human competency as the model to be emulated and it is common to see the cognitive capacities of animals likened to those of human children of various ages, as if to locate the animals with respect to particular benchmarks on the developmental trajectory from neonates to human adult cognitive competency. Although practically everyone acknowledges that these comparisons and the underlying picture of development are too simplistic, it is my view that few scientists study cognitive development in animals adequately. On the one hand, fewer than 5% of the 400+ articles published in the journal *Animal Cognition* since it was established in 1999 are about cognitive development. A considerable proportion of these “developmental” studies involve the attempt to establish that adult members of nonhuman species can succeed in cognitive tasks that are considered to be developmentally significant benchmarks in humans (for a defense of the approach see Parker 2002). But animals are not humans at an early stage of development, and my view, elaborated in the following sections, is that this approach does not encourage sufficient attention to the developmental and learning processes themselves. On the other hand, while many papers about psychological development in animals get published within the field of developmental psychobiology, nearly all the scientists doing this work operate within the framework of traditional animal learning theory, committed to forms of associationism that McGonigle & Chalmers sought to undermine. However, the skepticism of developmental psychobiologists about the meaning of cognitive concepts drives most of them to actively eschew cognitive vocabulary (see, e.g., Wasserman & Blumberg 2006).

In between these poles is a small group of developmentally savvy cognitive comparative psychologists, whose theoretical notions such as "emergents" (Rumbaugh et al., 1996; Rumbaugh, 2002) and "private codes" (McGonigle & Chalmers 2006) represent an attempt to close the gap between basic associationism that seems insufficient to explain some aspects of animal performance, and anthropocentric models of cognition that are implausibly applied to animals. But some critics might worry that the notion of a “code” smuggles back into comparative psychology a linguistically-inspired notion of representation that should be regarded with suspicion, or that the notion of an emergent labels the phenomenon of cognitive development rather than explains it.

From self to speech: mirrors, other minds, imitation, tools and talk

The aforementioned “high-level” tasks have become staples of animal cognition work for a variety of reasons, including that they seem to form a cluster of related capacities in human beings. Take, for instance, mirror self-recognition, which has been investigated via the widely-used “mark test” initially developed by Gallup (1970). In this experiment, anesthetized chimpanzees were marked (or invisibly sham-marked) on their foreheads, and then observed after recovery from the anesthetic. Gallup’s chimpanzees, who had extensive prior experience with mirrors, showed a significant increase in touching the mark and other apparently self-directed responses when a mirror was present during recovery. Gallup (1979; Gallup et al. 2002) argued that these results show that chimpanzees are self-aware -- a trait he believes to be limited to the great apes and humans (Gallup et al. 2002). Gallup et al. also draw a connection between mirror self-recognition and the capacity to attribute mental states to others. They write (2002, 329), “The rationale for postulating a connection between self-recognition and mental state attribution is simple. If you are self-aware then you are in a position to use your experience to model the existence of comparable experiences in others.” This links mirror self recognition to so-called “theory of mind” -- a topic that has considerable currency in studies of animal cognition, having originated in that context three decades ago with the work of Premack and Woodruff (1978).

Theory of mind also connects to the topic of imitation, a subject of intensive debate among animal cognition researchers. At the center of this debate have been questions about how to define ‘imitation’ -- whether to require that imitators understand the motives and goals of the demonstrator, and whether to require that the behavior of the observer is strictly mimicked. Somewhat ironically, goal-copying has turned out to be easier to demonstrate for nonhuman animals than has behavioral copying. It seems, for instance, that human children are much more likely than chimpanzees to mimic the actions of their teachers regardless of whether any other goal is served by doing so. For example, after watching a demonstrator retrieve an item of food from a multipart contraption, chimpanzees skipped the steps of the demonstration that were obviously functionless whereas children copied those steps faithfully (Horner & Whiten, 2005). Horner & Whiten suggest that the strong tendency of children to recognize the role of teachers and to copy their movements faithfully is a developmentally significant species difference.

Faithful copying has also been implicated in the use of tools and the emergence of cultures. Early hominids had used stone tools for over two million years, but changes in the kinds of tools they produced occurred very slowly during this enormous expanse of time. During the past fifty thousand years or less there has been an explosion of innovation in tools and technology, and the accumulation of tools and techniques across multiple generations has been described as due to a cultural “ratchet” (Tomasello et al. 1993). Tool use in animals has been a hot topic ever since Jane Goodall observed chimpanzees fishing for termites with sticks at Gombe, and there are now very many observations of other kinds of tool use in the wild by chimpanzees, and other more or less disputed claims for tool use in species as diverse as orangutans, New Caledonian crows, and dolphins. The differences in tool usage among wild populations have also led ethologists to argue that these differences are culturally acquired. In an experimental setting, Whiten et al. (2005) also demonstrated what they called conformity to cultural norms of tool use in chimpanzees, where group members copied the behavior of high-ranking individuals even though they had initially learned to retrieve food from an apparatus using a different technique. Comparative psychologists have also explored the physical understanding of tools in laboratory experiments using a variety of primate species, with conflicting results (Visalberghi et al. 1994, 1995; Povinelli 2000), and varying interpretations of what such studies show about causal reasoning in animals (Penn & Povinelli 2007). These studies have been connected back to theory of mind through thinking about the capacity of animals to reason about "hidden" or invisible causes, with the mental states of others falling into that category.

Questions about symbolic communication have also been extensively studied in a number of high profile studies. These include attempts to teach human-like languages to animals, such as the bonobo Kanzi (Savage-Rumbaugh 1996, Savage-Rumbaugh et al. 2004) and the African gray parrot Alex (Pepperberg 1999). But they also include studies of the natural communication systems of animals (see Radick 2007 for a history), such as the alarm calls of vervet monkeys (Cheney & Seyfarth 1990), prairie dogs (Slobodchikoff 2002), and chickens (Evans et al. 1993; Evans & Marler 1995), and the social signals of baboons (Cheney & Seyfarth 2007) and dogs

(Bekoff & Allen 1992). Philosophers familiar with the arguments of Grice (1957; also Dennett 1983) and Davidson (1982) will immediately understand the potential relevance of such studies to theory of mind, although few scientists have waded far into this philosophical thicket. Instead, those who conduct such studies (and their critics) have been concerned with the capacity of animals to convey environmental information ("functional reference" -- Evans & Marler 1995) and the signal properties alone and in combination that make this possible (Pepperberg 1999; Zuberbühler 2000, 2001; Slobodchikoff 2002; Savage-Rumbaugh et al. 2004).

The issue of animal communication also connects to culture through the issue of what have been called dialect differences in (for example) bird song, honey bee dances, and whale song. Of course, the relevance to understanding human natural languages, with their rich recursive syntax and compositional semantics, of animal communication, whether using natural or artificial signals, is hotly contested by linguists. For example, Pinker (1994) likened the ape-language studies to an attempt to learn something about elephants' trunks by teaching their nearest living relatives, the hyrax, to pick up objects with their rather unremarkable snouts. He similarly dismissed the relevance of field studies of animal communication to the origins of language by saying that as a scientific hypothesis it has as much going for it as Lily Tomlin's quip that language was invented by a woman who first exclaimed "What a hairy back!" (presumably not the first instance of self-reference). Nevertheless, there are many scientists who assert that the investigation and cautious interpretation of the communicative capacities of nonhuman animals is a worthwhile endeavor (Hauser, Chomsky & Fitch 2002).

In this section, I have barely scratched the surface of current research in animal cognition, which has especially burgeoned in the most recent decade. Nevertheless, I hope to have conveyed something about how studies of mirror self-recognition, theory of mind, imitation, tool use, and communication are conceived as addressing an interrelated set of questions about the cognitive capacities of animals. Other currently active areas of study can also be assimilated to this nexus. For instance, the experimental work on episodic memory in birds (Clayton & Dickinson, 1998) has engendered discussion of whether it has been or can be established that their memories are genuinely autobiographical (Tulving 2005), thus connecting to the concept of self-awareness with which Gallup began almost 40 years ago.

Comparative development

As the preceding section illustrates, studies of the cognitive capacities of animals span a wide range of taxonomic groups. Despite the appearance that such work is strongly comparative, it is rare however to find multiple nonhuman species compared in a single study. Single species studies of mirror self-recognition have been attempted in elephants (Plotnik et al. 2006), cotton-top tamarin monkeys (Hauser et al. 1995; Hauser et al. 2001 described the failure to replicate the first study), bottlenose dolphins (Reiss & Marino 2001), and (somewhat notoriously) pigeons (Epstein et al., 1984) -- the latter seeking to undermine the interpretation of Gallup's original chimpanzee work. Gallup (e.g., Gallup et al. 2002) has been very critical of several of these studies, and considers only the evidence from orangutans and bonobos to be on a par with that from chimpanzees.

While Hauser’s original studies with cotton-top tamarins are not persuasive, and his research program in general has come under increasing clouds since his academic misconduct was reported in August 2010, his attempt to increase stimulus saliency, by diverging from Gallup’s use of a small mark and instead using day-glo coloured hair dye on the white streak that gives cotton-tops their name, raises an important issue about the cross-species validity of the original mirror-mark test. Continuing in this vein, Rajala et al. (2010) provide evidence of mirror-guided self-directed behaviors in rhesus monkeys with cranial implants, including inspection of hinder parts of their anatomy.

Another major methodological problem concerns finding species-appropriate alternatives to the mark-touching behavior that provided the measure in Gallup's original and subsequent studies – a problem that is especially acute for dolphins, but also requiring modification for members of other species. There is, indeed, something compelling about seeing a dolphin twist and turn in front of a mirror in an apparent attempt to see a mark on its body that cannot be seen directly. However, the contortions that the experimenters themselves go through in order to adapt Gallup's experiment to other species can leave one with the impression that it is human ingenuity in impressing each other that is primarily on display. And since these contortions are not applied equally to different species, including humans, the resulting studies fail to be strictly comparable.

Those carrying out such investigations typically espouse strongly comparative aims, of course. For instance, Reiss & Marino framed their dolphin study in their paper's introduction as follows:

> In humans, MSR [mirror self recognition] does not emerge reliably until 18-24 months of age and marks the beginning of a developmental process of achieving increasingly abstract psychological levels of self-awareness, including introspection and mental state attribution. ... A provocative debate continues to rage about whether self-recognition in great apes implies that they are also capable of more abstract levels of self-awareness. (2001, 5937)

Although the quoted passage is overtly developmental and comparative, the study it frames is really neither. First, with respect to development, the dolphin subjects were not tracked longitudinally as part of the study, and thus we have no idea what prior experiences may or may not have been important to their responsiveness to mirrors or their actual self-conception. (Granting, for the sake of argument, that they have one.) Second, with respect to the comparative aspects, the study itself only involved one species (and only two members of that species), while the other species mentioned are from a lineage that diverged at least 65 million years ago. It is also clear that human development provides the benchmarks. Human children typically engage in mirror-guided, self-directed behavior before they pass "theory of mind" tests such as the false belief task (Wimmer & Perner 1983), and many psychologists have suggested that the former is a significant step towards the latter (as implied by Reiss & Marino in the quotation above). But whether this is necessarily so for all species is unaddressed by the mirror-recognition-or-bust approach that seems to predominate in this branch of comparative psychology. Furthermore, although the connection of MSR to "theory of mind" has not been tested directly by experimentation, other putative precursors to theory of mind such as imitation and pretend play do not emerge in a strongly correlated way with MSR (Nielson & Dissanayake 2004).

My intention is not to single out Reiss & Marino for special criticism, nor studies of mirror self-recognition in general. Similar things could be said about the approach taken to any of the other topics introduced in the previous section. Take, for example, the way that theory of mind tasks have been tackled. With the invention of the false belief task, theory-of-mind studies took a developmental turn, at least for human subjects. In the original version of this task, the "Sally-Anne" task (Wimmer & Perner 1983), young children were asked where a character named "Sally" would look for an object that they had seen another character "Anne" move and hide while Sally was out of the room. Children under the age of 4 were generally found to indicate the present location of the object as the place where Sally would look. Somewhere during their fifth year, children normally start to indicate that the Sally character will look in the first location for the hidden item. This is taken as evidence that they have developed the capacity to attribute false beliefs to others -- a benchmark for theory of mind. A substantial literature has grown in the human developmental psychology literature about children's attainment of this cognitive benchmark. But even if this was a fruitful way to study cognitive development in humans (not everyone would agree), the Sally-Anne task was unsuitable for comparative work with animals because of its reliance on verbal questions. Eventually, however, non-verbal versions of the false belief task were developed (Clements & Perner 1994; Call & Tomasello 1999), but they seem much more open to interpretation, and they have been applied with varying degrees of success to members of various species and much younger children. The approach seems to be one of plucking animals out of their cages to see whether they can do something that is important in human cognitive development, without fully investigating how their histories might have prepared them to succeed or fail.

One response to these complaints is to say that these scientists are simply motivated by different questions. It is one thing to ask *how* subjects come to have the capacities that they do, it is another to ask *what* those capacities are. Defenders of the approach claim that testing animals to see how they match up to developmental benchmarks that are significant in human development provides useful information about the cognitive capacities of different species, regardless of how those capacities were acquired. However, this response assumes that we have a clear conception of the experimental task that the research subject confronts. The problematic nature of this assumption -- that the tasks are the really the same for members of different species -- is highlighted by Susan Jones in her discussion of studies comparing imitation in chimpanzees and children (2005a). She writes that: “Although in all of their comparative work the researchers model the same behaviors for the two species and then measure the same imitative responses, the chimpanzees and children are never really in the same tasks" (297). She points out that the two kinds of subjects have different histories and are on different developmental trajectories, making the experimental results hard to interpret. Furthermore, Jones points out, children "learn to imitate in the context of thousands of vocal exchanges with the caretakers ... the poor chimpanzee is at a distinct disadvantage -- unless someone imitates his vocalizations from an early age" (301). The task confronting an individual who has been in imitative social interactions for almost her entire life is not the same as that confronting an individual who must try to figure out what is expected of her today.

The idea that human competencies provide a good benchmark presupposes that we fully understand what those competencies actually are, and how they were acquired. But we are far from understanding this, even for some of the "highest" achievements of human cognition. For instance, there is evidence that competent symbolic reasoners rely on processes that are sensitive to *unintended* regularities in the symbolic environment. Landy & Goldstone (2007a,b) found that various visual cues of perceptual grouping in symbolic formulae are exploited by competent algebraists, even though these cues are not explicitly taught, and even though their use can lead reasoners away from the intended meanings of the formulae. Likewise, McNeill and Alibali (2004, 2005) present evidence that elementary school children in the U.S. suffer a large drop in performance in some kinds of arithmetic reasoning problems between first and third grades because the intended meaning of the equality sign is obscured by their implicitly learned responses to spurious regularities in the worksheets used for addition drills, such as the directionality of the perceptual-motor task. What is considered a normative failure by those involved in setting the tasks may in fact be the result of powerful pattern extraction processes at work on objectively measurable structural regularities in the stimuli sets.

The upshot is that many ostensibly “comparative” and “developmental” studies fail to be sufficiently comparative because they fail to investigate the actual learning and experience during development of their own research subjects, and because they assume that the experimenters’ understanding of the context of the experimental task accurately represent the context in which the subjects are actually making their decisions (see also Stenning & van Lambalgen (2008) for a related critique of the experimental literature on human reasoning “errors”). Members of different species, and even individuals within the same species, are only in the same experiment if one chooses to ignore a raft of things that might matter to the outcomes of those experiments.

Development and Comparative Cognition

The idea that development needs to be taken more seriously is hardly new. Both ethology and behavioristic psychology were criticized by Lehrman (1953, 1971) for being insufficiently grounded in the biological facts: in the former case for assuming an untenable notion of instinct that ignored or severely underplayed the developmental plasticity of organisms (Lehrman 1953), and in the latter case for failing to sufficiently address species differences that constrain what animals can be trained to do (Lehrman 1970). A half-century of rapprochement between “biological” and “psychological” approaches to animal behavior and cognition (e.g. Shettleworth 1998) have not entirely closed the gap. Critics of cognitive ethology, such as Wynne (200x), Blumberg & Wasserman (1995, 2010), and Shettleworth (in press), still believe they hold the empirical high ground, while defenders, such as Cheney & Seyfarth (2007), question the ability of psychologists to uncover the full power of primate and other animal cognition in the socially and ecologically simplified conditions imposed on laboratory animals.

In the first paragraph of their 2002 paper, quoted above, McGonigle and Chalmers cite Fodor, Chomsky, and Piaget as paradigmatic critics of associationist accounts of human cognitive abilities. To philosophers steeped in the folklore of the cognitive revolution, Chomsky's “Cartesian linguistics” and Fodor's nativism represent the apogee of a rationalistic response to the radical empiricism of Skinnerian psychology. Piaget, however, is much less discussed by philosophers of mind and cognitive science. For instance, if one compares references to the two scientists in the Stanford Encyclopedia of Philosophy (SEP) (Zalta, 2010), Chomsky is mentioned or cited in over fifty articles while Piaget appears in fewer than a dozen. The SEP search engine results for "Piaget" reveal that the articles mentioning him (e.g. "The Philosophy of Childhood", "Philosophy for Children", "The Philosophy of Education", “Panpsychism”) are much less central to the discipline as a whole than those in which "Chomsky" appears, and the entry on "Cognitive Science" does not mention Piaget at all.

My point here is not to argue that philosophers should be paying more attention to Piaget specifically (although perhaps they should), but to point out the relative neglect of developmental perspectives in the philosophy of mind and philosophy of cognitive science. I don’t deny that philosophers of mind make a lot of hay of the work of some developmental psychologists, but the fact is that methodologically and conceptually the developmental psychologists receiving the most attention from philosophers represent a particularly language-centered perspective on cognitive development. They focus on organisms (human children) for whom the acquisition of language, intentional psychology (or "theory of mind"), and causal understanding of the world (including "folk physics), are (at least to a first approximation) environmentally robust outcomes, and they argue about the extent to which these stable outcomes should be attributed to the possession of innate concepts or knowledge. There has been little attention by philosophers to the role that developmental thinking (or its absence) might play in other debates within the philosophy of mind, such as arguments about the multiple realizability of cognitive capacities (for an exception, see Lloyd, 2004). And while there have been attempts to investigate the developmental effects of raising primates, especially apes, in human-like environments, these efforts suffer both from being examples of the anthropocentric, trophy-hunting approach, and from being shots in the dark with respect to the conditions that actually matter for cognitive development because we don't understand the subtleties of our own cognitive development well enough to know what might work with members of other species.

Private codes and public structures

It is precisely here, then, that I find the ambitious agenda set out by McGonigle and Chalmers to have the greatest significance. But I also wish to present some friendly amendments.

First, framing the discussion as a debate between associationism and cognitivism may not be entirely helpful (see also, Smith 2000). There are many issues in the mix here, but a general (although not universal) predilection among comparative psychologists for associative accounts of learning provide one major axis. Minimalists accept the label of "associationism" and seek to reduce all learning to limited set of principles concerning the strengthening and weakening of connections among various combinations of stimuli and responses. McGonigle and Chalmers resist this, of course, maintaining that it leads to the language as "magic bullet" view.

Today's associationism includes learning mechanisms that are much more powerful than those of the behaviorist heyday and McGonigle & Chalmers might be faulted for not acknowledging the more powerful model of classical conditioning offered by Rescorla & Wagner (1972), as well as subsequent developments of this model (Wagner 2008). The power of new models of learning makes it less clear whether there's any cognitive capacity, including language learning (although I shall not argue for that here), that cannot be explained in terms of today's more powerful associative mechanisms. Cameron Buckner argues (under review, & dissertation) that recent discussions about "associationist" versus "cognitive" explanations of animal behavior have tended to vacillate between different conceptions of associative mechanisms. For instance, some authors treat connectionist models as associationist. But, under certain idealizing assumptions, some connectionist architectures are Turing-equivalent and therefore as powerful as any computational model, the latter being subject also to specific idealizing assumptions. If associationists have the full resources of connectionism at their disposal, then associative mechanisms are inductively no weaker than other kinds of computational models. However, with such powerful mechanisms at their disposal, the problem becomes one of understanding how cultural and developmental phenomena provide scaffolding that shapes the learning that is actually observed.

Even if we exclude the most powerful connectionist architectures and newer statistical learning techniques from the class of basic, or minimal associationist models, and reserve this category for "the educated salivations of a Russian dog" as supplemented by the tutored bar pressing of a Harvard rat, this was only ever two-thirds of the original associationist package. By the original package, I mean Hume's claim to be the first to enumerate the "three principles of connexion among ideas, namely, Resemblance, Contiguity in time or place, and Cause or Effect" (Hume 1748). For Hume, of course, association was between ideas (faint copies of sense impressions) -- relata that most comparative psychologists would reject as too subjective (perhaps too much like private codes, in that specific sense) and based too much on a developmentally naive theory of concept learning that was opposed to equally untenable ideas about innate ideas. Setting aside such issues, and without hanging too much on acceptance of Humean associationism here, his three-part taxonomy does nevertheless help us see where behaviorism swept some things under the rug. For while it is arguable that Pavlov was onto contiguity, and Skinner onto cause-effect, a treatment of resemblance was needed for both – which is where notions like “stimulus generalization” were deployed. Arguably, although I won’t argue it here, “stimulus generalization” is more a description of the phenomenon than an explanation of it, and resemblance still resists a completely adequate treatment notwithstanding various theories of similarity among psychologists such as Tversky (1977), Shepard (1987), Markman & Gentner (2005), and others (but see Vigo 2009a,b; Vigo & Allen 2009).

By our best accounts, resemblance is a kind of structural relationship. With this in mind, I view McGonigle & Chalmers (2006) as making a move in Hume’s direction when they argue that the cognitive competences of monkeys with respect to relations between objects entail the existence of “private codes”. The notion of a “code” here introduces the idea that internally-represented, prelinguistic, structural relationships underpin such things as the capacity to learning serial orderings. McGonigle and Chalmers intend the availability of such codes equally as a counterweight to the aforementioned “overdependence on a relatively weak inductive mechanism” among comparative psychologists, and against the assumption that only language makes relational learning possible. The danger is that such “codes” are themselves just code for the “ideas” that psychologists became rightly suspicious of during the 20th Century.

The notion that such codes remain private in monkeys until externalized by human artifacts leaves us scrambling for an account of the origins of those codes. McGonigle & Chalmers (2006, 248) hedge on this, suggesting that the intensive, long-term, regime in which they investigate monkey learning may be better viewed as bringing *on* rather than bringing *out* the cognitive competencies. This suggests that the relational codes themselves may be themselves a product of development in a particular context. But if that is the case, then it doesn't seem right to suggest that these codes were already present, waiting to be externalized by dint of the superior manipulation skills of humans. The complex interplay between the machines, the monkeys, and the experimenters who designed the software (not to mention the comparative psychologists, mathematicians, computer scientists, engineers, and philosophers before them) must also be expanded to include the daily experiences of the monkeys *outside* the experimental situation.

However, when McGonigle and Chalmers (2002; see also the chapter by Kusel and Chalmers, this volume) suggest, following Clark (1973), that humans have a biological bias to scan from the ground up which makes the larger objects on the touchscreen more salient, and could provide an objective grounding for the relational codes deployed in the learning task. This also suggests an evolutionary origin for the private codes. But that it applies to monkeys (or even for humans) should not be considered as a given. For one thing, the experimenters could perhaps be mistaken about the relevant feature, having introduced spurious correlations between size and other features into the stimulus set -- e.g., if the icons on a screen are always arranged so that the relative position of the top or bottom edges correlates perfectly with size, then we have no assurance that size is what the monkeys are responding to. Without knowing more about the experiences of the monkeys in the rest of their daily lives, we might have no idea whether they come to the experiment with a bias towards noticing size or edge position, and further experimentation runs the risk of altering those biases since the relevant learning is likely to be context sensitive.

Furthermore, the extent to which experiments focus on sensitivity to manipulations in one feature dimension of simple stimuli, they ignore the more complex relationships among features of sets of stimuli that these animals have to deal with in their daily interactions with each other, their human caretakers (if captive) and the complex ecological context in which resemblance is not single dimensional affair. Animals learn how combinations of multiple cues predict various outcomes. A pressing goal for theoretical cognitive science is to describe the structural properties of sets of stimuli that make it possible for animals to coordinate their behaviors around objects and events in their environments without the linguistic ability to say what they mean. Such structure is beginning to be objectively described and quantified (e.g. Vigo 2009a,b; Vigo & Allen 2009) although much work remains.

What might a thoroughly developmental comparative psychology look like? Ideally it would consider the entire range of developmental inputs, including but not limited to participation in previous experiments, as potentially relevant to the outcome of any particular experiment, and would control for those inputs. But this is impossible. The sheer complexity and adaptability of organisms to different life histories makes the space intractable. The set of possible sequences of behavioral experiments itself defies enumeration let alone systematic investigation, not to mention the sheer variety of species that could be studied. Is comparative psychology therefore condemned to be just whistling in the dark? I take a more optimistic view. Given an intractable space, one can proceed by trying to build bridges between parts of the space initially under separate investigation. I think this is the real value of the interdisciplinary work that McGonigle and Chalmers pioneered. In the course of their work they demonstrated that it is possible to investigate the relationship between specific cognitive outcomes and various forms of scaffolding that were not usually in the purview of either strict learning theorists or magic bullet enthusiasts. They showed how their monkeys could develop cognitive capacities built on prior experience, acquired over the long term. A thoroughly developmental comparative psychology doesn't run away from the problem of the accumulation of capacities by refusing to reuse subjects in different experiments, but treats those prior experiences as a variable. And while it is not feasible to investigate every possible influence, it is possible to be on guard against over-interpreting the results of any given experiment as representing "the cognitive capacities" of all members of a given species. In the excitement of the chase for particular trophies, this can be hard to remember. So, I believe, comparative developmental psychology stands in need of a new canon of interpretation, not unlike Morgan's. Not a decision procedure, but a useful heuristic against over-interpretation of the results of experiments in comparative cognition: "In no case may we interpret the action as the outcome of the exercise of a general capacity of the species, if it can be interpreted as the outcome of a developmental process specific to the individual."

Acknowledgments

I am grateful to Keith Stenning, Margaret McGonigle-Chalmers, Iain Kusel, and the members of my dissertation writers group at Indiana University, especially Cameron Buckner, for helpful criticism of earlier drafts of this material.

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to appear in *The Complex Mind: An interdisciplinary approach*, McFarland, D., Stenning, K. and McGonigle-Chalmers, M. (Eds), Palgrave-Macmillan

Personal note for intro

It is my everlasting disappointment that I never met Brendan McGonigle. We carried on an intermittent correspondence by email that began about a year before his untimely death. Our correspondence began when I received an email message out of the blue in which he reacted to a paper I had written about “rational” vs. “associative” explanations of transitive inference in animals. I was greeted with “Colin!” ... and thus began our discussion of the themes appearing in my contribution to this volume. I sometimes let weeks and months go by between messages during that year. In retrospect, precious time squandered. Carpe diem.