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Mind & Language, ISSN: 0268-1064
Vol. 13. No. 2 June 1998, pp 195–204.

Evolutionizing the Cognitive Sciences: A Reply to Shapiro and Epstein

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We have argued that the social sciences—anthropology, economics, sociology—will be revolutionized when their practitioners realize that theories about the evolved architecture of the human mind play a necessary and central role in any causal account of human affairs (Tooby and Cosmides, 1989, 1992; Cosmides and Tooby, 1994a). We have further argued that cognitive scientists will make far more rapid progress in mapping this evolved architecture if they begin to seriously incorporate knowledge from evolutionary biology and its related disciplines—behavioural ecology, paleoanthropology, hunter-gatherer studies, and primatology—into their repertoire of theoretical tools, and use theories of adaptive function to guide their empirical investigations (Cosmides and Tooby, 1987, 1989, 1992, 1994b; Tooby and Cosmides, 1992). Shapiro and Epstein (S&E) have responded to our arguments with a series of mild criticisms and modest endorsements. Here, briefly put, are S&E's main points:

(1) Evolutionary theory may play a heuristic role in cognitive theorizing, unlike what Cosmides and Tooby think.
(2) An evolutionary point of view does not *deductively* force a domain-specific view—experiments will still have to be done.
(3) Sociologically, there is not a trend towards increasing acceptance of domain-specificity among cognitive psychologists—look at the connectionists.
(4) Some cognitive psychologists (e.g. Chomsky) have always favoured a domain-specific approach.
(5) General-purpose mechanisms can be applied to a wide variety of tasks successfully, so to show that the mind solves specific tasks does not force the conclusion that it does it through domain-specific means.
(6) One can employ functionalist reasoning without being evolutionary.

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(7) Evolutionary theory may help explain why cognitive mechanisms have the form they do.
(8) Not all behaviour is the product of cognitive processes.

There is no news here for any alert cognitive scientist, and several of these points are central to rather than in conflict with much that we have written, or at least form agreed-upon starting points for deeper explorations of the issues involved. Indeed, most of these points, when advanced as disagreements, are not real matters of intellectual substance, but rather definitional issues or arguments built out of a failure to understand the context and thrust of the initial assertions that they dispute. We will reply to each point in turn.

1. Points 1 and 2

Consider S&E's conclusion that evolutionary biology ought to play a heuristic role, in implied contrast to our supposed position. There is no contrast, however. We have always been very explicit that an evolutionary perspective is heuristic—it adds information that is useful in designing experiments and in building theories, supplementing the other sources of knowledge already mined by cognitive psychologists (Cosmides and Tooby, 1987, 1992, 1994; Tooby and Cosmides, 1992). More baffling, we have never argued that it should replace experimentation—on the contrary, we have always emphasized that its central contribution will be through guiding better experimentation. In our writings, we have identified a number of different ways that cognitive researchers could profit in experimental design from being more aware of the literature on behavioural ecology, hunter-gatherer studies, evolutionary game theory, paleoanthropology, primatology, and so on; we have discussed in rich detail how these heuristic links work; and we have gone on to provide some demonstrations of successful experiments that no one would have thought to do in the absence of an evolutionary functionalist perspective. To engage in a meaningful dispute, S&E would need to show (or at least argue) that these new bodies of knowledge cannot help researchers design experiments, but are in principle irrelevant. They do not do this. They merely argue the point that evolutionary biology by itself is not sufficient, independent of experimentation: something both 'sides' already agree on.[1] The real difference here may be that they consider theories

[1] Consider this entirely typical quote from Cosmides and Tooby, 1994, p. 51: '(Evolutionarily-based) computational theories address *what* and *why*, but because there are multiple ways of achieving any solution, experiments are needed to establish *how*. But the more precisely you can define the goal of processing—the more tightly you can constrain what would count as a solution—the more clearly you can see what a mechanism capable of producing that solution would have to look like. The more constraints you can discover, the more the field of possible solutions is narrowed, and the more you can concentrate your experimental efforts on discriminating between viable hypotheses.'

to be *merely* heuristic, whereas we think—out of millions of possible theories and experiments—that being guided towards hypotheses that are more likely to be true is critical: the difference between a living science and an inert one is whether practising scientists have good heuristic principles guiding their research.

2. Points 3 and 4

S&E state: 'As we show, psychology today is not 'coming around' to a domain specific view of the mind, as C&T intimate. Rather, psychology is in a state of flux on the issue' (p. 172). First of all, S&E do not 'show' this, but rather make a few points of a 'no, it isn't' kind. Not a very profitable line of discussion, and not very meaningful either, since matters of truth are not decided by majority vote. But, as before, we don't necessarily disagree. Scientific history is not deterministic, fashions occur, things might go either way. Head counting is not the point—it would be vacuous to argue that there is a bandwagon and people ought to jump on *because* there is a bandwagon. We raised the issue of what people *ought* to be doing, and discuss what people *are* doing only insofar as past track records of various approaches could be informative.

What we have argued is that *successful* areas of cognitive science—that is, areas with a sustained record of progress in elaborating increasingly well-validated models of well-operationalized phenomena—have been those in which researchers freely entertained the hypothesis that there might be principles special to their domain that were not applicable outside of it. Perception (e.g. colour constancy, depth perception, psychophysics), and language (e.g. syntax, phonology, semantics, argument structure in verbs) are notable in that they are relatively successful, and that these research communities freely apply domain-specific principles alongside hypothesized principles and processes that are held to apply more broadly. We discuss these as paradigm cases of the advantages of taking a domain-specific approach. (Clearly, since we have dwelled at length on the relative success of research in perception and language as compared to other areas, neither we nor anyone else really need to be informed that Chomsky's proposals for domain-specificity antedated evolutionary psychology.)

We have pointed out that some other cognitive science research communities are being similarly transformed by allowing domain-specific elements to play a role in their theories, and that these research areas are becoming similarly successful. For example, the field of cognitive development has metamorphosed from a theoretically domain-general community into one that is now very different. In the last 15 years a series of new experimental findings have prompted domain-specific theories of these phenomena (and vice versa), with topics such as theory of mind (Baron-Cohen, 1995; Leslie, 1987), intuitive physics and object mechanics (Baillargeon, 1986; Spelke, 1988, 1990; Leslie, 1994), eye direction detection (Baron-Cohen, 1994), and folk biology (Atran, 1990; Hatano and Inagaki, 1994; Keil, 1994, Springer, 1992)—a transition towards increasing tolerance of domain-specific theories, matched by increasing explanatory success.

S&E's discussion of how various cognitive science subcommunities remain unpersuaded (e.g. connectionists) is, of course, true, obvious, and not in contention. There would have been no point in our attempting to persuade researchers that a change in practice would be productive if most cognitive scientists already agreed with us.

At the conclusion, they argue that even if cognitive scientists did adopt a more domain-specific approach in response to learning more evolutionary biology, this would require no change in cognitive science because cognitive science has always included domain-specificity. This seems a quibble: when confronted with the hypothesis that the human cognitive architecture has, built in, mechanisms specialized for reasoning about social exchanges, threats, precautions (and so on), most cognitive scientists have treated it as one of the most outlandish and science fictional claims they have heard. (Indeed, a far more modest proposal—that there are cognitive specializations for distinguishing living from nonliving things—is considered 'an apriori implausible hypothesis'! (Farah, Meyer and McMullen, 1996).) They do not even consider pursuing similarly domain-specific hypotheses in their own work. If in ten years many more cognitive scientists were open to finding narrow specializations that might be playing a role in the mechanisms they are studying, this would be a dramatic transformation of research practice, despite the fact that Chomsky introduced domain-specific approaches 30 years ago.

What would be worth disputing is whether perception and language either (1) are not successful areas, or (2) do not owe their success to employing at least some domain-specific theoretical entities. S&E do argue that there have been some token connectionist attempts at explaining syntax, but they do not review the controversy in depth, and seem unaware that (domain-general) connectionist models of syntax are quite sharp failures. It is not sufficient to cite someone who disagrees with a point (e.g. connectionists); one needs, rather, to mount a serious argument.

3. Point 5

According to S&E, showing that the mind solves specific tasks does not demonstrate that this happens via domain-specific means. This is true, but beside the point. We have made some serious and extended arguments about why many more cognitive mechanisms will turn out to include domain-specific features alongside whatever domain-general principles of operation they have (Tooby and Cosmides, 1990, 1992; Cosmides and Tooby, 1987). These include the following:

(1) *Functional incompatibility* (Sherry and Schacter, 1987): the argument

that whenever the requirements for computing biologically successful outcomes are different from each other (as they seem to be in a large number of different domains, most of which have not been addressed by cognitive scientists), then computational machinery designed for processing each of these domains will require some domain-specific features (e.g. predicting inanimate vs. animate motion; avoidance learning with ingested toxins as opposed to externally inflicted pain; applying the propositional calculus as opposed to identifying cheaters; computing food value according to different criteria than mate value; avoiding incest by applying different mate criteria when dealing with kin than non-kin; helping kin versus non-kin, and so on).

(2) *Tradeoffs between the scope of problem-solving architectures vs. their efficiency*. Crudely, the more narrow the problem-type, the more cognitive procedures can be specialized to solve the problem with high efficiency. The more general the problem-solving strategy, the less efficient it will be: generality can be purchased only by eliminating all knowledge that could improve problem-solving in one domain but be misleading in another (e.g. concepts such as belief, desire, and intention are useful for explaining the behaviour of people, but not of rocks). For evolved systems, efficiency makes a critical difference in which designs will prevail.

(3) *Computational sufficiency*. Most existing well-specified domain-general models fail learnability tests (e.g. Pinker, 1979, 1984; Wexler and Culicover, 1980) or computational sufficiency tests on an entire range of problem-types that we know biological systems can solve.

S&E do not discuss these arguments, and provide no serious analysis in their place. The most we get is a brief analogy: the same screwdriver can unscrew or screw in the same screw: two different tasks solved by the same mechanism. This hardly passes as serious discussion: cognitive explanations strive to make all aspects of a system mechanically explicit, and here the authors have separated off and excluded from analysis the decision-making machinery that determines how the screwdriver is used—an element critical to making their argument work. This machinery does, in fact must, distinguish operationally between these two different uses (i.e. it has 'domain-specific' elements).

It is, of course, true that domain-general (and domain-specific) mechanisms can solve a variety of tasks, including ones they were not designed to solve. No one disagrees here. Acknowledging this does not, however, refute the claim that there exist formally definable problems that humans routinely solve (or evolved to solve) that no presently proposed domain-general architecture is capable of solving. Chomsky made initial forms of this argument 30 years ago, Pinker has continued to demonstrate its efficacy in the case of language learnability, and we have applied it to a range of problems outside of language. The authors S&E need to analyse these arguments seriously, rather than simply register their disagreement with them.

Of course, the deepest issues in cognitive science involve the heterarchical and cross-cutting scope of different mechanisms: what domains are native to the human mind, what is the mixture of domain-specific and domain-general devices that compute over these domains, what are the designs of these devices, and how do they pass inputs and outputs back and forth. These same issues apply to human anatomy and physiology: even tissues of very different organs employ identical processes at some levels (RNA transcription, mitochondrial energy production) and highly specialized processes at others (hemoglobin binding for oxygen transport). Some structures and processes are universally employed by all cells; some are near-universals with sharp exceptions (all cells in the body have cell nuclei except red blood cells); and some are unique to one kind of tissue. Sorting this out at an information-processing level will be a fascinating task, and the answer will not be reducible to 'Everything is domain-specific' or 'Everything is domain-general'. Indeed, we ourselves have proposed (relatively) domain-general frequentist mechanisms that play a role as inputs into many types of judgments (Cosmides and Tooby, 1996; Brase, Cosmides, and Tooby, 1998), and domain-specific inferential systems for others (Cosmides, 1989; Cosmides and Tooby, 1992, 1997).

4. Point 6

Yes, there is no doubt one can employ functionalist reasoning without being evolutionary, if one is content with folk theories of functionality. Folk theories are sometimes good enough, as with Harvey and the circulation of the blood, and with vision—especially when research hypotheses can be generated by reference to already existing machines (valves and pumps; cameras) that are functionally analogous. But biological systems have biological definitions of functionality built into them, and these frequently depart radically from folk notions (e.g. the adaptive problems associated with habitat selection (Orians and Heerwagen, 1992; Kaplan, 1992), disgust (Tooby, 1982), aggression (Popp and DeVore, 1979), pregnancy (Haig, 1993; Profet, 1992), coalition formation (Tooby and Cosmides, in press), kin-directed social actions (Hamilton, 1964; Trivers, 1974). So far, psychology's successes have been limited to cases in which folk theories of functionality happen to converge on evolutionary functional definitions. It would be nice to understand what the rest of the machinery is like. Yet the design of that machinery exists because it performed evolutionary functions—not because it performed functions that twentieth-century humans happen to intuit.

5. Point 7

According to S&E, evolutionary theory may help explain why cognitive mechanisms have the form they do. Of course. But this post hoc explanatory role is only one of its uses: it can lead to the discovery of new mechanisms no one would ever have looked for without its use.

6. Point 8

According to S&E, 'Not all behaviour is the product of cognitive processes; 'we criticize C&T's claim that evolutionary considerations require cognitive psychologists to accept the view that all behaviour is under the control of cognitive programmes. While this view seems obviously false upon brief reflection. . .', 'The facts of human evolution will reveal, C&T believe, that *all* of human behaviour must be the product of cognitive processes' (p. 172). This is a clear case of talking past each other. We have used the traditional cognitive distinction between an information-processing level of description and a physicalist level of description. A cognitive approach is not a claim about a type of process (e.g. whether it employs representations), but a way of viewing all processes. It is a choice of analytic level: facts have nothing to do with it. One could have a cognitive analysis of how breathing is regulated through the chemical analysis of the blood (see also Jackendoff, 1987, pp. 29–32). If one chooses to view humans at an information-processing level, by definition all behaviour would be under the control of cognitive processes. If one chooses to view humans at a physicalist level, by definition no behaviour would be under the control of cognitive processes.

What we have argued is that a cognitive-level description is particularly useful in combination with an evolutionary-functionalist approach. The reason is simple. Evolutionary biologists study and provide descriptions of information-processing problems that organisms should be designed to solve (e.g. how should discovering that another worker in the hive is a half-sister change one's behaviour towards that worker and the hive), and cognitive psychologists study the machinery that evolved to solve these information-processing problems. Theories about adaptive function carry implications about the structure of mechanisms designed to realize these functions. When these theories are about information-processing problems, they carry implications about mechanisms described at the cognitive level. Insofar as the same cognitive programme can be instantiated by different physical systems, however, theories of adaptive information-processing problems say less about physical instantiation. Cognitive descriptions mesh with evolutionary theories in a way that physicalist ones do not.

We did not use this language to suggest widening the scope of cognitive research to include physiology. The point was that humans engage in hundreds of activities not considered by cognitive scientists, and that hunter-gatherer studies and evolutionary biology can illuminate which activities are likely to be associated with evolved competences that have so far gone unexplored: danger avoidance, habitat selection, kin recognition, incest avoidance, foraging, and so on.

S&E's analysis might have been more compelling had they chosen some key issues to analyse in depth. But their overall topic is also to blame: How much impact will evolutionary thinking have on cognitive science? Answers can be: not at all, not so much, some, a lot, or tremendously. Opinions on this are not interesting. What is interesting are novel arguments that researchers can use to show what evolutionary biology can—or cannot—do for the cognitive sciences.

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