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Get Over: Massive Modularity

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There's evolutionary psychology, and there's Evolutionary Psychology. The former is a *field of inquiry*, a loose confederation of research programs that vary widely in theoretical and methodological commitments and that are federated only by a commitment to "adopting an evolutionary perspective on human behavior and psychology" (Barrett, et al. 2002, p. 1). The latter, Evolutionary Psychology, is a specific *doctrinaire research program* within this field of inquiry, a central doctrine of which is the so-called *massive modularity hypothesis* (MMH). The

MMH rejects the view that learning results from the operation of a few *domain-general* mechanisms "like operant conditioning, social learning, and trial-and-error induction," which operate in all learning "domains" (Tooby & Cosmides 1992, p. 39). Rather, according to the MMH, the mind consists of a multitude of *domain-specific* "modules or mental organs, each with a specialized design that makes it an expert in one arena of interaction with the world" (Pinker 1997, p. 21). Being domain specific, each module is activated by, and only by, mental representations of the problem(s) in its area of expertise (Buss 1995, p. 6). When activated, each module operates solely with its own innate "set of procedures, formats, and representational primitives closely tailored to the demands of its targeted family of problems" to generate behavioral solutions to the problem(s) in its domain (Tooby & Cosmides 1995, p. xiv).

The principal theoretical argument for massive modularity has been this: Like the human body, the human mind has evolved under selection. Accordingly, just as the body contains distinct morphological adaptations that evolved to perform distinct physiological functions, the mind must contain distinct "mental organs," which evolved to solve a Vast Number of adaptive problems that required distinct behavioral solutions (Symons 1992, p. 142). Since the behavioral solution to any one of these problems wouldn't have transferred to any of the other problems (since what you do to avoid inedible flora, for example, won't help you form a social alliance), each adaptive problem would have selected for its own specialized cognitive adaptation. Thus, "our cognitive architecture resembles a confederation of hundreds or thousands of functionally dedicated computers (often called modules)" (Tooby & Cosmides 1995, p. xiii).

This argument has converted many to the "high church" of Evolutionary Psychology, and churchgoers boast compelling empirical evidence for certain modules. But a significant number of laborers in the field of evolutionary psychology have questioned both the empirical evidence and the adaptationist rationale for massive modularity. David Over's *Evolution and the Psychology of Thinking: The Debate* brings both sides of this debate to a single volume of collected essays. It's an interesting and useful collection, which addresses several of the central issues concerning modularity. It's also nicely balanced, containing three essays by "high-church" deacons defending the faith, three essays by infidels, and one essay that makes an ecumenical effort to straddle the fence. These features make it essential reading for those interested in the evolution of cognition.

Several chapters in the volume discuss Cosmides' so-called *cheater-detection module*, the postulated module for which Evolutionary Psychologists have claimed the strongest empirical evidence. The cheater-detection module is purportedly dedicated to detecting cheaters in social exchanges, those non-reciprocators who accept the benefit of a social exchange without paying the requisite cost or providing a benefit in return, and it allegedly evolved to save us the fitness costs of doing business with Rat Bastards (see Cosmides 1989; Cosmides & Tooby 1992). The evidence for the cheater-detection module derives entirely from studies with the Wason selection task, in which subjects are given a conditional, *if P, then Q*, together with four two-sided cards that contain information on one side about the truth value of *P* and on the other side about the truth value of *Q*. Subjects are allowed to see only one side of the cards (displaying *P*, *not-P*, *Q*, and *not-Q*), and they are instructed to turn over those cards necessary in order to determine whether the conditional is true. The presupposition is that the *logically correct* solution is always to turn over the cards displaying *P* and *not-Q*, since only they can contain the conjunction of *P* with *not-Q* and only that conjunction falsifies the conditional.

An allegedly robust result of selection tasks is the so-called *content effect:* Selection of the correct cards appears to vary as a function of what the conditionals *are about*. For example, when subjects are given four cards displaying *E*, *K*, *4*, and *7* and are instructed to select the cards necessary to determine whether "If a card has a vowel on one side, then it has an even number on the other" is true, only about 10 percent choose the *E* and *7* cards (the *P* and *not-Q* cards). In contrast, when given the conditional "If a person is drinking beer, then they must be at least twenty-one years old" and four cards displaying *drinking beer*, *drinking coke*, *twenty-five years old*, and *sixteen years old*, about 75 percent of subjects choose the *drinking beer* and *sixteen years old* cards. If subjects apply domain-general logical principles to solve these problems, Evolutionary Psychologists argue, they should perform equally well on both problems. The fact that there is a performance difference thus appears to indicate that subjects are responding to the *content*, not the logical form, of the conditionals.

Cosmides claimed that the performance difference is due to the fact that the tasks on which subjects perform well involve conditionals expressing *social exchanges*, situations in which obligations are incurred in the act of exchanging benefits (such as in the drinking-age conditional), whereas the tasks on which subjects perform poorly involve abstract conditionals (such as the letter-number conditional). The reason, Cosmides argued, is that the social-exchange conditionals activate a cheater-detection module, which then looks for cheating against the conditional (*drinking beer* while being *sixteen years old*), whereas the abstract conditionals fall on deaf modules, which didn't evolve to solve non-adaptive problems.

After Cosmides proposed her hypothesis, a number of researchers found that performance on Wason selection tasks is also "facilitated" by some conditionals that don't concern social exchanges. Cheng and Holyoak (1989) found that 90 percent of their subjects chose the "logically correct" cards when instructed to check compliance with the conditional rule "If the form says 'ENTERING' on one side, then the other side includes cholera among the list of diseases" (for which an individual received an inoculation). Similarly, Manktelow and Over (1990) found that 69 percent of their subjects chose the "logically correct" cards when instructed to check compliance with the conditional rule "If you clean up spilt blood, then you must wear rubber gloves." Sperber, Cara, and Girotto (1995) published similar findings. These researchers argued that "facilitation" with non-social-exchange conditionals undermines Cosmides' claim that there is a module that specializes in detecting cheaters in social exchanges.

In chapter two of Over's volume, Laurence Fiddick sets out to defend Cosmides' cheater-detection hypothesis against this evidence and to colonize new modular territory. Fiddick argues (pp. 34-37) that Cosmides' critics commit a simple error: They assume that evidence of facilitation on non-social-exchange selection tasks falsifies her hypothesis. But Cosmides' hypothesis is compatible with facilitated performance on selection tasks involving all manner of non-social-exchange conditionals, Fiddick argues, because her hypothesis isn't an account of how the mind works *in general*, just an account of how the cheater-detection mechanism works. In fact, the conditionals employed by Cheng and Holyoak and Manktelow and Over both concern the taking of *precautions*. Thus, rather than counting *against* the cheater-detection module, the evidence that precaution conditionals facilitate performance on the Wason selection task is evidence *for* a *hazard-management module* (see also Fiddick, et al. 2000).

But Fiddick's argument misses the point. A principal piece of the evidence *for* Cosmides' hypothesis is the purported fact that there is a *specific content effect* in the selection task. The standard inference has been: facilitation specific to social exchanges, therefore module specific to detecting cheaters. Evidence of facilitation on non-social-exchange selection tasks was never assumed to falsify the conclusion of the standard inference (the cheater-detection module), but to undermine the supposition that cognition is specifically sensitive to social-exchange contents and, in turn, the inference to a cognitive mechanism designed specifically to deal with such contents.

But further problems lurk here. The idea that there are "content effects" in Wason selection tasks presupposes that the conditionals in those tasks have the same logical form and differ only in their contents. In chapter five, David Over argues that this presupposition is false (pp. 124-125). For instance, "If a card has a vowel on one side, then it has an even number on the other" and "If a person is drinking beer, then they must be at least twenty-one years old" *do not* have the same logical form. The former is an *indicative conditional*, which makes the truth of one proposition conditional upon the truth of another, whereas the latter is a *deontic conditional*, which makes *an obligation* conditional upon the truth of a proposition. This means that the results from the Wason selection tasks actually demonstrate a *logic effect:* Subjects apply different logical principles to indicative and deontic conditionals, and then select the so-called *not-Q* cards with greater frequency in response to the latter because the appropriateness of that response is made more perspicuous by the logical form of deontic conditionals (see Fodor 2000, appendix). And, if people apply logical principles in making truth-functional inferences, their mental logics undoubtedly consist of a single *set of inference rules*, some of which they apply to sentences of one logical type and others of which they apply to sentences of another logical type. Thus, Wason selection tasks fail to show that people don't solve problems by applying domain-general logical principles (see Buller in press, chap. 4).

Moreover, in chapter four Amit Almor argues that a connectionist mechanism could generate the pattern of performance on Wason selection tasks that Evolutionary Psychologists have taken as evidence of modularity (pp. 109-116). While "this mechanism relies mostly on domain-independent principles," Almor argues, "it allows for the emergence of seemingly domain-specific behaviours when operating on particular kinds of inputs" (p. 102). Though Almor doesn't push this argument further, his comment points to a conflation at the heart of the principal theoretical argument for the MMH. Evolutionary Psychologists argue that distinct adaptive problems throughout our evolutionary history would have required distinct solutions, and they conclude that these distinct solutions must be distinct cognitive mechanisms. But all that follows is that distinct adaptive problems would have required distinct *behavioral solutions*, not that they would have required distinct modules. Indeed, domain-general cognitive mechanisms could operate on domain-specific inputs, and make use of information "specific to" those inputs, in order to generate domain-specific behavioral solutions to the problems they encounter. The need for behavioral solutions specific to adaptive problems in our evolutionary history wouldn't have necessarily selected for modules (see Buller in press, chap. 4).

The issues raised in the chapters discussed so far merely concern whether Evolutionary Psychologists have *made their case* for the MMH. But, issues about the interpretation of selection-task evidence aside, there is a serious question as to whether the MMH is even a theoretically viable hypothesis about the structure of the mind. To see why, suppose that the human mind is massively modular, with each module knowing a tremendous lot about how to solve the problems in its proprietary domain. It's a benefit to have such highly specialized cognitive mechanisms only if they know their proprietary problem domains when they see them. For example, it's a benefit to have a module dedicated to reasoning about how to form a social alliance only if that module gets to work in situations that call for forming a social alliance. If it gets to work, consuming your cognitive energies, when your deadly-predator-avoidance module should be working, having a social-alliance-formation module won't benefit you for long. Since modules are activated by mental representations of the problems in their proprietary domains, whether the appropriate module is activated in any given situation is a matter of whether the mind appropriately represents its situation. But the problem of *representing* your situation as presenting Machiavellian opportunity or life-threatening danger is not itself a problem *in* the domain of either alliance formation or predator avoidance. Thus, neither the alliance-formation nor predator-avoidance modules, which function only after being activated by representations of problems *in* their domains, can solve the problem of deciding when it's needed. Since the same is true of all other problems and modules, how does a massively modular mind respond appropriately to the situations in which it finds itself?

This is "the allocation problem," the problem of delegating a situation to the appropriate module, and it is the focus of Gary Brase's chapter. Brase hypothesizes that the mind contains an *allocation system*, comprising a Vast Number of signal detection mechanisms, each of which exhibits a binary response ("hit" or "miss") to its own evolutionarily significant "cue." These signal detection mechanisms function in parallel, and specific patterns of activation trigger specific mental representations, which in turn activate modules. As Brase argues: "In complex situations (such as most real-world settings), there are multiple cues to the nature of the situation, each of which is due to particular properties of that situation.... It is supposed, therefore, that the allocation system is structured to take advantage of these multiple cues by utilizing signal detection mechanisms for particular cues.... So, for example, a social exchange situation ... would involve all or some of the cues of another person's wanting something you possess, of them having something you want, of the relative values of both these items (and if their exchange would result in a net benefit for both parties), and how likely it is that you would receive what you want in the absence of action" (pp. 16-17).

But Brase's allocation system constitutes a genuine solution to the allocation problem only if the "cues" to which his allocation system is responsive are *mere detectables*, properties such as shapes and colors. According to Brase, however, the "cues" to which the allocation system is responsive are such things as *the value of a socially exchangeable item* and *the probability of your receiving what you want in the absence of taking action*. These aren't properties that can simply be *detected* by some signal detection mechanism. They are the representational *outputs* of rather sophisticated cognitive processing. "After all," as Fodor says, "it's not as though some Lurking Benevolence paints social exchanges a proprietary color" (2000, p. 76).

And here's the catch: To determine whether something offered to me — ranging from a food item or a basket to a telephone number or a favor — can be bartered with in the future, I have to draw on knowledge of an unbounded range of social contexts and interactions. To determine how much that item might be worth to others, with whom I may want to barter, I have to draw from an unbounded body of knowledge about the needs, desires, and motives of others and the possible uses to which they could put my item. In short, figuring out the value of a socially exchangeable item requires a brainload of *domain-general cognition;* it's what Fodor (1983) calls a "Quinean" cognitive process, which can draw on relevant beliefs from virtually anywhere in the web. But, if domain-general cognition has to mediate between environmental inputs and the inputs to modules, and if it can effectively solve the *unbounded hard problems* of figuring out how situations should be represented, it's not at all clear why highly specialized modules are needed to solve the *narrowly circumscribed easy problems* that remain after domain-general cognition has solved the hard problems.

If the allocation problem is to be solved in a way that provides aid and comfort to the MMH, Evolutionary Psychologists will have to propose an allocation system that is *less*, rather than more, cognitively sophisticated than modules. The representational inputs to modules will have to be exhaustively analyzable into properties of the environment that are detectable by sensory transducers. Such analysis into sensorially detectable properties could *perhaps* work for representations of nubility, which purportedly activate the male mate-preference module, but it will never work for all the cognitively sophisticated modules postulated by Evolutionary Psychologists (such as those dealing with social exchanges).

Another challenge to the theoretical viability of the MMH is what Sterelny and Griffiths (1999) call the "grain problem." The grain problem stems from Evolutionary Psychology's argument that distinct adaptive problems would have selected for distinct specialized modules. The problem is: If there is an isomorphism between our modules and the adaptive problems faced by our ancestors, how are adaptive problems to be *individuated*, such that we can be confident that we have correctly carved *the mind* at its modular joints? As Sterelny and Griffiths say: "Is the problem of mate choice a single problem or a mosaic of many distinct problems?

These problems might include: When should I be unfaithful to my usual partner? When should I desert my old partner? When should I help my sibs find a partner? When and how should I punish infidelity?" (p. 328). Do we have here one adaptive problem, with several sub-problems, or many distinct adaptive problems? There appear to be no principled grounds for answering either way, hence no principled grounds for claiming either that one module or many evolved in response to the problem(s).

In the third pro-modularity chapter in Over's volume, Anthony Atkinson and Michael Wheeler set out to neutralize the grain problem. But Atkinson and Wheeler think that Sterelny and Griffiths actually understate the grain problem for Evolutionary Psychology. For in addition to the issue of how to individuate adaptive problems, there's an analogous issue of how to individuate phenotypic characters. Is the hand one adaptation or many? Are each of the digits adaptations? And each of their joints? And the cartilage therein? "These observations," Atkinson and Wheeler say, "open the door to what we might call the *'two-dimensional grain problem*,' which can be glossed as the difficulty of matching phenotypic features with selection pressures, *given that selection pressures are hierarchical and nested* (the grain problem according to Sterelny and Griffiths), coupled with the mirror difficulty of matching selection pressures with phenotypic features, given that phenotypic features are hierarchical and nested" (pp. 69-70; emphases added).

According to Atkinson and Wheeler, the two-dimensional grain problem concerns *explanation:* We need to be able to "fix the level at which we describe selection pressures" in order to explain how we should identify the psychological mechanisms that have evolved in response to them; but we also need to be able to "fix the level at which we describe features of an organism's phenotype" in order to explain how we should identify the selection pressures that gave rise to those features. But, "whichever level-fixing decision one takes, to get an explanation going, that decision will be arbitrary" (p. 70).

Having articulated this steroidal version of the grain problem, Atkinson and Wheeler argue that *it's not really a problem*. First, they argue, we aren't forced to privilege one level of description over another for either adaptive problems or psychological mechanisms. Along both dimensions, the phenomena are genuinely hierarchical and nested, so descriptions of different levels in these hierarchies are mutually compatible. Second, they argue, "recurring reciprocal feedback between theories at different levels of analysis and description has the effect of neutralizing the grain problem" (p. 85). For, once we arrive at our richly integrated theoretical knowledge of psychological functioning at all levels of analysis, from the wetware details of neural implementation to the high-level details of information processing, "the pressure to single out a unique level of description at which adaptive problems and phenotypic traits (including cognitive devices) must be described simply dissipates" (p. 85). Indeed, Atkinson and Wheeler argue, Evolutionary Psychologists have already succeeded in utilizing their extensive knowledge of the structure of the mind to "infer adaptive problems from their solutions" (pp. 73-78) and their extensive knowledge of the human "environment of evolutionary adaptedness" to "infer solutions from adaptive problems" (pp. 78-84). (For antidotes to this wide-eyed enthusiasm, see Kaplan 2002; Buller in press, chap. 3).

There are (at least) two problems with Atkinson and Wheeler's argument. First, interpreting Sterelny and Griffiths' argument as showing that selection pressures are "hierarchical and nested" begs the question. For the question of whether the problem of when to be unfaithful and the problem of when to punish infidelity are sub-problems of a single problem or whether they are separate problems is actually the question of whether they selected for *one module* with subroutines that solve each of them or whether they selected for *two independent* modules that don't share and exchange information. If we're to "infer the solutions from the problems," we need to know when we're dealing with sub-problems and when we're dealing with separate problems. By taking the moral of Sterelny and Griffiths' argument to be that adaptive problems are "hierarchical and nested," Atkinson and Wheeler beg the question in favor of seeing them as related sub-problems dealt with by a single hierarchical module.

Second, Atkinson and Wheeler take the grain problem to concern the *explanation* of properties of modules, but the grain problem arises in the context of *discovery* of the modules that make up the mind, not in the context of explanation. For, contrary to Atkinson and Wheeler, *we have no extensive knowledge of the structure of the mind*. The reason that Evolutionary Psychology has generated so much excitement is that it holds out the promise of *discovering the structure of the mind*, so that "in 50 or 100 years one will be able to pick up an equivalent reference work [to *Gray's Anatomy*] for psychology and find in it detailed information-processing descriptions of the multitude of evolved species-typical adaptations of the human mind" (Tooby & Cosmides 1992, p. 69). And "a central premise of evolutionary psychology" is that the "main nonarbitrary way" to *discover* our evolved modules, and their information-processing design, is to begin by articulating "the specific adaptive problems they were designed by selection to solve" during our species' evolution (Buss 1995, p. 6).

But what were "the *specific* adaptive problems" faced by our ancestors, in response to which we purportedly evolved modules? Were the problem of when to be unfaithful and the problem of when to punish infidelity separate problems confronting our ancestors, or were they aspects of a single problem? The answer to this question depends on the nature of the cognitive architecture of our ancestors and whether that architecture processed information about these problems *as* information about aspects of a single problem or *as* information about distinct problems. This is why Sterelny and Griffiths say: "If (but only if) there is a single cognitive device that guides an organism's behavior with respect to issues of mate choice, then mate choice is a single domain, and these are all aspects of the same problem. It is not the existence of a single problem confronting the organism that explains the module, but the existence of the module that explains why we think of mate choice as a single problem" (pp. 328-329). So, in order to correctly identify the adaptive problems that helped shape human psychology, we would need to know something about ancestral human cognitive architectures. But *we don't know* anything about ancestral human cognitive architectures. Thus, we will never be able to reliably identify the grain at which a description of an adaptive problem might permit us to "infer cognitive solutions from adaptive problems."

The final chapter of the volume, by Keith Stanovich and Richard West, attempts the ecumenical reconciliation of the MMH with domain-general cognition, and it is in many ways the most interesting and ambitious of all the chapters. Stanovich and West argue for a *dual-process model* of the mind, according to which the mind contains two cognitive systems. What they call "System 1" is the evolutionarily oldest part of the human cognitive architecture, and it consists of a very large set of the sorts of modules postulated by Evolutionary Psychologists (p. 182). These modules exhibit "tunnel cognition" in tracking goals, and solving problems related to achieving them, that are "in the interests of genes" (pp. 186-188). Relatively recently in human evolutionary history, however, an additional cognitive system was "layered on top of" System 1 (p. 187). This is "System 2," a "central processor" characterized by "serial, rule-based, language-biased, computationally expensive cognition" (p. 182), which tracks the goals that are "in the interests of the vehicle," rather than goals that are "in the interests of genes" (pp. 186-187). Stanovich and West argue that problem-solving conflicts can arise when pursuit of System 1 goals conflicts with pursuit of System 2 goals — when, for example, System 1 commands infidelity, but System 2 argues for restraint (pp. 191-192). In cases of such conflict, they claim, System 2 can override System 1; domain-general cognition can override domain-specific cognition and arrive at a solution to one's problem situation that isn't driven by what was adaptive in the ancestral environment in which System 1 evolved (p. 172).

Stanovich and West develop an interesting hypothesis and offer provocative arguments for it, and it seems beyond doubt that some sort of dual-process model of the mind is correct. Of course, dual-process models of the mind are nothing new (Fodor 1983). The real issue concerns the nature and number of mechanisms in what Stanovich and West call System 1. Fodor argued that only our perceptual and syntactic input systems are modular and "innately specified," while all other cognition is domain general. Evolutionary Psychologists occupy the opposite extreme in arguing that the mind contains "hundreds or thousands" of cognitively sophisticated modules. There are real problems with Evolutionary Psychology's end of the spectrum, and the truth is probably in the region, though shy, of Fodor on the other end of the spectrum (see Buller in press, chap. 4). And this is where Stanovich and West encounter problems. They uncritically accept Evolutionary Psychology's MMH (although confine its applicability to System 1), and in so doing they inherit all of the problems of empirical inadequacy and theoretical inviability that plague the MMH.

In closing, I would like to comment briefly on the premise of Over's volume. In his introduction, Over characterizes the debate over the MMH as having deep implications for whether evolutionary psychology will be able to produce enlightening explanations of social behavior and cultural products. If the MMH is true, Over argues, the development of modules and the behaviors they generate should be highly canalized, in which case we should expect a great deal of human social behavior to be *relatively* uninfluenced by "education or upbringing" (pp. 2-3). On the other hand, if the MMH is "completely false, evolutionary psychology will not have striking implications, beyond psychology, for the study of society and culture" (p. 4). For, in that event, social behavior will be almost entirely explicable in terms of the "education and upbringing" encountered during the course of individual lives, and evolutionary considerations will have little to nothing to add to these explanations (p. 3).

But I don't see the debate over the MMH as having such broad implications for the explanatory relevance of evolutionary psychology. Evolutionary psychology could still have a great deal to contribute to the explanation of social behavior even if Evolutionary Psychology's MMH is false. The MMH maintains that our psychological adaptations are *cognitively sophisticated problem-solving devices*, which endow us not only with goals that are correlated with survival and reproduction, but with lots of "innate knowledge" of how to achieve those goals (Tooby & Cosmides 1992, p. 59; Tooby & Cosmides 1995, p. xiv). Further, these modules "come online" as the adaptive problems they are designed to solve become relevant within the life cycle, in much the way that teeth and breasts "come online" when they are needed (Tooby &

Cosmides 1992, p. 81). If the MMH is false, it merely follows that our psychological adaptations do not take the form of such epistemically well endowed problem-solving mechanisms.

But this leaves other possibilities. One possibility opens up if we separate goals from the knowledge of how to achieve them. It is overwhelmingly likely that, over the course of our evolutionary history, selection has endowed humans with *motivational goals* the *successful* pursuit of which tends, on average, to promote reproductive success. But selection needn't thereby have also endowed us with rich, innate bodies of knowledge about how to navigate our environs to achieve these goals. Indeed, the fact that environments have changed relatively rapidly throughout human evolution means that highly canalized procedures for achieving motivational goals would have been ineffective more often than not. Rapid environmental change typically selects for *phenotypic plasticity*. Thus, selection could have coupled our motivational goals to *cognitive plasticity*, a cognitive system that gradually acquires, through domain-general procedures applied *in situ*, knowledge of how to achieve its goals (see, for example, Sterelny 2003; Buller in press, chap. 4). In short, selection could have endowed us with certain *wants* without thereby also endowing us with *knowledge* about how to get what we want. If this is the case, what we want might "come online" in development before our knowledge of how to go about getting it (a fact to which pubescent males can attest). If this is the structure of the mind, evolutionary psychology may one day tell us a great deal about how selection has designed human *utilities* and brain plasticity, and these explanations will be relevant to much social behavior. Thus, the future prospects of evolutionary psychology don't rise or fall with the fate of Evolutionary Psychology's MMH. We can get over massive modularity while still pursuing an explanatorily robust evolutionary psychology.

References

Barrett, L., et al.: 2002, *Human Evolutionary Psychology*, Princeton University Press, Princeton.

Buller, D.J.: in press, *Adapting Minds: Evolutionary Psychology and the Persistent Quest for Human Nature*, MIT Press, Cambridge, MA.

Buss, D.M.: 1995, "Evolutionary Psychology: A New Paradigm for Psychological Science." *Psychological Inquiry* **6**, 1-30.

Cheng, P.W. & Holyoak, K.J.: 1989, "On the Natural Selection of Reasoning Theories." *Cognition* **33**, 285-313.

Cosmides, L.: 1989, "The Logic of Social Exchange: Has Natural Selection Shaped How Humans Reason? Studies with the Wason Selection Task." *Cognition* **31**, 187-276.

Cosmides, L. & Tooby, J.: 1992, "Cognitive Adaptations for Social Exchange," in J.H. Barkow, et al. (eds.), *The Adapted Mind: Evolutionary Psychology and the Generation of Culture*, Oxford University Press, New York, pp. 163-228.

Fiddick, L., et al.: 2000, "No Interpretation Without Representation: The Role of Domain-Specific Representations and Inferences in the Wason Selection Task." *Cognition* **77**, 1-79.

Fodor, J.: 2000, *The Mind Doesn't Work that Way: The Scope and Limits of Computational Psychology*, MIT Press, Cambridge, MA.

Fodor, J.A.: 1983, *The Modularity of Mind*, MIT Press, Cambridge, MA.

Kaplan, J.M.: 2002, "Historical Evidence and Human Adaptations." *Philosophy of Science* **69**, S294-S304.

Manktelow, K.I. & Over, D.E.: 1990, "Deontic Thought and the Selection Task," in K.J. Gilhooly, et al. (eds.), *Lines of Thinking: Reflections on the Psychology of Thought: Representation, Reasoning, Analogy and Decision Making*, Wiley, New York, pp. 153-164.

Pinker, S.: 1997, *How the Mind Works*, W. W. Norton, New York.

Sperber, D., et al.: 1995, "Relevance Theory Explains the Selection Task." *Cognition* **57**, 31-95.

Sterelny, K.: 2003, *Thought in a Hostile World: The Evolution of Human Cognition*, Blackwell, Malden, MA.

Sterelny, K. & Griffiths, P.E.: 1999, *Sex and Death: An Introduction to Philosophy of Biology*, University of Chicago Press, Chicago.

Symons, D.: 1992, "On the Use and Misuse of Darwinism in the Study of Human Behavior," in J.H. Barkow, et al. (eds.), *The Adapted Mind: Evolutionary Psychology and the Generation of Culture*, Oxford University Press, New York, pp. 137-159.

Tooby, J. & Cosmides, L.: 1992, "The Psychological Foundations of Culture," in J.H. Barkow, et al. (eds.), *The Adapted Mind: Evolutionary Psychology and the Generation of Culture*, Oxford University Press, New York, pp. 19-136.

Tooby, J. & Cosmides, L.: 1995, "Foreword," in S. Baron-Cohen (ed.), *Mindblindness: An Essay on Autism and Theory of Mind*, MIT Press, Cambridge, MA, pp. xi-xviii.