The Mind is Not (Just) a System of Modules Shaped (Just) by Natural Selection

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1. Did the Mind Evolve by Natural Selection?

Of course our minds and brains evolved by natural selection! They aren't the result of divine intervention or fabrication by space aliens. Nor are they solely products of drift or any other naturalistic alternative to selection. That natural selection profoundly "shaped" the mind and brain is accepted by both by evolutionary psychologists and virtually all of their most vigorous critics.

What, then, is at issue in the debate surrounding evolutionary psychology (hereafter, "EP")? First, there are disagreements about the likely intellectual payoffs of EP's characteristic research strategy. EP employs a 'reverse engineering' methodology: the researcher (i) notes some competence or behavior, (ii) conjectures that it is a solution to some 'adaptive problem' faced by our tree- or savanna-dwelling ancestors, and then (iii) proposes that natural selection engineered a specialized psychological mechanism or 'module' to produce that competence or behavior. Some EP researchers also offer (iv) behavioral or psychological evidence for the proposed module, but, as we shall see, this

evidence is rarely compelling, and other relevant evidence (from, e.g., neurobiology, genetics, or developmental biology) is often not cited. Critics of EP, like us, think that this methodology is unlikely to yield much insight.

We also dispute EP's views about the structure of the human mind, the way in which it develops, and the relation between evolution and mental architecture. Evolutionary psychologists claim that the mind is 'massively modular.' It is composed of a variety of more or less independent 'organs,' each of which is devoted to the performance of a particular kind of task, and each of which develops in a largely genetically-determined manner. EP's hypothesis of massive mental modularity is not just the uncontroversial idea that the mind/brain consists, at some level of analysis, of components that operate according to distinctive principles. For as we discuss in §4, EP endows its modules with a number of additional properties such as informational encapsulation (§4.3) and independent evolvability (§4.4). In addition, EP also makes specific claims about which modules we have. Thus, the modules at issue in EP are not, e.g., small groups of neurons, but are rather the complex processing structures that underlie high-level cognitive tasks like 'cheater detection.'

EP's views about mental structure and development are motivated by two very general evolutionary considerations. First, EP holds that evolution is likely to have favored strongly modular mental architectures. Second, and relatedly, EP holds that mental modules are the fairly direct products of natural selection. This requires that the different modules must be independently evolvable: they must have independent genetic bases so that natural selection can act to change one module independently of the others. It also means that while EP theorists are careful to say in their 'official' pronouncements

that they allow a role for learning and other environmental influences, their more detailed arguments typically assume that the development of modules is tightly genetically constrained.

There are problems with all of these assumptions. First, there is no reason to think that evolution 'must' produce modular minds. Evolutionary psychologists (e.g., Cosmides and Tooby, 1994, Tooby and Cosmides, 1990, Carruthers, this vol.) argue that general-purpose psychological mechanisms would not have evolved because they are too slow and require too much background knowledge and computational space for the making of life-or-death judgments. Specialized modules, on the other hand, deliver fast and economical decisions on matters crucially affecting an organism's fitness, so would have been preferred by natural selection. However, it is simply wrong to suppose that modules are invariably (or even usually) superior to general-purpose devices. What sorts of mental organization will be favored by selection depends entirely on the details of the selection pressures an organism is subject to and its genetic structure. As Sober (1994) shows, such factors as how variable the environment is, the costs of making various sorts of mistakes, the costs of building various sorts of discriminative abilities into the organism etc., can have large effects on the relative fitnesses of general-purpose vs. more specialized strategies. In addition, the ability to adapt quickly (i.e., within the individual's lifetime) to changing circumstances is vital for organisms inhabiting unstable environments. (Maynard Smith et al., 1985). Indeed, there is evidence that both the physical (in particular, climatic) and social environments inhabited by early hominids were highly unstable (Potts, 1996, 1998; Allman, 1999). There thus would have been considerable selective pressure favoring the evolution of cognitive mechanisms allowing

the rapid assimilation of new information and behavioral flexibility, rather than innately-specified modules. (For more on this issue, see §3 below, and Woodward and Cowie, forthcoming.)

Secondly, EP's view that the modules existing in the adult mind are largely genetically specified (or are the products of learning mechanisms that are themselves genetically constrained to produce a particular module as output) is inconsistent with what is known about the role of experience-dependent learning and development in shaping the mature mind. As we argue in §2.3, whatever modular processing mechanisms the adult mind contains emerge from a complex developmental process. Less modular structures and capacities that are present in infants interact with both with the environment and the genes to generate (or be transformed into) new competences that were not directly selected for.

Thirdly, the notion of a module is itself quite unclear. As we show in §4, there are several different (and non-coextensive) criteria for modularity employed in the EP literature. Researchers move back and forth among different notions of modularity, illicitly taking evidence for modularity in one sense to bear on modularity in other senses. They also tend to conflate issues about the modularity of processing in the adult mind with quite separate issues to do with the role of modules in development and learning. These unclarities make EP's claim that the mind is 'a system of modules' somewhat difficult to assess. Our view, defended in §3, is that the mind is not just a collection of specialized modules. Although our minds probably do contain modules in some sense(s) of that term, these structures are unlikely to correspond to the modules (for cheater detection, mate selection, predator avoidance, etc.) postulated in EP.

2. Reverse Engineering: A Backward Step for Psychology

EP is premised on the idea that modern human mental organization is a more or less direct reflection of the ways in which hominids evolved to solve the problems posed by their physical and social environments. Thus, by reflecting on the tasks our ancestors must have been able to solve, and by supposing that whatever psychological abilities enabled our ancestors to solve those tasks would have been selected for, evolutionary psychologists seek to map our current psychological organization. Because they also assume that selection engineers a proprietary solution for each of these 'adaptive tasks,' evolutionary psychologists see the modern mind as 'massively modular': it contains numerous specific mechanisms (or 'modules') which evolved for specific tasks and houses few (if any) general purpose psychological mechanisms.

One problem with this strategy has already been mentioned: it ignores the possibility that *flexibility* might well have been at a selective premium for hominids inhabiting rapidly changing environments. In this section, we discuss three further problems with EP's adaptationist or 'reverse engineering' approach to generating psychological hypotheses.

2.1 Reading Structure from Function.

EP believes that since "form follows function" (Tooby and Cosmides, 1997, 13), one can figure out how the mind is just by considering what it does (or rather, what our ancestors' minds did, back in the 'environment of evolutionary adaptation' ("EEA")). One reason that EP's reverse engineering strategy is misguided is that you *can't* infer

structure from function alone. Instead, formulating and confirming functional and structural hypotheses are highly interrelated endeavors, with information about structure informing hypotheses about function and *vice versa*.

As an illustration, consider how our thinking about human declarative memory has evolved over the last half century (cf. LeDoux, 1996, ch.7). By the 1940s, neurophysiologists had concluded that memory is distributed over the whole brain, not localized in a particular region. [A structural hypothesis.] But then came the patient H.M., who had had much of both temporal lobes removed to treat severe epilepsy. Post-operatively, HM remembered much of what had happened to him prior to the surgery and could form new short-term memories lasting a few seconds. However, he was unable to form new long-term memories. HM thus indicated that short-term memory and long-term memory are distinct [a functional hypothesis], that they are supported by different brain systems [a structural hypothesis], and that the areas responsible for the formation of new long-term memories are different from those allowing storage of the old ones [structural]. Also prior to this, the limbic system (including the hippocampus and amygdala) had been thought to comprise the emotional circuitry of the brain [functional]. But the hippocampus was one of the areas that was so badly compromised in HM, and in other patients with severe memory deficits [structural], indicating that the limbic system was also involved in cognitive functioning [functional] and suggesting that the hippocampus was the seat of memory [structural]. As the workings of the hippocampus were further investigated [structural], it was found to be especially implicated in learning and memory of spatial information [functional]. Further, since all of the patients on whom the early hippocampal memory story had been based had also had damage to the amygdala

[structural], this was an indication that the amygdala was also involved in memory [functional]. (This latter claim is still controversial [functional], given that later studies have shown that hippocampal lesions alone will produce amnesia [structural].)

This vignette illustrates how views about functions are (or should be) highly sensitive to structural information. It thus underscores the naïveté of the assumption (endemic in EP) that one can accurately individuate psychological functions by enumerating the tasks that the mind can perform. Evolutionary psychologists try to avoid this difficulty by inferring functions not (or not just) from behavioral data about what our minds can do at present, but rather from their ideas about which psychological capacities were selected for back in the EEA. In effect, then, evolutionary psychologists think of psychological functions as *biological* functions (in the sense of Wright, 1973): capacities that the mind had in the past that are still present because they were selected for, rather than as functions in the sense of what the mind does at present, regardless of whether they were selected for (*causal role* functions in the sense of Cummins, 1975).

Prima facie, however, this move compounds, rather than solving, the problem just discussed. After all, if it's hard to delineate the functional anatomy of our own minds on the basis of merely behavioral evidence, it's even harder to limn the minds of our ancestors by speculating about what they did and the selection pressures they faced: biological functions are typically *tougher* to figure out than causal role functions. For one thing, as Lewontin has repeatedly pointed out (e.g., 1990), cognitive functions leave no unambiguous marks on the hominid fossil record and humans have no close living relatives whose homologous psychological capacities might allow inferences about ancestral functioning. In addition, as Stolz and Griffiths (2002) argue, the evolutionary or

'adaptive' problems faced by an organism cannot be specified independently of the organism's capacities (and/or the structures that underlie those capacities). If you didn't know, to take their example, that a given fossil bird had a reinforced beak and skull (like a modern woodpecker), you would be unable to reconstruct its niche (living in a forest), its habits (eating insects living under the bark of trees) or the adaptive problems (getting at the insects) and selection pressures it faced. In the absence of detailed knowledge of what the mind is actually like, speculating about the adaptive problems faced by hominids in the EEA is like speculating about the niche and feeding habits of a headless fossil bird. Thus, EP's strategy of inferring the mind's functional architecture from speculations about its biological function(s) is seriously off track.

2.2 The One-to-one Assumption

The epistemological problems just outlined are quite endemic to adaptationist reasoning about the mind. However, there is a second problem with EP's view of the relation between structure and function: EP assumes that once a psychological function is somehow identified, it is legitimate to postulate a *single* mechanism – a 'module' – that performs that function. As Carruthers puts it:

> …in biology generally, distinct functions predict distinct
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> mechanisms to fulfill those functions….[Hence] one should expect
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> that distinct mental functions – estimating numerosity, predicting
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> the effects of a collision, reasoning about the states of another
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> person, and so on – are likely to be realized in distinct cognitive
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> learning mechanisms…" (this vol. pp.9-10)

This 'one to one' assumption is not a dispensable part of EP methodology. If a single mechanism could subserve many different functions or if a single function required the cooperation of a number of different mechanisms, then the characteristic EP procedure of inferring mechanisms from functions would be undermined. For in that case, there would be many different alternative hypotheses about the mechanisms involved in the performance a given function, and the identification of the function itself would provide no evidence about which of these alternatives was correct. The one-to-one hypothesis avoids this difficulty by assuming that the only possibility is that a distinct mechanism performs each function.

Given the central role played in the EP methodology by the 'one-to-one' assumption, it is then a real problem for EP that this assumption embodies a serious misapprehension about how natural selection works. Far from "characteristically [operating] by 'bolting on' new special-purpose items to the existing repertoire" (Carruthers, this vol., p.9), natural selection usually operates by jury-rigging what is already there to perform new tasks instead of (or in addition to) the old ones. Feathers originally evolved for thermal regulation, and subsequently were exapted for flight and mating displays as well. Vertebrate limbs originally evolved for swimming, and subsequently were fitted for walking, climbing, flying and manipulation. At the genetic level, too, exaptation and multifunctionality are common, both within organisms and across species. The Hox genes that control the development of a chicken's legs and feet, for instance, also control development of its wings. Moreover the self-same genes are responsible (with only very minor changes in sequencing) for limb development in all

tetrapods – wings, claws, paws, flippers, flukes and hands all have the same genetic origins. (Davidson 2001:167-76; Gilbert 2000: 503-21).

Exaptation and multifunctionality are also features of the mind and brain. If a given mechanism M_1 carries out some task, T_1, and in so doing processes information that is relevant to some other task, T_2, then M_1 could well be selected because of its role in performing T_2 in addition to T_1. For example, the processes of object identification may generate information that is relevant to depth perception. If so, those processes may be recruited for both functions and we'd have two functions utilizing a single mechanism. On the other hand, what is intuitively a single task may involve multiple mechanisms cobbled together over time: T_2 may involve M_2 and M_3 in addition to M_1. Depth perception looks like this: mechanisms that are at least partly distinct, both anatomically and phylogenetically, are involved in the processing of the various depth 'cues' such as binocular disparity, occlusion, texture gradients, etc.

The reuse of old materials for new purposes, with all the redundancy and ad hoc interconnectedness that it implies, is characteristic of selection's 'tinkering' mode of operation. Because natural selection typically does not operate by designing new, single-purpose devices to solve new environmental challenges, EP's one-to-one assumption is highly dubious.

Another problem with the one-to-one assumption concerns EP's individuation of functions or tasks. Consider the detection of numerosity. How should we decide whether this is one psychological function subserved by a single module (as Carruthers assumes, this vol., p.X) or several functions subserved by several modules? The detection of numerosity, after all, is actually a highly complex task. It involves (e.g.) object detection

and individuation, which involve (e.g.) depth and edge perception, which involve (e.g.) perception of luminance and color boundaries….etc. Detecting numerosity is a function carried out by the performance of other, simpler functions: functions are nested. They are also shared. Just as the detection of numerosity itself can play a role in higher-level functions (say, performing a task in a psychology experiment), all of the lower-level functions just discussed play roles in the performance of other tasks: depth perception also subserves motion detection; perception of color boundaries subserves depth perception; object individuation subserves object recognition, etc. Given that functions are both nested and shared in this manner, it is hard to see how evolutionary psychologists – relying only on the one-to-one assumption and eschewing the sorts of detailed investigations into neural and cognitive mechanisms described in §2.1 – could have any principled reason for saying that a given function (like the perception of numerosity) is carried out by one module or many. Similarly for face-recognition, cheater detection, and the various other capacities that are the focus of EP theorizing.

The observations in §§ 2.1 and 2.2 clearly undermine EP's assumptions that mechanisms or modules and functions correspond in a neat 1:1 manner and that as a result, the existence of modules can be inferred from a specification of the tasks the mind performs. Of course, one could read EP as simply *stipulating* a notion of 'module' such that each function is *ipso facto* performed by one and only one module. But such a reading of EP's structural hypotheses trivializes them. In addition, this 'thin' interpretation of what a module is is inconsistent with the fact that the modules postulated in EP are virtually always assumed to have other properties, such as being independent

targets of selection, being independently disruptable, being informationally encapsulated, and so on. (See §4.)

2.3 The Role of Learning and Development

Another crucial limitation of EP's methodology is its misunderstanding of the role of learning and development in shaping the mature mind. It's not that evolutionary psychologists assign no role at all to learning and development. It is rather that they think of these processes as strongly genetically pre-specified. Not only does this 'preformationist' picture have little empirical support, it engenders a crucial misspecification in the EP literature of what stands in need of adaptive explanation.

Evolutionary psychologists take some behavior or capacity possessed by mature humans – say, mate preferences, or cheater detection, or the desire to rape – and then proceed to give an adaptive explanation of the postulated mechanism underlying that behavior or capacity (cf., e.g., Thornhill and Thornhill, 1987, 1992 on rape; Wright, 1994 on family relations). But if learning plays an important role in the acquisition of these mechanisms or behaviors, then what really needs adaptive explanation is *the processes underlying the development of those mechanisms*.

Admittedly, some evolutionary psychologists do see their task as involving the explanation of development – cf. Carruthers' emphasis on "evolved learning mechanisms" as giving rise to various modules (p.XX, this vol.). However, the assumption here seems to be that if some competence (and the module, M, underlying it) are adaptations built by natural selection, then either (i) the unfolding of M is directly genetically pre-specified; or (ii) M is produced by a 'learning module,' L, which is itself built by the genes and tightly constrained to produce M as its output. On this view, the

relationship between L and M is very direct: to the extent that experience plays any role at all in the development of M, it merely serves to 'trigger' a cascade of effects in L, the outcome of which is tightly genetically constrained.

However, there are a number of serious flaws in this reasoning, even assuming that a given processing module M in the adult mind is indeed an adaptation built by natural selection. First, as a number of psychologists, biologists and philosophers of biology have emphasized, adaptive traits may be 'coded for' in the environment. (Cf. Oyama, 1985, Sterelny and Griffiths, 1999, Sterelny, 2003.) That is, instead of building M into the genes (either directly or indirectly via learning mechanism L), natural selection may have given us dispositions to *construct an environment E* in which M would arise as a result of learning and/or other developmental mechanisms which are *not* genetically determined to produce M. For example, rather than building in a 'folk psychology' module, evolution may have given us dispositions to create the kinds of social and familial environments in which children's generalized developmental and learning abilities enable them to acquire knowledge of other minds.

A second problem here concerns the relation of current evidence from neurobiology and genetics to EP's assumption that modules like M or L are "innate or innately channeled." (Carruthers, this vol. p. X.) Several writers (e.g., Bates *et al*., 1998) have advanced a simple counting argument against the notion that numerous cognitive modules (with all their detailed representations and complex algorithms) are genetically specified. Human beings have approximately 30,000–70,000 genes. (Venter *et al*. 2001; Shouse 2002.) By contrast, there are an estimated 10^{14} synaptic connections in the brain.

Thus, it is argued, there are too few genes by many orders of magnitude to code for or specify even a small portion of these connections.

We find this argument suggestive but not decisive. The role of regulatory genes and networks in governing the expression of structural genes probably generates many more combinatorial possibilities than the figure of 30,000 genes suggests. Still, the counting argument does draw attention to the need for evolutionary psychologists to explain, consistently with what is known about brain development, how cognitive modules could be genetically specified. This, we think, is a non-trivial task, especially vis à vis the cerebral cortex, which is known to play a central role in the sorts of high-level cognitive tasks (like language acquisition, cheater detection, theory of mind, etc.) that figure in EP theorizing. For while the gross architectural features of the cortex do appear to be genetically specified, there is considerable evidence that the cortex is in other respects initially relatively undifferentiated and equipotent. In particular, the patterns of synaptic and dendritic connections that develop in different cortical areas – and presumably correspond to the representations (of syntax, folk psychology, etc.) which EP's modules contain – are very heavily influenced by sensory inputs, and influenced in a way that the evolutionary psychologist's 'triggering' metaphor seems ill equipped to capture. Indeed, many areas of cortex have the capacity to acquire fundamentally different sorts of representations depending on experience. For instance, the cortical areas normally devoted to visual processing in sighted subjects are used for tactile tasks, such as Braille reading, in congenitally blind subjects, and auditory cortex is recruited for processing sign language in deaf subjects. (E.g., Büchel, Price and Friston, 1998; Nishimura *et al.*, 1999.) This phenomenon of 'cross-modal plasticity' makes it very hard

to see how the cortex could contain innate representations specialized for specific cognitive or learning tasks, and undermines EP's notion that the development of cognitive modules like M or L is genetically driven. We think that until we hear more about the ways in which the genetic and regulatory mechanisms needed to build the mental modules postulated in EP actually work, we are entitled to view EP's developmental story – or, really, its lack of such a story – with suspicion.

2.4 Non-Darwinian Traits

Such suspicions are reinforced by consideration of a final shortcoming of EP's reverse engineering strategy, namely, its blindness to the fact that many psychological traits may not be susceptible of direct Darwinian explanation at all. First, while we grant Carruthers' point (this vol., pp.7-8) that the entire mind is unlikely to be the product of drift or some other non-selective process, it's by no means impossible that particular psychological mechanisms might be the results of such processes. Developmental, allometric and physio-chemical factors are all known to play significant roles in neural functioning and organization, and may well turn out to be responsible for some psychological traits as well.

Alternatively, some psychological mechanisms might be 'spandrels' in the sense of Gould and Lewontin (1979). That is, they might be lucky byproducts of traits that evolved for other purposes. There's evidence, for instance, that our capacity to organize continuous acoustical signals into linguistically-relevant segments (phonemes) is a byproduct of the way that mammalian brains happen to have evolved to process auditory information. Of course, byproducts that happen to be advantageous may themselves be subject to positive selection pressure – they may become 'secondary adaptations.' But the

possibilities that psychological mechanisms are spandrels or mere secondary adaptations undermine, in different ways, EP's assumption that each psychological mechanism is built to order to solve a distinct adaptive problem. The spandrels possibility puts into doubt EP's assumption that modules are *optimal* or near-optimal solutions to adaptive problems: a turtle's fins may be optimized for propelling a heavy body through water, but they are far from optimal means of crossing the sand at nesting time. And the possibility that some mental mechanisms are exaptations further undermines EP's one-to-one assumption, discussed in §2.2: complex exaptations (like, arguably, the human capacity for language or cheater detection) are often cobbled together from multiple mechanisms that are designed (and still used) for other purposes. While one can certainly *call* such complex secondary adaptations *single* mechanisms or modules, it's unclear that they can be attributed the other features commonly ascribed to modules, such as informational encapsulation or independent disruptability. (See below, §§4.3, 4.4.)

3. The Mind as A System of Modules

EP claims not just that the mind contains various mental modules, but that it is a *system* of modules. In this section, we examine the arguments for this claim. (We assume here, for the sake of argument, that the notion of a 'module' is relatively clear. This assumption will be criticized in §4.)

The main argument for the claim that the mind is a system of modules is originally due to John Tooby and Leda Cosmides. They claim that domain-specific modules would inevitably be selected for because relatively content-independent (or

general-purpose) architectures are in principle not viable objects of selection (e.g., Cosmides and Tooby, 1992b, 1994; Tooby and Cosmides, 1990, 1992; see also Samuels, 1998 for a forceful statement of EP's 'massive modularity' hypothesis). There are two arguments given for this claim. First, general learning mechanisms face the 'Frame Problem.' Unless the factors relevant to a problem are delineated in advance, general-purpose inference mechanisms face a massive combinatorial explosion – and their owners get eaten before they can reproduce. (See §3.1.) Secondly, Chomsky's poverty of the stimulus argument for the existence of a language-learning module is generalized to show that general-purpose inference is ineffective in the face of *any* learning problem. For one thing, there will always be more hypotheses compatible with the available data than the learner can effectively test. For another thing, testing is itself problematic. There are no domain-neutral criteria for success: evaluating foraging strategies involves different measures from those used to test hypotheses about cheaters. Worse, there are some hypotheses and strategies that an individual cannot evaluate at all – mate selection strategies would be an example, assuming, of course, that the appropriate measure here is inclusive fitness. (See §3.2) The upshot is that hominids equipped only with general-purpose inference or learning mechanisms wouldn't have survived in the EEA. Additional constraints on learning mechanisms are clearly needed, and those are what modular architectures supply.

3.1 Combinatorial Explosion and the 'Frame Problem'

Fodor (1983) maintained that many or most cognitive (or 'central') processes are non-modular, since reasoning, deliberation and planning etc. must potentially have access to everything an agent knows. He recognized that this meant that such non-modular

processes are subject to the so-called 'frame problem' – the problem of specifying what information is relevant to which problem – and for this reason, speculated that they would prove unamenable to cognitive-scientific investigation. The pessimism of evolutionary psychologists is deeper even than Fodor's: they view the frame problem not just as an obstacle to *theorizing about* central processors, but rather to their very existence! Carruthers (this vol. p.X), for instance, argues that "any processor which had to access the full set of the agent's background beliefs…would be faced with an unmanageable combinatorial explosion" and hence concludes that "the mind…consist[s] of a set of processing systems which…operate in isolation from most of the information which is available elsewhere in the mind." EP thus (dis)solves the frame problem by assuming that the processes underlying decision-making and behavior are modular: they neither have nor need access to the bulk of the agent's beliefs and desires.

Whether this is a satisfactory solution to the frame problem depends on what one takes that problem to be. *If* human reasoning, deliberation and planning processes can generate satisfactory decisions and behavior without access to large numbers of the agent's beliefs and desires, then this will indeed be an important point in favor of the modularist picture. However, it seems plain that in many cases, reasoning etc. *cannot* issue in even minimally satisfactory decisions and behaviors without such access – consider, for instance, the range of factors bearing on a decision to cooperate with a conspecific. If this is so, however, then EP's claim to have solved the frame problem is undermined, and the modularist must confront the question of how our processes of reasoning, deliberation and planning *could* have access to so many and so varied of our background beliefs and desires. Presumably, evolutionary psychologists cannot invoke a

single, hardwired 'Decision Making Module' here, for natural selection clearly cannot anticipate all the decisions we potentially face in a lifetime; moreover, the beliefs and desires that are relevant to these decisions vary with context and hence cannot be prespecified. Suppose that it is instead suggested that a *group* of encapsulated modules collaborate in the planning and execution of complicated actions. In that case, we must ask how their operations are coordinated. There seem to be two options. One is that there is a fixed hierarchy of modules, such that each module sends its outputs to the next one up in the hierarchy, and so on, until a behavioral command is outputted. Alternatively, there is some kind of 'Module Integration Module' (what Samuels, Stich and Tremoulet, 1999 unironically call a "Resource Allocation Module") which takes the outputs of various lower-level modules, evaluates them, and issues in the same behavioral instruction – Carruthers (this vol., §6) proposes that "an existing module…the natural-language faculty" (p.X) performs this integrative task.

But neither of these alternatives is plausible. An evolved, hard-wired hierarchy of modules is vulnerable to the same objections as the Decision Making Module: our behaviors are simply too complex, and the mental processes giving rise to them too varied, for the frame problem to be solved by a pre-specified hierarchy . This leaves us with the idea of a Module Integration Module, which takes in the deliverances of all the other modules whose computations are potentially relevant to a given problem and decides what to do with them. But a 'module' that can (i) assess which of the plethora of modular outputs are important in a given context and (ii) decide what outcome is desirable and then (iii) figure out which behaviors (and in what order) will result in that outcome *isn't a module (in the EP sense) at all*! Instead, it's functionally equivalent to

Fodor's Central Processor, and, assuming that the frame problem and combinatorial problems are real problems, it raises them all over again. As soon as one looks in detail at how a massively modular mind is supposed to work, one sees that the frame problem is not an argument *for* the theory that the mind is massively modular; instead, it's an argument *against* that thesis! [1]

3.2 The Argument from the Poverty of the Stimulus

Suppose that a poverty of the stimulus argument has convinced us that some hypothesis or skill which people acquire could not have been learned just from the evidence available. This shows us that additional constraints, not present in the evidence, are required for successful learning. Evolutionary psychologists (like other proponents of poverty of the stimulus arguments) are quick to assume that the constraints in question must be (i) representational, (ii) cognitively sophisticated and (iii) specific to various common-sense domains or subject matters. Thus, for instance, we are told that the necessary constraints are 'theories' of various sorts (e.g., universal grammar, theory of mind). And because the content of these theories so far outruns the available data, this view suggests in turn (iv) that the needed constraints on learning are embodied in innately-specified modules (Language Acquisition Devices, Theory of Mind modules, etc.).

However, this picture itself outruns what is warranted by the poverty of the stimulus argument. For that argument indicates only that some constraints are needed, not

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[1] We concede that our discussion does not even begin to explain how human beings manage to take account of a wide range of background information and act flexibly and reasonably. But modular theories are in far worse shape. They not only fail to provide a positive account of how the problem is solved, but also make assumptions that are inconsistent with the fact that we do (somehow) solve the problem. Alternatively, and to the extent that they do attempt to accommodate this fact, they are forced to abandon basic commitments of the modular account.

what *kinds* of constraints those are. Thus, learning may be subserved by other types of constraint in addition to (or instead of) the sophisticated representational constraints postulated in EP. There might, for instance, be perceptual biases of various sorts, or dispositions to direct our attention to certain kinds of stimuli, or facts about our reward structures that encourage certain sorts of behavior rather than others. For example, there is evidence that subcortical mechanisms preferentially direct infants' visual attention to objects that fit a loosely face-like template, and that reward mechanisms release chemicals that make infants feel good when attending to such stimuli. (Johnson, 1997) By themselves, these mechanisms are incapable of generating the full range of adult face-recognition behavior. However, they do help in reducing the underdetermination problem faced by the child (why focus on faces rather than elbows? why focus on eyes rather than chins?), and the preferential looking and attending that they produce may result in the gradual construction of cortical circuits that behave like a 'face recognition module.'

Other possible constraints are developmental or architectural. Chronotopic factors governing the timing of different aspects of development can reduce underdetermination by guiding the *sequencing* of various learning tasks: learning to the grammar of a language is easier if you already have a representation of its phonemes, for example. In addition, although the detailed pattern of synaptic connections that develops in the cortex is experience-dependent, the gross architecture of the cortex (e.g., different areas' characteristic laminar structures and basic circuitry types) may well be genetically specified (cf. §2.3). These architectural features do not themselves amount to innate representations or modules, yet they may help the brain to solve learning problems by biasing certain areas to assume some tasks rather than others, or encouraging certain sorts

of representations rather than others to develop in response to sensory input. As these examples show, it is a mistake to suppose, as evolutionary psychologists frequently do, that the only two possibilities are either a completely unconstrained, general purpose learner or a heavily modular learner pre-equipped with large bodies of domain specific knowledge.

One final point deserves to be made about EP's claim that the mind is a 'system of modules.' Both of the arguments discussed in this section are arguments for a very strong version of the modularity hypothesis, namely, that the mind contains *nothing but* modules. As already indicated, we don't think that the evidence for this 'massively modular' view of the mind is at all compelling. However, there is also a more 'Modest' modularity hypothesis to the effect that the mind contains *some* modules. (E.g., Fodor's (1983) modularity hypothesis was Modest: it postulated both modular sensory mechanisms and non-modular central processing mechanisms. Modesty also embraces the possibility that some cognitive (as opposed to sensory) processing is modular too.) Our discussion so far leaves it open that some kind of Modest modularity thesis is correct. In the next section, however, we argue that the notion of a module, as deployed in EP, is fundamentally unclear. Thus, while the mind may indeed contain some 'modules' (in some sense of that word), we will see in §4 that even advocates of Modest modularity need to clarify considerably what their thesis amounts to.

4. In Search of Mental Modules

We turn now to the question of what modules are. We argue that the various different criteria used for modularity in the EP literature are far from co-extensive and

thus lead to quite different notions of a 'module.' We also emphasize that these different

modularity claims require (but often do not get) different sorts of supporting evidence.

We conclude that EP's widespread failure to recognize these points both weakens its case

for the modularity of mind and undermines the status of the specific cognitive modules it

postulates.

4.1 Modularity and Neural Specificity

As Carruthers notes (§1.1, pp.2-3) and as we will be lamenting in this section, the

meaning of term 'module' in EP is highly elastic. However, one negative point about

EP's notion of modularity has been foreshadowed in previous sections: it bears little

relation to the neuroscientist's notion of *neural specificity*. This is the idea, first, that

different brain regions are (relatively) specialized to different tasks. In most people, for

instance, the left hemisphere is dominant in language processing – word production, e.g.,

is more or less localized to Broca's area, Wernicke's area and the left thalamus. (Indefrey

and Levelt, 2000: 854) Secondly, the idea of neural specificity embraces the fact that the

representations and computations that are used in different brain regions and for different

tasks may be quite diverse. For example, the perception of an object's color involves the

representation of its spectral properties by the three retinal cone types, adjusted so as to

compensate for properties of the ambient light (Wandell, 2000), whereas perception of

sounds involves the representation of low-level acoustical features as onset time, pitch

and location, followed by the computation of higher-order properties such as timbre,

resulting ultimately in the representation of items of speech, music, or other types of

noise. (Shamma, 2000)

Now, if all that were meant by EP's claims that perceptual and cognitive processing and mechanisms are 'domain specific' or 'modular' were that such processes and mechanisms are neurally localized and involve different kinds of computations over different kinds of representations, we would readily agree. Not even the most rabid anti-modularist doubts, for example, that retinal cones are ineffective at extracting acoustical information. However, as we have already suggested, adherents of EP generally have something much stronger in mind than this.

As evidence for this, consider first the fact that the neural specialization described above is typically *relative,* rather than absolute. Cells in a certain area may respond especially strongly to certain kinds of inputs or may be particularly active in the execution of a certain task. But as neural imaging data are increasingly making clear, they will typically also respond, though less vigorously, to many other inputs and task demands. Andersen et al. (2000), for example, give evidence that the posterior parietal cortex, classically thought to be specialized for attention and spatial awareness, is also involved in the planning of goal-directed behavior. Similarly, DeAngelis, Cumming and Newsome (2000) argue that cortical area MT, normally held to be highly specialized for motion detection, is also implicated in the perception of stereoscopic depth.

Just as the same brain areas may subserve different tasks, many tasks that common sense might count as unitary can involve activation of numerous different brain regions. Face recognition, for example, involves not only the areas in the fusiform gyrus that are cited in lesion and dissociation studies, but also the parahippocampal gyrus, the hippocampus, the superior temporal sulcus, the amygdala and the insula. (McCarthy, 2000) Likewise the production of verbs involves areas in the left frontal cortex, anterior

cingulate, posterior temporal lobe and right cerebellum. (Posner and Raichle, 1994: 120).

At the neural level, then, tasks like recognizing a face or producing a spoken word are

performed by a 'single mechanism' only in a very attenuated and task-relative sense.

This sharing of tasks by the same neural areas and the distribution of tasks over

numerous different areas contrasts strongly with EP's talk of distinct modules devoted to

distinct cognitive and perceptual tasks. Hence, evolutionary psychologists' claims about

'domain specific' or 'dedicated' modules should not be confused with the facts about

neural specificity just described. But if that's the case, what *does* EP's talk of 'dedicated'

or 'domain specific' processing amount to?

Evolutionary psychologists answer that one needs to distinguish between what

Marr (1982) called 'implementational level' details on the one hand, and theories at the

'computational level' on the other (cf. Griffiths, forthcoming). Since their theories are at

the psychological or computational level, we should not expect the modules they

postulate to be reflected in the nitty gritty of neural organization. As Cosmides and

Tooby put it, EP "is more closely allied with the cognitive level of explanation than with

any other level of proximate causation." (1987:284)

But while the urge to theorize at one level of description while ignoring

constraints from other levels is endemic to cognitive science, we think that it is a mistake.

No psychologist should ignore the neurosciences because psychological theories must be

implementable in brains and, as is increasingly becoming apparent, this constraint is an

extremely strong one. It is doubly a mistake for evolutionary psychologists to neglect

facts about how psychological tasks are performed by the brain. First, as §2.1 made clear,

the individuation of psychological functions must be constrained by implementational

information. Second, as §3.1 urged, one cannot usefully theorize about how natural

selection operates on the mind and brain while neglecting implementational issues. Third,

a sharp psychological/implementational divide undermines one of EP's central sources of

evidence: if EP's modules have nothing to do with the brain, it is hard to see the

relevance of the sorts of neuroscientific data (about localization, dissociations, etc.) that

are frequently cited in the EP literature (cf. Carruthers, this vol., §§4.2, 4.3; Pinker,

1999). Most importantly, though, neglecting implementational constraints threatens to

leach EP's notions of modularity and task-specificity of any real content. If the notion of

a module is not tied to claims about neural specificity, what does it amount to? In what

follows, we review several features that have been ascribed to modules and examine their

interrelations.

4.2 Modularity and Dissociability

One feature that is often ascribed to modules in EP is dissociability or independent

disruptability, the idea being that if two modules are distinct, then it should be possible

(at least in principle) to interfere with the operation of each one without affecting the

operation of other.[2] As we have already observed (§2.1), EP lacks an intrinsic

characterization of modules that would allow one to determine directly whether one

independently-identified mechanism has dissociated from another. Instead, modules are

characterized functionally, in terms of the tasks that they are assumed to perform, and the

dissociations that are actually observed are dissociations between *tasks* (e.g., between

production of words and comprehension of grammatical sentences). It is these

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[2] Dissociations are often thought to particularly compelling evidence of independent modules when there is a 'double dissociation' of tasks, that is, when a pair of individuals is observed, one of whom can perform task A but not task B, and the other of whom can perform B but not A.

dissociations among tasks that are taken to be evidence for the existence of independent modules. Thus, Carruthers (this vol., §4.2) and Pinker (1994: 49ff), for instance, argue that the double dissociation between general cognitive tasks and language production and comprehension tasks seen in subjects with Specific Language Impairment and Williams syndrome is strong evidence that there is a task-specific mental module underlying language.

While the evidential significance of dissociations is a complicated subject to which we cannot do justice here, such inferences are far more problematic than is generally appreciated.[3] First, there are a number of intuitively non-modular architectures that can give rise to double dissociations among tasks (cf. Shallice, 1988: 245ff). Second, it is crucial to distinguish between dissociations arising from developmental disorders and dissociations resulting from injuries to (or experimental manipulations of) adult brains. The former bear on mechanisms of learning or development, and the latter on mature psychological competences. Third, inferences from a double dissociation of capacities to the distinctness of modules generally require additional empirical assumptions, such as (i) a 'universality' assumption to the effect that both normal and abnormal subjects share a cognitive architecture (excluding the damaged modules in abnormals); (ii) a 'subtraction' assumption to the effect that brain damage only removes modules or the connections between them and it does not engender any significant neural reorganization; and (iii) various 'gating' assumptions about whether the destruction of one or all connections between modules involved in a task is necessary for disruption of the task. (Cf. Shallice, 1988: 218ff.; Glymour, 2001:135-6, 143-4.)

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[3] This issue is the subject of considerable debate. See Shallice, 1988 and Glymour, 2001 for surveys.

These assumptions are empirically questionable, especially when the dissociations in question are developmental or genetic in origin. First, subjects with genetic abnormalities (or childhood brain injuries) are likely to differ from normal subjects in many different ways. Secondly, incapacities appearing early in childhood are known to call forth compensatory psychological strategies and substantial neural reorganization. Hence, and contrary to what Carruthers and Pinker imply, it is extremely unlikely that subjects with Specific Landguage Impairment differ from normal subjects only in having impaired language function. Instead, as many empirical studies attest, such subjects have numerous other cognitive and perceptual deficits as well.[4] Thus, the cleanness of the dissociation between language and general cognitive abilities is undercut – as is EP's inference from that dissociation to the existence of distinct modules underlying those abilities.

We conclude this section by again acknowledging that there is a very 'thin' notion of module such that, given certain other assumptions, a double dissociation entails modularity (in that sense). For example, if we simply assume that a distinct module underlies each distinct capacity (c.f. the 'One-to-one' assumption discussed in §2.2), with all normal subjects sharing the same architecture, and if we count a dissociation of capacities in any two people as indicating that those capacities are distinct (across all subjects), then we have an unproblematic inference from dissociability to distinctness of modules. However, this pretty inference is bought at the cost of a not-very-interesting notion of 'module.' As soon as we begin to invest modules with other, 'thicker' properties – like informational encapsulation or independent evolvability (§§4.3-4.4) –

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[4] See, e.g., Vargha-Khadem and Passingham 1990, , Anderson, Brown & Tallal 1993, Merzenich et al. 1996.

the inference becomes far less compelling, as these properties do not necessarily apply to modules as distinguished by the dissociability criterion.

4.3 Modules and Encapsulation

Modules are also often said to be *informationally encapsulated* in the sense that other psychological systems have access only to the information that is the output of the module; the processing that goes on within it is not accessible to, or influenced by, information or processes in other parts of the mind (Fodor, 1983). However, it is not clear how useful this feature is in picking out distinct cognitive mechanisms. First, informational encapsulation is often a relative, rather than an all-or-nothing matter. It's plausible that some brain or psychological mechanisms may be completely informationally isolated from *some* other mechanisms (in the sense that there are no circumstances in which mechanism A is internally influenced by mechanism B). But many if not virtually all mechanisms are influenced in their internal operations by *some* other mechanisms – or at least this is true if we don't trivialize the notion of an 'internal operation' (see below) . Relatedly, informational encapsulation often seems to be task-relative. Whether mechanism or brain region A is influenced in its internal processing by information or processing in mechanism or region B may vary depending on the tasks A and B are engaged in.

As an illustration of these points, consider the role of attention in many psychological processes. There is evidence that although low-level visual processing, such as occurs in the primary visual area V1, is often relatively encapsulated, it can be modified by higher-level processes involving visual attention, which occur in other neural regions. (Luck and Hillyard, 2000.) This kind of result raises familiar issues about EP's

individuation of tasks: are the processes in V1 performing *different* tasks or functions depending on whether attention is involved? It also undermines the usefulness of the encapsulation criterion for modularity. Does the fact that the processing of a visual stimulus by V1 is altered depending on whether subjects pay attention to that stimulus show that V1 is unencapsulated with respect to tasks that involve attention but encapsulated with respect to other tasks not involving attention? If so, are there *two* modules associated with V1, one operative when attention is involved and the other when it's not? Peter Carruthers (private communication) suggests that if attention sometimes influences processing in V1, then attention should count as an *input* to V1, not an influence on its internal processing. Hence, he argues, processing in V1 *is* encapsulated after all. Our response is that the notion of informational encapsulation only makes sense if there is some basis for distinguishing between an inputs to a module and processes that influence the internal operation of the module, for it's the latter kind of influence that claims of encapsulation deny. If *any* informational influence on the internal processing of a mechanism can be reconceptualized as an input to that mechanism, and if influence *via* input is consistent with encapsulation, then the notion of informational encapsulation is vacuous.

We have already argued (§3.1) that dissociability is a dubious criterion for modularity. It's also of little help in the present connection, for contrary to what is often assumed, encapsulation bears no simple connection to dissociability. Consider the well-known diagram of the macaque visual system due to van Essen.

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Insert figure here

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We see here some 32 cortical 'areas,' as well as some subcortical areas. These areas are differentially sensitive to different sorts of stimuli and/or specialized for different sorts of processing (although typically not in an all-or-nothing fashion). Assuming that they are also susceptible of at least some degree of dissociation, we would appear to have (by the 1:1 and dissociability criteria) as many as 32 distinct modules depicted in this diagram. However, these cortical areas are also highly interconnected: van Essen traced 197 linkages (= roughly 40% of the $32 \times 31/2 \approx 500$ linkages that are in principle possible). Most of these linkages appear to be reciprocal, indicating that there is no simple sequential or hierarchical direction of information flow among the postulated modules; instead, each module talks to (and is talked to by) numerous others at numerous different stages of visual processing. This raises serious questions about how the dissociability criterion is supposed to line up with the encapsulation criterion for modularity and how the latter criterion is to be interpreted. Is the sort of interconnectedness found in Figure 1 consistent with these areas being distinct modules? If so, it looks as though encapsulation (and perhaps modularity as well) come in degrees, rather than being all-or-nothing matters, in which case we need (i) some measure of degree of encapsulation and (ii) a theory about how this bears on judgments of modularity. If, on the other hand, modularists prefer to say that this degree of interconnectedness is *in*consistent with the idea that the areas form distinct modules, then it follows that distinctness of function and dissociability are not reliable criteria for individuating modules.

4.4 Modules and Independent Evolvability

Still another criterion for modularity is that modules are *independent targets of natural selection*. That is, selection must be able to change each of them independently of the others. This feature of modules is presupposed by EP's characteristic view of organisms as confronting a large collection of separate adaptive problems, each of which gets an independent evolved module by way of solution.

The independent evolvability criterion, however, is again problematic. For if a trait is to be an independent target of selection, it must be what Sterelny and Griffiths (1999) call a 'mosaic' rather than a 'connected' trait.[5] To use one of their examples, skin color is a plausible candidate for a mosaic trait because "it can evolve with relatively little change in the rest of the organism" (1999: 320). By contrast, having two lungs is a connected trait: you can't change this trait without changing a great deal else in the organism because lung number is influenced by the genes and developmental mechanisms that govern the bilateral symmetry of the organism. Hence, natural selection can only influence lung number by influencing these genes and developmental mechanisms, and this in turn would affect many other phenotypical features. Since lung number is not an independent target of selection – since it is part of the "bigger package" (Sterelny and Griffiths, 1999:320) – it would be a mistake to try to give an adaptive explanation of our having two lungs *simpliciter*. Instead, what needs to be explained is the evolution of bilateral symmetry.

Evolutionary psychologists assume that modules are independently evolvable, that is, that they are mosaic traits (like skin color) rather than connected traits (like having two lungs). However, there is evidence that many human cognitive abilities may be

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[5] Gilbert (2000:693ff.) calls this a requirement of 'modularity' – not to be confused with the cognitive modularity that concerns EP.

connected rather than mosaic traits. For example, Finlay and Darlington (1995) show that brain structures change in size across species in a highly coordinated and predictable manner: homologous structures enlarge at different but stable rates when compared to overall brain size. It is thought that these regularities reflect deeply entrenched developmental constraints on the order of neurogenesis, suggesting that while natural selection can increase (or decrease) the size of the brain as a whole, the sizes of particular cortical regions cannot be changed independently, even in response to specific and pressing selective problems. Thus, natural selection may *not* be able to 'fine tune' the cortical regions responsible for (say) cheater detection or the perception of numerosity independently of the (allegedly) distinct cognitive modules that underlie other cognitive capacities like face recognition or language.

A further question concerns the relationship between the independent evolvability criterion and the other features of modules discussed above. We submit that there is no connection between these properties: independent evolvability does not entail, and is not entailed by, either independent disruptability or informational encapsulation. Indeed, it is a consequence of the arguments presented in this paper that there is *no connection whatsoever* between *any* of the properties – independent disruptability, informational encapsulation, innateness, independent evolvability – that are commonly ascribed to modules.

This is important, because it undermines a pattern of argument that is highly prevalent in the EP literature. Evolutionary psychologists provide evidence for the existence of a module in *some* sense (e.g. in the sense that performance on two tasks dissociates) and then go on to assume (without argument) that the module in question

satisfies the other criteria discussed above as well. Thus, they slide from hypotheses of modularity in one of the various 'thin' senses we have discussed in this paper to claims about the existence of modules in a much 'thicker' and more substantive sense.

This slide is wholly unjustified. As an illustration, consider Cosmides and Tooby's (1992a) well-known experiments on the Wason selection task and their subsequent hypothesis of a 'cheater detection' module. Prima facie, what their experimental results show is that people behave differently (and in some respects more reliably) when dealing with conditionals framed as rules governing social exchange than they do when dealing with conditionals with other contents. Even if we accept that these results establish differential performance on cheater-detection tasks *tout court* (and not just those that involve conditionals – itself a big jump), they do *not* constitute evidence for the existence of a distinct cheater-detection module in any more robust sense. That is, they do not even remotely suggest that cheater detection is subserved by an independently-disruptable, informationally-encapsulated psychological mechanism which has been subject to distinct selection pressures and which as a consequence is genetically specified or 'innate' etc. It is of course conceivable, although (we think) unlikely, that a cheater detection module possessing all these features exists; our point is that Tooby and Cosmides' experiment provides no evidence that it does.

Our overall argument in §4 can be put as follows. Interpreted one way (as involving a sufficiently 'thin' conception of a module), EP's claims about modularity amount to little more than redescriptions of certain experimental results or evolutionary psychologists' functional speculations. So construed, claims about the existence of 'mental modules' are uncontroversial – but also uninteresting. Modularity claims become

more contentful and more interesting as the 'thin' notion of a module is extended to include the other properties described above. However, not only is the evidence that would support such extensions is rarely provided, what we know about the brain makes it unlikely that there could be 'thick' mental modules for the sorts of high-level cognitive capacities that are EP's main theoretical focus.

5. Conclusion

Much of the appeal of EP derives from the fact that it appears to provide a way of 'biologizing' cognitive science, with evolutionary considerations supposedly providing powerful additional constraints on psychological theorizing. We think that this appearance is misleading. Evolutionary psychologists largely ignore the biological evidence that has the strongest scientific credentials and is most directly relevant to their claims about psychological mechanisms. This includes not only evidence from neurobiology, genetics, and developmental biology, but also any evidence from evolutionary biology, ethology and population genetics that threatens to undermine their armchair adaptationism. Their methods assume, wrongly, that one can usefully speculate about biological and psychological functions in ignorance of information about structure, genes, and development. Their central theoretical concept – modularity – is left fundamentally unclear. And their picture of the mind as 'massively modular' fails to do justice to many of its most important features, such as its capacity to engage in long-range planning and its remarkable cognitive and behavioral flexibility.

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