Macquarie Monographs in Cognitive Science

From Mating to Mentality

Evaluating Evolutionary Psychology

edited by
Kim Sterelny
and Julie Fitness

Psychology Press

FROM MATING TO MENTALITY

Macquarie Monographs in Cognitive Science

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Contents

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Kim Sterelny grew up in and near Sydney, doing both his degrees at Sydney University. He taught philosophy at various places and times on Australia's east coast before moving to New Zealand in 1988. Since 1999 he splits his time between New Zealand and the Australian National University in Canberra. His interests have always been on the border between philosophy and the natural sciences; most recently, evolutionary biology and psychology.

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Preface

Evolutionary psychology is an innovative approach to the study of the human mind and human social behavior that is currently the focus of passionate interest and debate within both academia and the wider lay community. To its enthusiasts, the rise of evolutionary psychology heralds an exciting new era of enquiry that promises to illuminate the origins and functions of the human mind. To its detractors, evolutionary psychology is a reductionist, determinist creed that at best, represents a simple-minded predilection for post hoc, "just-so" storytelling, and at worst, promulgates an insidious form of what was once referred to as "Social Darwinism." To the interested but healthily skeptical observer, evolutionary psychology raises as many questions as it attempts to answer, and some of its answers have been justly criticized for being glib and impossible to substantiate. (Not to mention "insensitive," although that is, of course, a political, rather than scientific criticism.) On the other hand, evolutionary psychologists are not guilty of every aspersion cast in their direction, and misconceptions and misunderstandings are rife in this field.

In an attempt to clarify some of these issues and to explore a variety of approaches to evolutionary psychology, the Macquarie Centre for Cognitive Science (MACCS) sponsored an interdisciplinary workshop in July 2001 in conjunction with the Philosophy Program, Research School of the Social Sciences, Australian National University (ANU), Canberra. Organized by Professor Kim Sterelny (Philosophy, ANU) and Dr. Julie Fitness (Psychology, Macquarie University), the aim of the workshop was to bring together a group of internationally acclaimed scholars with interests and expertise in various aspects of evolutionary psychology for discussion and debate.

The workshop participants hailed from a diversity of academic backgrounds, including social, clinical, developmental, and comparative psychology, philosophy, anthropology, and biology; and their papers covered some of the most currently contentious and intriguing aspects of evolutionary psychology, including such topics as the evolution of language, theory of mind, the mentality of apes, and human mate selection strategies. Throughout the proceedings, presenters paid particular attention to the implications of their research and theorizing for the broader field of cognitive science and for the ways in which an evolutionary psychological approach might enrich our understanding of the origins, workings, and functions of the human mind.

The current volume comprises the output from the workshop, along with an additional couple of chapters from scholars who were unable to attend but who were invited to contribute. As would be expected from such a multidisciplinary gathering, a variety of different, and frequently critical, perspectives are represented here. Evident throughout every chapter, however, is the contributors' manifest enthusiasm for grappling with challenging theoretical and methodological issues and their profound fascination with the "why" of human behavior.

We would like to sincerely thank a number of people for their contributions to the resounding success of the workshop and the production of this volume. In particular, we wish to thank the staff of the MACCs Centre, with a special mention to Professor Max Coltheart for his unstinting support, and to Suzanne Mendes for her superhuman (and successful) efforts to keep track of, and look after, such a large number of international visitors. We would also like to thank the philosophers (in particular, Michael Smith and Martin Davies) and the administrative staff (especially Di Crosse) of the Research School of the Social Sciences at ANU, Canberra, for their enthusiastic support, including the provision of such a beautiful location for the workshop. Finally, we express our deepest appreciation to all the workshop participants for their generosity in coming so far to share their ideas, and for their stimulating, thought-provoking contributions. We are confident that the chapters in this volume will continue to fuel lively debate among academics and lay people alike who have an interest in the evolving discipline of evolutionary psychology.

Julie Fitness
Kim Sterelny

1

Introduction

The Evolution of Evolutionary Psychology

KIM STERELNY

JULIE FITNESS

THE ADAPTED MIND PROGRAM

The publication of *The Adapted Mind* saw the canonical formulation of an important version of evolutionary psychology (Barkow, Cosmides, & Tooby, 1992). This collection both articulated and instantiated a very bold program for integrating evolutionary theory with cognitive psychology; an integration free of the defects of previous attempts to create an evolutionary theory of human nature. In comparison with its predecessors—especially Wilsonian sociobiology and its relatives—the Adapted Mind program had many attractions. In particular, see the following points.

The aim of this program was to identify and explain the computational mechanisms of the mind. Its focus was on the cognitive devices that generate behavior, not patterns in human behavior themselves; thus, this program shared the same explanatory focus and explanatory tools as contemporary cognitive psychology. Moreover, the adopted paradigm of the Adapted Mind adherents was chomskian linguistics. Over the last 40 years or more, Chomsky and his coworkers have developed a theory of language that is: (a) nativist, emphasizing our innate endowment for language acquisition; (b) cognitivist, in that the task is to specify the information possessed by speakers about their native language rather than to predict actual linguistic behavior; and (c) domain-specific, in that the cognitive mechanisms that explain our core linguistic abilities are used only for language.

This paradigm has been enormously influential in cognitive psychology: modular, nativist theories of (for example) the child's theory of mind (Baron-Cohen, 1995; Leslie, 1994; Leslie & German, 1995) or of folk biology (Atran, 1998) are applications of Chomsky's picture in new domains. And despite Chomsky's own lack of interest in evolutionary questions, this picture dominates the Adapted Mind program too. Adapted Mind evolutionary psychologists ask different questions about our cognitive equipment, and use different heuristics to identify telling experiments. They aim to explain why we have the proximate mechanisms we do, and they exploit the evolutionary scenarios they construct to guide their experimental program. But they share with orthodox cognitive psychologists the conviction that the apparatus of the mind consists of domain-specific computational tools. This agreement about the fundamental mechanisms of the mind makes an evolutionary cognitive psychology possible.

Its defenders argue that the focus of the Adapted Mind program on cognitive mechanism brings with it a second advantage: testability. Specifically, Adapted Mind hypotheses (the idea goes) can be tested by the techniques of experimental psychology. For example, Cosmides and Tooby confirmed their hypothesis that we have domain-specific specializations for social exchange (they argued) by showing that we have *differential* inferential abilities. We reason better about social exchange contexts than others, for we have special adaptations for such domains (Cosmides, 1989; Cosmides & Tooby, 1989, 1992).

In contrast, Wilsonian conjectures are outrageously information-hungry. Consider, for example, one of the most provocative conjectures to emerge from the Wilson program: Thornhill's conjecture that rape is a facultative adaptation to sexual exclusion (Thornhill & Thornhill, 1987). To confirm this hypothesis, we would need to measure the fitness effect of rape on rapists, and because the offspring of an act of rape may be themselves doomed to low fitness, that entails measuring that fitness in the second generation. We would have to count rapists' grandchildren, and then compare that count with a population of nonrapists matched for their other relevant characteristics. And it gets worse, because we would have to project these fitness effects back in time. This is because the claim that rape is an adaptation is a claim about the evolutionary history of that behavior; its effect in ancestral populations on the fitness of males with the facultative propensity vis-à-vis those that lacked it. Moreover, we would need to show that the propensity is heritable. Therefore, even if the conjecture were right, assembling the evidence needed to confirm it would be virtually impossible. In short, Wilsonian conjectures are intrinsically difficult, perhaps impossible, to confirm. If it is really true that Adapted Mind conjectures can be tested by the methods of experimental psychology, then this is an enormous advance over its predecessor.

The Adapted Mind program is nativist, but it avoids all hint of genetic determinism by emphasizing the role of conditional strategies and by underlining the role of the environment in setting the parameters for innately struc tured modules. In its emphasis on the universality of human cognitive design, intentionally or not, the Adapted Mind program has avoided some of the political mud that stuck to Wilson and to those interested in the phenotypic effects of genetic variation in human populations. Whatever its other failings, no one can claim the Adapted Mind program is racist.

EVIDENTIAL ISSUES

It is thus by no means surprising that over the last decade the Adapted Mind conception of the mind as an ensemble of adapted, innately specified, domain-specific, computational subsystems has set the agenda for integrating psychology and evolutionary theory. This picture is compelling and bold. But, most important of all, is it really empirically tractable? Because historical processes destroy evidence about their own dynamic, testability is a perennial problem for any theory seeking to reconstruct evolutionary history. For example, the morphological trajectory that links the last common ancestor of humans and chimps is only partially preserved.

Moreover, information about the selective environments that drove those changes is both patchy and difficult to interpret. We do have increasingly well-documented evidence about the *physical habitat* of hominid evolution: landscape, climate, vegetation, and ecology. But to a very considerable extent hominids construct their own selective environment (Odling-Smee, 1994; Odling-Smee et al, 2003; Sterelny, 2003). The size of hominid groups; whether they were sedentary or nomadic; their mating and child care practices; the extent of lethal intergroup conflict; the extent to which group life was cooperative (e.g., in supporting sick or injured members) are all factors that profoundly influence fitness and hence hominid evolutionary trajectories. Yet sociocultural organization is hard to read off pollen counts and handaxe abrasion patterns.

The tendency of hominids to modify their selective environments in ways that are invisible in the palaeoecological record is a problem equally for the explanation of morphological and cognitive evolutionary trajectories. We cannot fully explain those trajectories if critical features of the selective environment have left no trace. However, the problem of identifying the trajectory to be explained is far more challenging for cognitive evolution. We now have a fairly good picture of the broad outline of morphological change in the hominid lineage over the last 3 million years (Klein, 1999). That is not true of the evolution of cognition. We cannot put firm dates to the first appearance of language; to the evolution of teaching and imitation learning; or to the formation of enduring pair bonds and paternal investment. Thus, the trajectory through which human cognitive capacities were assembled must be reconstructed from the physical traces of the behaviors generated by ancient minds. However, we are three ways removed from those minds. We see, not the behaviors themselves, but only the physical products of those behaviors. Worse: We see only a sample of those physical products. Moreover, the sample that survives is further modified; we get nothing in mint condition.

Our task, then, is to infer the nature of ancient minds from a small sample of their decayed products. This might seem a difficult ask, especially when we recall how controversial the hypotheses of cognitive psychology often are, despite unobstructed experimental access to subjects' actions, and the opportunity to experimentally intervene, prompting action of special interest in controlled circumstances. Cognitive psychologists are not doomed to passively observe the actions that happen to emerge from the cognitive stew of the agent. Even so, they argue.

These considerations are not meant to suggest that palaeontology is valueless; far from it. Indeed, two chapters in this volume (Corballis & Brockway) make pretty serious attempts to grapple with the trajectory problem for key human cognitive capacities (language and theory of mind). These considerations give us cause to squeeze as much information as possible out of living humans and their relatives. This brings us to perhaps the prime experimental showcase of the Adapted Mind program; the Wason selection task. In its original form, the Wason selection test presents subjects with a rule and a set of potential tests of that rule. One version might go like this. The subject is presented with the rule "If there is a square on the left side of the card, there is an even number on the right side" and four cards, each of which is half masked and half visible. The subject can see the left side of two of the cards. Of these, one shows a square, the other a circle; the right-hand side is masked. The other two cards have their left side masked, and on the right are the numbers 8 and 11. Subjects are asked what cards they must see in order to test the rule. Subjects do very badly. Yet Cosmides and Tooby were able to show that if subjects are given a logically equivalent test about social monitoring and norm enforcement, they do much better. If subjects are asked which of these individuals they should check:

> Sue is 16 and is drinking what?
>
> Kate is 20 and is drinking what?
>
> Samantha is drinking a Coke and is how old?
>
> Louise is drinking a margarita and is how old?

and if the rule to be tested is: "If you are drinking alcohol, you must be over 18," they do fine.

They used this experimental finding as a key part of their argument for a social exchange module. This seems like a very impressive case for the Adapted Mind program. First, this is a surprising datum: in contrast (perhaps) to some of the results about human mating preferences, no one can accuse Cosmides and Tooby here of just dressing folk prejudice in Darwinian clothes. Second, it seems very elegantly explained by the idea that the mind is a set of domain-specific modules, because that hypothesis predicts that our cognitive performance will be highly variable across logically equivalent problems. Thus, it seems quite reasonable to argue that these results provide some support, not just for the claim that our reasoning capacities are content sensitive, but also for the evolutionary scenario that promoted the Cosmides-Tooby experiment. Specifically, they argued as follows: We evolved in social environments in which cooperation and exchange were essential to survival, but where free riding was a threat. We would have been under selection to cooperate; to trade, but warily. The unconditionally trusting would not have done well, but nor would those who did not trade at all. We should be adapted to reason well about social exchange; in particular, we should be alert to free riders and their ilk.

This scenario is the *best explanation* of the Wason selection phenomenon, and hence is confirmed by that phenomenon. That confirmation, of course, would be strengthened by parallel cases. In particular, we should look to other inferential tasks to see whether we observe the same patchy performance, but with improved performances showing up on problems about the same topics. For example, we are notoriously fallible probabilistic reasoners, being subject to a variety of gambler's fallacies and the like. The Adapted Mind case for a domain-specific adaptation is strengthened to the extent that other tendencies to poor reasoning disappear when reasoning about policing social exchange.

In the language of philosophy of science, then, the Adapted Mind inference from domain-specific reasoning skills to the confirmation of an adaptationist hypothesis about those skills is an "inference to the best explanation." Such inferences are tested by attempting to develop alternative explanations, and the chapters in this volume by Sperber and Gray exemplify this strategy. These authors argue that, as it stands, this experimental program does not reveal the existence of domain-specific reasoning skills, because there are alternative explanations of elevated performance on the Wason test. In particular, they argue that a number of relevance heuristics make the possibility of cheating especially salient in the "rule violation" versions of these tests but not in the neutral controls. The ability to use these heuristics is itself quite likely to be an adaptation, but it is a much broader spectrum one than one specifically designed to monitor social exchange. Accordingly there is no adaptationist inference to the best explanation of these reasoning skills.

THE SHADOW OF THE PLEISTOCENE

The chapters in this volume also signal a rapprochement between evolutionary psychology and human behavioural ecology. Around the time the Adapted Mind program was developing, there was a vigorous exchange of views about adaptive behavior (especially in a special issue of volume 11 of *Ethology and Sociobiology*). The Adapted Mind program has no special interest in the extent to which contemporary humans act adaptively. Human cognitive machinery (their idea goes) evolved in the ecological and social world of Pleistocene hunting and gathering. None of us now live in the Pleistocene, and very few of us live in huntergatherer social worlds of any kind. Thus, measuring (say) the birth spacing of Polynesian women in the Cook Islands would test no evolutionary hypothesis. Suppose, for example, we were to discover that their birth spacing is adaptive, tending to optimize the expected number of their grandchildren by approximating the right trade-off between the number of children and the investment per child. So what? We have no idea, from that, what spacing pattern the proximate mechanisms that determine birth spacing decisions would deliver in the very different environment of the Pleistocene. Still less do we know whether that pattern would be adaptive in that environment. Adaptations to past environments might well drive actions that are now maladaptive as a result of environmental change. Fondness for sugar and fat are favorite examples, but Siegert 's chapter is a much more ambitious example of this genre. Indeed, it is largely concerned with this possibility: He explores the hypothesis that psychological disorders (in particular, depression) are the result of cognitive mechanisms misfiring maladaptively because of a mismatch between current and ancestral environments. Moreover, currently adaptive behaviors can have similar explanations: Adaptations to past environments might generate fortuitously adaptive behavior in response to evolutionarily novel phenomena. According to this perspective, only the past explains the present.

In contrast, anthropologists with evolutionary interests (dating from Alexander and Chagnon) have always been interested in the extent to which the behaviors of contemporary humans are adaptive (Betzig, 1999). Those anthropologists have not developed very explicit models of human cognitive capacities. But their working assumption has been that human capacities to respond adaptively across a wide range of social and physical environments are not sharply constrained by innate constraints on our reasoning and motivation systems. Thus, they expect to find humans responding adaptively to many of the challenges they face. Moreover, they expect those findings to be evolutionarily significant, although, not, of course, in the sense that current patterns of action explain existing cognitive mechanisms. Rather, they expect contemporary behaviors— and especially the spread of adaptive responses in a range of different social environments—give us a good guide to the fitness challenges faced by our ancestors and how those challenges were met. According to this perspective, then, the present is a guide to the past (Downes, 2001; Irons, 1999; Smith & Borgerhoff Mulder, 2001).

This volume sees that gap between these two views narrowly dramatically. In particular, the chapters by Simpson and Oriña, Fletcher and Stenswick, and Kenrick et al. (all on aspects of mate choice) signal a move to synthesize these once competing alternatives. They contrast with earlier versions of evolutionary anthropology in developing explicit hypotheses about the cognitive mechanisms involved in mate choice and relationship maintenance. But they also accord with evolutionary anthropology in emphasising the multiplicity of human mating strategies—in particular, the extent of intrasexual variability—and the extent to which different individuals acting within the constraints of their actual situations succeed in acting adaptively.

For example, Simpson and Oriña see women's sexual strategy as dominated by the trade-off between seeking good genetic resources and economic investment from their mates. Male strategies make it difficult in general for women to optimize both types of resource at once; therefore, often one trades off against the other, with the appropriate trade-off being very sensitive both to environmental factors and to women's specific circumstances. Moreover, their data suggest that women make these trade-offs astutely and adaptively. To the degree that they make predictions about the extent to which agents act adaptively (and the chapters vary somewhat in this), these theorists increase the empirical content of their hypotheses.

Our guess is that it is no accident that mate choice is the leading edge of this synthesis; it is most unlikely that mate choice in the Pleistocene was simple. Thus, we are not surprised by Gray's data suggesting that male attractiveness judgments, including the response to waist-to-hip ratios, are developmentally plastic rather than being wired in. In contemporary hunter-gatherer societies, such choices are constrained and complicated by idiosyncratic features of the group's culture (e.g., their outbreeding rules); the extent to which individuals can choose autonomously rather than being constrained by family and group norms; and the agents' assessments of their own position and that of others on the relationship market (Lee & Daly, 1999). There is no reason to believe that Pleistocene environments were any less variable, and hence no reason to believe that there are sharp innate constraints on the mate choice situations to which humans can respond adaptively.

THE COMPARATIVE CONTEXT

No one expects to understand the evolution of the human mind through observations of contemporary humans alone. Comparative biology is now central to testing adaptationist hypotheses, for (as in the menstruation hypothesis Gray discusses) such hypotheses often make very clean predictions about the origin and distribution of traits on a phylogenetic tree. Human cognitive evolution must be placed in a comparative and historical context. One role of that context is to establish cognitive baselines; that is, to determine the cognitive capacities that evolved in the hominid lineage. Comparative biology is the standard tool for establishing such baselines. If a particular capacity—for example, mirror self-recognition—is found in both sapiens and the great apes, then the most parsimonious hypothesis is that this capacity existed in the last common ancestor of sapiens and the great apes, and that these species inherited this capacity from that ancestor rather than evolving it for themselves.

Suddendorf and Whiten, Corballis, and Brockway, in different ways attempt to paint in a comparative and historical background. Corballis takes on the notoriously difficult problem of language, whereas Brockway and Suddendorf and Whiten address an equally crucial human skill: our ability to interpret others. In the trade, this capacity is variously known as "theory of mind" or "mind reading" (as opposed to mere behavior reading), though both these terms are somewhat tendentious. The "theory of mind" is so because the term begs the question in favor of the contentious claim that our capacity to predict and explain the actions of others depends on a genuinely theorylike representation of human cognitive architecture; "mind-reading" because it suggests a sharp dichotomy between the capacity to represent behavior and the capacity to represent others' mental states.

It is precisely this dichotomy that Suddendorf and Whiten reject. They argue that the mind reader/behavior reader distinction is *too coarse.* Although there is little or no evidence that great apes have beliefs about beliefs, they argue that there is evidence (of a preliminary kind) that great apes have a range of capacities to represent their environment in relatively abstract ways; a range the Suddendorf and Whiten group together as "secondary representation". Secondary representation includes factive but referentially displaced representation: representations of how the agent takes the world to be at other times and places. It includes indicative mood but nonfactive representation: representations of how the world might be, or of how the world will be, if certain conditions are or were met. It includes abstract representations of how the current environment actually is. It includes an agent's ability to represent the goals of his or her actions. Something like a "theory of mind" has evolved in the hominid lineage: We do have beliefs about beliefs. But if Suddendorf and Whiten are right, this hominid invention takes off from the rich representational capacities of the last common ancestor.

In virtue of making claims about a process that in part destroys its own causal history, evolutionary psychologies face a testability challenge, and so far in this Introduction we have emphasized that fact. But they face (at least) two other challenges as well. One is the origin problem: When a cognitive ability has a social function, we need to explain how it can be adaptive when it is rare. This is a notorious challenge facing evolutionary theories of cooperation: how could the disposition to cooperate (however judiciously) be selectively advantageous in an uncooperative world? Theories of the evolution of language— especially those that emphasize innate constraints on language—face a similar challenge. How could a mutation that (say) enabled a speaker to use modal terms be advantageous in a world in which no one else used or understood that vocabulary? The second is the trajectory problem: How can a complex capacity like language be built incrementally? To paraphrase the late Stephen Jay Gould, of what use is 5% of a relative clause?

Corballis does not address the trajectory problem. But his chapter is an indirect attack on the origin problem: He argues that the core innovation that makes language possible is recursion, because recursion makes syntax possible. And recursion, he argues, is a more general capacity than language. It lies behind our interpretative capacities; our capacity to project ourselves in thought forward and backward in time, and, perhaps, some of our capacities to manipulate our physical environment. This adaptation, then, makes language possible. But it makes other capacities possible too. So when it is still rare, it does not have to be selected for its language-making potential. It could be established in the population through boosting planning, interpretative and manipulative skills, and then be exapted for language.

Brockway takes on our theory of mind capacities, tackling both the origin problem and the trajectory problem, although admittedly in speculative fashion. Her hypothesis is that theory of mind may have evolved from motherinfant mind reading, and her hypothesis has some important advantages. (a) The advantage of maternal mind reading does not depend on mutual interpretation, so the fitness of this trait would not be lower when rare. (b) We know that ours is a lineage that has invested massively in vertically transmitted social learning. Young hominids have adaptations for social learning; in particular, learning by imitation. They have a prolonged period of dependency during which they live in prolonged association with (mostly) their mothers. These facts tend to make maternal mind reading both possible (mothers are in a position to know a lot about their offspring) and advantageous, for it is a tool for managing social learning. (c) As mind reading goes, presumably infant mind-reading is relatively simple, because infants have a relatively restricted set of goals and opinions. They also have not yet had the chance to learn deception strategies. (d) Even setting aside the fact that infants have not yet learned to dissemble, the mind-reading task is less complicated by resistance. In most social interactions, it is not in agents' interests to be wholly transparent to one another, so the targets of mind reading often partially subvert attempts to read them. That is less true of mother-offspring interactions. Mothers and their infants do not have identical evolutionary interests, but their interests do overlap very considerably. For this reason, Brockway's hypothesis that mind reading has invaded the hominid lineage via a mother-infant beachhead is decidedly plausible. The next task will be to develop more rigorous empirical tests of this conjecture.

ADAPTATION AND DEVELOPMENT

Chisholm's chapter contrasts markedly with that of Brockway. Whereas Brockway emphasizes maternal role and the overlap of maternal interests with those of her offspring, Chisholm emphasizes the infant's role and the potential for evolutionary conflict between mother and child. As Chisholm sees it, there are a number of factors in hominid evolution that potentially exacerbate that conflict. First, humans are a highly K-selected species. The hominid breeding strategy—even more than that of the average primate—depends on producing few offspring, each of which is the target of massive amounts of parental (especially maternal) investment. Second, human life history changes have lengthened the period of juvenile dependency. Third, the shift to bipedal locomotion coupled with the expansion of hominid brains has led to the "obstetric dilemma." The result of that dilemma is that human newborns are even more dependent on their mothers than those of our K-selected great ape relatives.

The consequence, as Chisholm sees it, is that the investment demands on children—their price—has gone up. It follows that there is selection on mothers (and investing fathers) to be more discriminating investors. Mothers must be discriminating both in being prepared to cut their losses early, and also in distributing their investment optimally among their actual and potential future children. Optimally, that is, *from the mother's point of view.* If there is selection for discriminating investment by mothers, it follows that there must be selection on infants. First there is selection on infants to secure initial attachment (see especially Hrdy, 1999), and then to suck up for themselves the optimal amount of parental investment. Optimal, that is, *from the child's point of view.* Now it is not impossible that the parental optimum equals the child's optimum. For example, infant fitness might not increase as a simple function of increasing maternal investment. If it is subject, for the child, to a strong law of diminishing returns, the child's inclusive fitness may be maximized by switching that investment to a sibling. But we certainly have no reason to expect that the parental optimum equals the child's optimum. There is a clear potential for parent-child evolutionary conflict.

It is Chisholm's conjecture that this potential has been realized. In his picture, the evolution of theory of mind capacities are offspring-driven rather than parent-driven. As he sees it, those capacities are an exaptation of two phylogenetically quite ancient systems: an attachment mechanism whose original function was just to ensure that offspring and parents recognize and respond to one another with ancient mechanisms for predicting future actions on the basis of present cues.

Allied to this interesting evolutionary hypothesis is an important idea about development. One of the chomskian themes of the Adapted Mind program is nativism; the claim that the development of cognitive specializations is innately programmed by our genetic endowment. Indeed, according to the Adapted Mind program, cognitive evolution consists in the assembly of new genetically specified cognitive mechanisms. Without denying the role of innate mechanisms of the mind, Chisholm brings a more sophisticated developmental perspective to these issues. He accepts that theory of mind skills do indeed have an innate basis. But that innate basis is more ancient and less specific than a theory of mind module. It is social contingency detection. That is, we are innately programmed with capacities for anticipating future actions on the basis of present social cues. The theory of mind is an adaptation, all right, on both Brockway's and Chisholm's views, because our acquisition through learning of a theory of mind is no accident. We are *primed* to learn it; we more-or-less automatically develop to see actions as functional, and purposeful. The "teleological stance" is natural for infants. Moreover, we learn it because learning feels good: As Chisholm says, contingency detection elicits an intrinsically rewarding "got it" feeling. And, of course, it is socially scaffolded.

Chisholm has put together an interesting—although admittedly still preliminary—case for the idea that the theory of mind is an ontogenetic specialization. This case is important, for it underscores the difference between claiming that a capacity is an adaptation and claiming that it is innate. The developmental psychology of evolutionary psychology does not have to be nativist. Learning and other mechanisms of environmental inducement can be reliable mechanisms for ensuring that offspring cognitively resemble their parents. As Chisholm shows, evolutionary psychologists need make no bets on the extent to which cognitive development is genetically buffered against environmental perturbation.

In summary, evolutionary psychology clearly still has a long way to go before it manifests the pattern of progressive improvement typical of a mature science. We think that the chapters in this volume are part of the beginning of the journey down that long road. Importantly, they demonstrate the ways in which evolutionary hypotheses about the origins and functions of the human mind may be both tested and extended by being brought into systematic contact with data.

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2

Evolutionary Cognitive Science

Adding What and Why to How the Mind Works

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Students who take a general psychology course often ask: How does this all fit together? Why in the world does one course range over neural biochemistry, cognitive development, group decision making, and sex differences in aggression? More broadly, how do all the scattered tidbits of wisdom in their psychology courses fit with what they are learning in their anthropology, biology, and economics courses? Psychology textbooks have not traditionally provided an overall framework that would answer these questions. Instead, typical texts have proceeded from one set of empirical findings to another. "We're done talking about neurons and hormones, now let's jump over to talk about babies and their mothers, and then let's skip to Pavlov's dogs."

Even when students advance to a course in a particular subdiscipline, such as social psychology, they again confront isolated findings rather than an integrated whole. When the first author entered the field, one of his advisors informed him that social psychologists favor minitheories. Indeed, the minitheories dealing with aggression, attraction, stereotyping, and leadership proved to be as remote from one another as they were from mini-theories about vision, psycholinguistics, or schizophrenia in the other subdisciplines. Our colleague Bob Cialdini likens a typical social psychology text to a three-ring circus—entertaining and bedazzling, but without much of a plot.

During recent decades, there have been three attempts at conceptual integration involving different areas of psychology. Cognitive science represents an interdisciplinary fusion of ideas from cognitive psychology, artificial intelligence, philosophy of mind, linguistics, and several other fields (Anderson, 1980; Gardner, 1985). Evolutionary psychology is a synthesis of developments in ethol-ogy, cognitive psychology, evolutionary biology, and anthropology, with tributaries from animal learning as well as social, developmental, and environmental psychology (Buss, 1999; Cosmides, Tooby, & Barkow, 1992; Pinker, 1997; Shepard, 1992; Sherry & Schacter, 1987). Dynamical systems theory, or complexity theory, has roots in computer science, ecology, mathematics, and physics, and its implications have been explored in fields as diverse as economics, genetics, and social psychology (Lewin, 1993; Nowak & Vallacher, 1998; Waldrop, 1993).

We believe the behavioral sciences stand on the brink of a meta-synthesis that will unite these three developments, and in the process provide psychology's long-awaited integrative paradigm. Later in this chapter, we present the outlines of an evolutionary dynamic psychology (Kenrick, Li, & Butner, 2003; Kenrick, Maner, Butner, Li, Becker, & Schaller, 2002). We argue that such attempts at grand synthesis are not just a luxury, but a necessity. If researchers don't stand back to look at the big picture, they will miss the significance of the research details they're staring at through their magnifying glasses.

This chapter proceeds in three sections, and make eight partially overlapping points. Two points deal with very general questions about why the mind works.

1. *The assassination of Archduke Ferdinand in Sarajevo on June 28, 1914, did not really start World War I.* As our high school history teachers taught us, we need to consider ultimate or background factors to understand the significance of a proximate cause. When asking about the structure and function of living organisms, including behavior and cognitive processing in *Homo sapiens,* that means putting present events in the broader context of evolutionary history. We have to ask the deeper "why" questions, and not be satisfied with a 1day window on the news.

2. *The war is over, we all won.* The wider perspective of evolutionary history is essential in limiting our hypotheses, and helping us understand how structure and function go together. But this is not to say that psychologists should drop their laboratory experiments on attention, learning, decision making, or social interaction, buy pith helmets, and march off to Africa to live among the chimpanzees or dig up australopithecine bones.

Four of our points deal with what the mind works on.

3. *The mind is not a blank slate, it's a coloring book.* An ever-broadening base of findings suggests that humans come into the world prepared to selectively attend to, perceive, and remember some things more readily than others, and to respond to those things with innate, albeit flexible, decision-making mechanisms.

4. *The human brain is not designed to recognize printed words presented at 250 msec.* Part of the human heritage makes it possible for us to learn to process printed words, and the study of word recognition has been foundational for modern cognitive psychology. However, understanding word recognition is not sufficient for an understanding of most of the things humans are naturally inclined to think about.

5. *Human reproduction is more than just copulation.* Ultimately, evolution is about differential reproduction. But that hardly means that evolutionary psychology applies only to topics such as sexual attraction and mate selection. Our ancestors reproduced successfully only after a lot of talking, negotiating, navigating around in space, and learning about the trajectories of flying objects.

Hence, an evolutionary approach to psychology is relevant to the entire field, not just selected areas of social psychology.

6. *Six minds are better than one.* Evolutionary studies of birds suggest that even their relatively parsimonious little brains process different types of information in very different ways, using different software packages and different hardware components (Sherry & Schacter, 1987). Likewise, human information processing is modular in important ways.

The last two points caution that we mind our misconceptions about evolutionary psychology.

7. *Reductionism ain't what it used to be.* Contrary to one commonly promoted misconception, evolutionary psychology is not about isolating single genes. Instead, it is about using an adaptationist perspective to help understand complex living organisms interacting with other complex living organisms in interconnected networks. To better understand how that all works, we need to integrate insights from dynamical systems theory with those of evolutionary psychology.

8. *The dynamical adaptationist map: Don't leave home without it.* This point will bring us full circle back to Sarajevo and Archduke Ferdinand. Graduate training in psychology equips researchers with analytical tools for dissecting and isolating single proximate causes of behavior. These tools are useful, but if one doesn't take an occasional look at the map, it is easy to lose the way and to make incomplete and sometimes misleading conclusions about where the local roads begin and end.

WHY THE MIND WORKS

An evolutionary perspective suggests that psychologists reconsider the point at which they are happy with a causal explanation, favoring a set-point relatively closer to the ultimate than the proximate end of the spectrum. But this is not to suggest that research addressing proximate mechanisms should be abandoned, or that evolution somehow trumps cognition, learning, or culture.

The Assassination of Archduke Ferdinand Did Not Really Start World War I

Historians point to the assassination of Archduke Francis Ferdinand as the event that ignited World War I. But a student asked to intelligently discuss the causes of that war would get a failing grade if he answered: The assassination of Archduke Ferdinand, period, and then moved on to the next question.

Causal explanations can be arranged along a continuum from "proximate" to "ultimate." Proximate explanations focus on immediate precipitating circumstances, such as Archduke Ferdinand's assassination. Ultimate explanations focus on background factors that give meaning to those proximate effects. In the case of World War I, the background factors that gave significance to a murder in the Balkans included increasing nationalism in Europe in combination with a network of military alliances. A satisfying explanation connects proximate effects with what we might call their root causes.

Perhaps because we have been infatuated with the control offered by the experimental method, and perhaps because of the strong influence of behaviorism during the last century (which eschewed inferences about hidden causes in favor of empirical measurements of the immediately observable), psychologists have often been satisfied with proximate explanations. But looking too closely at a phenomenon leads us to incomplete, and sometimes even incorrect, conclusions.

For example, before the cognitive revolution, social psychology was dominated by behaviorist models. Behaviorists, by definition, were opposed to the practice of searching for “underlying causes,” and would stop searching when they determined that a particular class of stimuli would increase or decrease the probability of a response. The most popular theory of interpersonal attraction was inspired by behaviorist principles, and viewed attraction in terms of very simple principles of classical conditioning (Byrne, 1971). In that model, people were seen as attracted to physically attractive others because attractiveness was “rewarding,” and sure enough, experiments demonstrated that people would work harder to gaze at good-looking than less good-looking people, and that it made people feel good to gaze at good-looking others (e.g., Byrne, London, & Reeves, 1968; Dion, 1977).

However, the reinforcement-affect theory told us little about why certain features are judged as physically attractive in the first place. And it also begged the question of why the features that make for an attractive woman are sometimes very different from those that make for an attractive man (consider jaw size, shoulder size, and waist-to-hip ratio, for example). When asked why certain features are rewarding, social learning theorists assumed that it was a function of a past history of rewards—the models in advertisements having fun at the beach were tall athletic-looking people with shiny hair, smooth skin, and small waists as opposed to short people with thin arms, large waists, dry hair, and pock-marked skin. To prove a causal role for such learning factors, it would be necessary to show that attractiveness preferences differ among people who have had differing amounts of exposure to such social stimuli. We are not aware of any social learning theorists who made such tests. However, Cunningham and his colleagues have collected cross-cultural data on attractiveness judgments that are not consistent with this assumption, but instead suggest some common criteria for attractiveness across different cultures (Cunningham, Roberts, Barbee, Druen, & Wu, 1995).

Moving to a slightly less proximate level to explain attraction, some social psychologists have invoked the concept of “culture” (which can be conceptualized as a lifetime of learned reward contingencies). It is in fact easy to observe that many preferences are shared by everyone in the local culture, and the cultural genesis theory was often accepted as the ultimate cause without further ado. American social psychologists, for example, had frequently explained gender differences in mate preferences in terms of the current norms of North American culture. But these explanations were rarely based on comparisons across the range of human cultures. With regard to age preferences in attraction, for example, Kenrick and Keefe (1992) found a number of problems with the existing explanations, most of which focused on local cultural determinants. To begin with, most psychologists had slightly mis-described the phenomenon by saying that men are attracted to women who are slightly younger, whereas women are attracted to men who are slightly older. This pattern indeed seemed to fit with obvious norms in American society. One team of researchers, who observed a difference in preferred ages stated by men and women in singles ads, explained the difference in terms of "traditional sex-role specifications…frequently valued as sex appropriate in American Society,"which specify that women should "look up to" their partners (Cameron, Oskamp, & Sparks, 1977, p. 29). Deutsch, Zalenski, and Clark (1986) similarly speculated about a "double standard of aging" in our society.

In the face of additional data generated from an evolutionary perspective, though, the theory that age preferences originated in American cultural norms in which women look up to men and men look for women with less power started to unravel. For one thing, teenage males, normally very sensitive to sexrole norms, didn't seem to get it. Teenage boys are attracted to relatively older women in their twenties, even though they realized that these women had no reciprocal interest in them (thereby violating the normative and reward theories) (Kenrick, Gabrielidis, Keefe, & Cornelius, 1996). And men in their twenties hardly had an aversion to women a couple of years older, on average advertising for women up to 5 years older (Kenrick & Keefe, 1992). It was only older men who showed a strong preference for relatively younger women, and that preference got stronger and stronger as the men aged, so that men above 40 were not generally interested in women their own age at all, but sought much younger women. Furthermore, data from a number of different cultures and historical periods revealed the same general pattern found in North America. Indeed, as one got further away from modern urban societies such as the United States and Holland, older men's preference for younger women tended to get stronger rather than weaker. For example Kenrick and Keefe (1992) examined data from the U.S. and the Philippines (Fig. 2.1).* Since then, other researchers have replicated the pattern in Africa, Asia, and South America (e.g., Harpending, 1992; Otta, Queiroz, Campos, daSilva, & Silveira, 1998).

We believe a more parsimonious explanation of age preferences in mates requires a move far beyond the proximate realm, considering human mate preferences not simply in terms of rewards or local norms, but in terms of the conditions of human reproduction. Human beings, unlike 95% of other mammals, tend to pair up to care for their offspring. Both males and females contribute to those large-brained offspring, but they contribute slightly different resources. A woman contributes direct bodily resources, carrying the fetus inside her body and afterward nourishing it with the rich milk produced by her body. A man can do neither of these things, but can contribute indirect resources, such as food and shelter. Women's ability to carry and nurse children is low before the full onset of puberty, very high throughout the twenties, and then progressively drops in the thirties and forties until it completely terminates in menopause. Men's ability to gather food and gain positions of trust and status within cooperative alliances is low when they are teenagers, and tends to increase with age (at least until senescence). Hence, women at all ages are attracted to men who are somewhat older. Young men, like women, are attracted to relatively older partners, whereas men in their twenties are attracted to women of their own age, and men over 30 are attracted to relatively younger and younger women.

Consider the interesting case of the Tiwi of North Australia, which at first glance seems to be an exception to the pattern. Among the traditional Tiwi, a young man often married a much older woman (Hart & Pillig, 1960). Do Tiwi preferences actually violate the seemingly parsimonious life history model that Kenrick and Keefe (1992) laid out? As it turns out, the unusual cultural prac tices of the Tiwi actually support the model quite nicely. How can this be? Among the traditional Tiwi, all women were required to be married. Widows were remarried at their husbands' gravesites, and infant girls were betrothed at birth. It was not required that men be married, on the other hand, and because the society was polygynous, many men remained single for a good portion of their lives. There were two ways for a man to get a wife—to have an older married man betroth his infant girl to cement an alliance, or to marry an older widow to gain her resources, thereby cementing an alliance with her male relatives. As it turns out, traditional Tiwi men married older women not because of a reversal of normal attraction preferences, but as a pathway to gaining the younger wives they found most desirable.

Evolutionary theorists do not assume that people are aware of the ultimate causes of their behavior, any more than they assume that other animals are aware of the connection between their behaviors and their inclusive fitness. A bee-eater is a bird that sometimes helps its parents at the nest, but the helping is contingent on whether or not it has a chance to raise offspring of its own this season, and whether its parents' new hatchlings are its full siblings or half-siblings (Emlen, Wrege, & DeMong, 1995). Biologists do not assume the bee-eater consciously calculates benefits to its inclusive fitness. Instead, they simply assume that animals with certain environmentally sensitive behavioral mechanisms were successful in passing on copies of their genes; other animals with other behavioral proclivities were less successful.

A satisfactory explanation of a natural phenomenon traces a behavior to its functional roots, rather than focusing on only proximate triggers. We *ask why* is

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*Although marriage data from the U.S. sample indicated that males had brides younger than themselves—and that this tendency was more pronounced the older the male— the trend was even more evident in the sample from the Philippines.

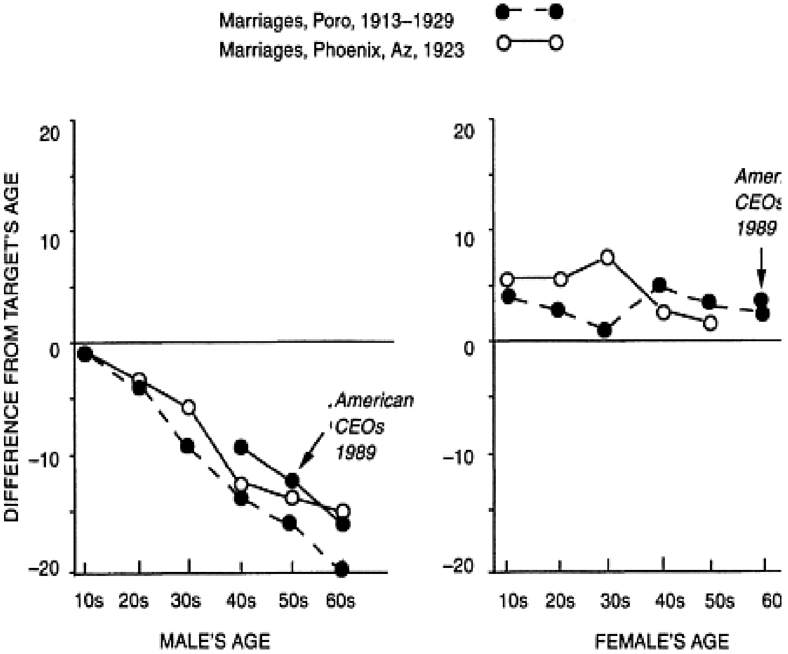


FIGURE 2.1. Age differences between men and their wives (left), and between women and their husbands (right) on a small Pacific island early in the 20th century, in Phoenix, Arizona in 1923, and in a population of wealthy chief executives of American corporations (based on Kenrick & Keefe, 1992; Kenrick, Trost, & Sheets, 1996). Marriages from other societies and times, and mating advertisements from different cultures suggest that the sex difference is universal.

it that men feel attraction toward women in their twenties with low waist-to-hip ratios rather than toward high-status women with broad shoulders, and why is it that bee-eaters only help their own siblings, and reduce that help when they are half-siblings (Emlen et al, 1995; Kenrick & Keefe, 1992). We don't hope to arrive at an answer by asking the bee-eater or the middle-aged man with a trophy wife. Instead, researchers must dig beneath the surface and examine a nomological network of data obtained from different methods applied to different species.

There is a line in a movie about the Mafia in which Joe Mantegna introduces an aging Don Ameche to a younger hoodlum by saying, "This is the guy behind the guy behind the guy." We are arguing that a complete analysis of a behavioral phenomenon needs to look for the "why" behind the "why" behind the "why" But how far does one carry the search for background causes? To the genesis of life on Earth, or further back to the Big Bang? If this were the case, a student could answer all causal questions in every course with the same answer: Big Bang. Of course, tracing things that far would be not only unnecessary, but also unproductive. When searching for the root causes of the behavior of living organisms, the satisfactory stop-point is the point at which we connect the current proximate causes to their adaptive function—the particular way in which behaviors of this sort might have served ancestral survival and reproduction. It is important to note that a causal explanation that simply pointed to "differential reproduction" would be going one step too far up the ladder, as it would not distinguish the explanation for a bat's sonar capacities, human color vision, and bee's ability to detect ultraviolet light. We want to understand the particulars—why it is that these different organisms have such different sensory capacities. A more satisfying, and more useful level of explanation, for example, would connect the bat's sonar with the demands of flying about hunting for flying insects at night. This was a problem that ancestral bats, but not ancestral humans or bees, needed to solve. Thus, an adaptationist account seeks to answer how an animal's cognitive and behavioral mechanisms are connected to the demands and opportunities its ancestors regularly confronted in dealing with the particular physical and social environments.

The War Is Over, We All Won

When Shoichi Yokoi was asked why he hid in a cave in the jungles of Guam for 28 years after World War II ended, he explained: "We Japanese soldiers were told to prefer death to the disgrace of getting captured alive." Psychology cer tainly has its share of Shoichi Yokois, who have taken it as a matter of pride to continue resisting the evolutionary revolution for decades.

Would embracing an evolutionary explanation mean giving up research on ongoing phenomenology or learning processes or culture? Imagine that the historical issue raised earlier was framed as a multiple-choice rather than an essay question:

World War I was caused by:

a. The assassination of Archduke Ferdinand.
b. Military alliances and nationalism in 1914.
c. Historical and cultural trends in Europe with roots that began in the decades preceding 1914.
d. An interaction between cultural factors in 20th century Europe and fundamental human motivations such as self-protection.

A student reading that question would probably turn the page to see if the typist had somehow misplaced "e. All of the above." A complete explanation of a phenomenon covers a range from proximate to ultimate causes. An explanation in terms of adaptive function does not somehow negate the requirement that we understand the proximate causes of a behavioral or cognitive phenomenon. And a consideration of the evolutionary roots of behavior does not put experimental psychologists out of business, and require that we all devote our time to studies of hunter-gatherers, gorillas, or Neanderthal excavation sites. In fact, because we carry the vestiges of ancestral adaptations around inside our heads, one of the best ways to gather evidence regarding the adaptive significance of human behavior is to study the biases used by modern humans in solving problems. There are serious limitations to what we could learn about behavior and cognition from studying pottery shards and skull fragments. Much more can be learned from studying living human beings as they think and behave.

At the same time, a consideration of information at multiple levels can help us gain an understanding of a phenomenon that would not be possible if only one level were considered at a time. For example, experimental social psychological studies suggest that nonverbal indications of social dominance increase the sexual attractiveness of males, but not females (e.g., Sadalla, Kenrick, & Vershure, 1987). Comparative studies done with other animals indicate a link between an individual's testosterone level and his or her social rank (e.g., Rose, Bernstein, & Holaday, 1971). Physiological studies indicate that males typically produce more testosterone than do females (Mazur & Booth, 1998). Correlational studies indicate more antisocially competitive behavior among individuals with high testosterone, particularly when other paths to social success are blocked (Dabbs & Morris, 1990). Together, these and other sources of evidence provide a nomological network of findings that fit together nicely to tell a compelling story about sexual selection and gender differences (Geary, 1998). No one source of data is superior to the others, and none is superfluous—each is necessary to understand a complicated but ultimately sensible natural process. Although data from psychological experiments are not, by themselves, sufficient, they are, in alliance with data from other disciplines and methods, necessary for complete explanations of behavior.

WHAT THE MIND WORKS ON

To understand the why question—the ultimate function of a behavioral mechanism, evolutionary psychologists believe it is important to consider the what question—examining specific content as well as general process (Kenrick, Sadalla, & Keefe, 1998; Tooby & Cosmides, 1992). We argue in this section that humans, like other animals, come pre-equipped with a number of specialized psychological mechanisms designed to deal with particular problems confronted by our ancestors, and we offer some preliminary ideas about some of the key problem domains.

The Mind Is Not a Tabula Rasa, It's a Coloring Book

When the senior author of this chapter was a graduate student, psychologists still talked with a straight face about the mind as a *tabula rasa*—a blank slate on which experience wrote. Since that time, sufficient numbers of discordant findings have proliferated to make the blank slate look like the grafitti-filled wall of a New York subway station.

Several large bodies of literature have undermined the blank slate viewpoint.

Behavior Genetic Research. Research from studies of twins and adoptees suggested that behavioral, affective, and cognitive capacities and predispositions are indeed passed from one generation to the next (Plomin & Caspi, 1999; Rowe, 1997). Examples include personality traits such as extraversion and conscientiousness (Loehlin, 1992), sexual orientation (Bailey & Pillard, 1991), and general intelligence (McGue, Bouchard, Iacono, & Lykken, 1993).

Cross-Cultural Findings. Viewed through the standard ethnocentric lenses, our eyes are drawn to the strange and unusual, the ways in which “they” are different from “us.” But beneath the sometimes colorful differences, like those separating Tiwis and Tierra del Fuegans, ours is one species, and our neighbors the world over share certain important ways of behaving, thinking, and feeling (e.g., Daly & Wilson, 1988; Ekman & Friesen, 1971; Kenrick & Keefe, 1992). Some well-known examples of broad human commonalities include emotion recognition (Ekman & Friesen, 1971), color perception (Rosch, 1973), and a facile ability to learn a complex spoken language (Pinker, 1994).

Cross-Species Comparisons. Findings from different animal species reveal that our species, though unique in some ways, also shares many common behavioral and psychological mechanisms with other animal species. Comparative research has revealed powerful principles such as inclusive fitness, differential parental investment, and sexual selection that can help explain many of the patterns found in animal behavior (Gould & Gould, 1989; Trivers, 1985). Functional analyses have proven essential to understanding why some animals, but not others, see in color, whereas others don’t see at all; why some have dominance hierarchies; and why some have females that are more colorful and competitive than males (Alcock, 2001; Mollon, 1989; Williams, 1996).

Developmental Findings. A few years ago, the first author asked the current editor of *Child Development* what was new and exciting in the field. She responded that there was more and more evidence coming in to suggest that human infants enter the world equipped with specialized perceptual and cognitive abilities (see Spelke & Newport, 1998, for a review). For example, preverbal infants respond to colored lights not in terms of a continuous distribution of physical wavelengths, which it is, but in categorical terms that match the verbal labels adults use, breaking blue, green, yellow, and red just as adults do (Bornstein, 1979). Infants are also highly attentive to features of human faces (Johnson, Dziurawiec, Ellis, & Morton, 1991), and to the sounds of human phonemes (Pinker, 1994).

But although the blank slate doesn’t work as a metaphor, neither does the prepainted canvas. A better metaphor is that of a coloring book. It has been three decades since Seligman and Hager (1972) brought together an impressive selection of papers on what they called *Biological Boundaries on Learning.* The point of that body of work was not to say that animals come pre-equipped with answers, but with a strong inclination to follow clues of the particular sort that would have solved the particular problems their ancestors were likely to confront. For example, Wilcoxon, Dragoin, and Kral (1971) reported that quail, which use vision to locate food, condition nausea more easily to visual cues than to tastes, whereas rats, who normally use taste and smell to locate food, show the opposite pattern. In other work, Öhman and Dimberg (1978) demonstrated that people have difficulty extinguishing fear responses to snakes and spiders and to angry, but not happy or neutral, faces (Öhman & Dimberg, 1978; Öhman, Erixon, & Lofburg, 1975). Each of these phenomena indicates learning, but learning that is directed in adaptive ways.

Research on human language also fits the view that organisms come equipped with a collection of psychological mechanisms to assist them in selectively learning particular information in particular categories. For example, we noted earlier that infants are particularly attentive to human phonemes. And human language all around the world is similar in a number of nonrandom ways, including level of complexity and types of mistakes that are more or less likely during acquisition. But of course the well-equipped child must learn a particular language through experience. There may be a language instinct, but there is not a Spanish or Italian instinct. And just as birds have song-imprinting mechanisms that help them narrow their song learning in adaptive ways, there appear to be imprinting-like mechanisms in human language learning as well (Marler, 1970). Although 3-month-old babies can discriminate all the phonetic possibilities of any human language, the ability is lost by 12 months, when infants categorize phonemes just like the adults who speak around them (Werker & Desjardins, 1995).

The Human Brain Is Not Designed to Recognize Words Presented at 250 msec.

It may sound obvious to say that understanding how people process written words may give an incomplete understanding of most of the things humans are naturally inclined to think about. But in the introductory chapter for their influential cognitive psychology textbook, Glass and Holyoak (1986) stated that reading "calls into play virtually every aspect of the cognitive processes that we will explore in this book" (p. 15). The fascination with written words may trace to many sources: Words are abstract representations, ideally suited to a model of pure information processing; words have features that are eminently controllable; and so on.

When social psychologists joined the cognitive revolution, they adopted the approach quite literally. Indeed, experiments on social cognition for a long time consisted of directly importing paradigms from cognitive studies of word recognition, such as the work on lexical priming. Hence, the social stimuli in experiments on impression formation or group prejudice were often rapidly presented words (e.g., Bargh & Pietromonaco, 1982). Along with these paradigms, cognitive social psychologists explicitly adopted a central assumption of traditional cognitive psychology—that the same general cognitive processes applied in like manner to all types of content, with social and nonsocial stimuli of various types being more or less interchangeable (e.g., Markus & Zajonc, 1985).

However, different content categories are processed very differently. Humans are immensely better at attentional, memory, and problem-solving tasks when they involve certain types of content as opposed to others. Indeed, certain types of social information seem to have favored status, and to operate according to different rules from other types of social information. We are quicker to detect an angry than a happy face, for example (Hansen & Hansen, 1988). We are adept at solving otherwise difficult logical problems if they involve the detection of cheaters on social contracts (Cosmides & Tooby, 1992). Even words are processed differently when we think they are about people. Warm goes with friendly, hot goes with sexy, and cold goes with calculating when subjects are thinking about people, but not when they are thinking about metal objects (DeSoto, Hamilton, & Taylor, 1985). And humans are universally much better at learning to transmit, receive, and process spoken language than written words (Liberman, 1988). Many human cultures had no written language, and most humans throughout history, even in cultures with written language, have been illiterate. Just think of the papers written by your undergraduate students, and consider that they are in the top percentile of literacy by the standards of most of their ancestors. Yet these terrible writers pick up the complexities of spoken language with no formal training at all, and humans the world over, literate or illiterate, use language of the same high level of complexity (Pinker, 1994).

From an evolutionary perspective, the human brain was not designed to deal with general information, which can be represented by abstract symbols. It was instead designed to assist our human ancestors to deal in particular ways with the variety of different cognitive tasks normally involved in successful reproduction for a member of a human group. The next two sections consider what we mean by that.

Human Reproduction Is More Than Just Copulation

The bottom line of natural selection is differential reproduction. Because the *sine qua non* of successful reproduction is sexual intercourse, and because sexual reproduction involves a division of tasks between males and females, evolutionary psychologists have developed the reputation of being “obsessed with sex.” However, successful human reproduction involves a diverse array of tasks— making friends, negotiating a status hierarchy (which may involve tasks from throwing a baseball to programming a computer), maintaining long-term relationships, child care (including not just providing resources, but training and supervising the child as he or she negotiates all of the above). And to participate in all these social activities, the would-be reproducer must navigate all sorts of challenges and opportunities in the nonsocial world as well—distinguishing edible from inedible foods, keeping out of the rain and the midsummer sun, finding the way back to camp after a hike, remembering where the fruit trees and good fishing holes are located, and so on. Indeed, there is a relatively high ratio of hours spent in all these other tasks to hours clocked in copulation, even for famous athletes or rock stars.

Thus, although the brain may in one sense be an extension of the gonads, its functions are more complex than directing us toward copulation, and an evolutionary analysis should help us understand all its functions. One of the lessons of evolutionarily informed research, as we suggested earlier, is that we need to expand our view of human motivation beyond the general rule offered by the learning perspective: "Do it if it feels good." It turns out that what feels good depends importantly on what functional motive is active at any given time. In the next section, we consider one way to think about this issue of domain specificity.

Six Minds Are Better Than One

Psychologists' commitment to a domain-general view can be traced to a number of sources, including Lashley's (1929) *equipotentiality theory* of cortical functioning. Lashley found that when increasingly larger areas of the cortex were destroyed in rats, there was gradually greater disruption of memory, and that other areas could sometimes take over function for the damaged area. Such findings stood alongside abundant support for localization in findings such as Broca's observation that aphasics all had damage to the left frontal lobe. The view that brain functions could be localized, however, may have been tainted by a perceived association with phrenology. Another important historical influence was domain-general learning theories. Traditionally, prominent theories of learning assumed that all behaviors, regardless not only of content domain but even of the species manifesting them, could be explained in terms of one or a few simple domain-general mechanisms (e.g., Skinner, 1953).

The assumption of a general learning mechanism that operates according to a few general purpose learning rules is more parsimonious than the assumption of domain-specific mechanisms. And there is no doubt that principles such as classical and operant conditioning have a wide range of applicability. Further, it would be a mistake to assume that each cognitive module must be completely independent of the others, or that the processes used by different modules are all housed in spatially nonoverlapping regions of the brain.

Nevertheless, evolutionary approaches to cognition have generally favored the view that organisms' brains are composed of some number of content-specific mechanisms, designed to deal specifically with particular cognitive problems confronting particular species. Sherry and Schacter (1987) reviewed evidence that different types of memory tasks are sometimes functionally incompatible. For instance, song learning in birds often occurs during a very restricted period in which the bird is sensitive to whatever songs it hears. Several months may pass before the bird actively reproduces the song, and learning of new songs is closed after the critical period. On the other hand, to remember where it stored its food caches, a bird needs to continually update its memory for novel locations, because it may store food in many places over the course of the season. A shared memory process for these two types of learning would be highly inefficient. In fact, each system is associated with particular neural structures. Sherry and Schacter (1987) are conservative in proposing modules, arguing that different systems may evolve only when the demands of one system are incompatible with another. It is also quite feasible that cognitive modules may be incompatible in certain ways, yet share some general processing mechanisms with other modules for certain subtasks.

Beyond the empirical justifications for assuming content-specific mechanisms, evolutionary cognitive psychologists believe there are compelling theoretical reasons (Kenrick, Sadalla, & Keefe, 1998). Tooby and Cosmides (1992) have noted that evolution generally favors specific organs designed to solve particular tasks. Rather than having a general "sense organ," for instance, we have special organs designed to process sound waves, light waves, touch, smell, taste, and temperature. Again, at the neurological level, there is evidence for distinct brain mechanisms for analyzing color, shape, movement, depth, and other complex features of visual stimuli (Livingstone & Hubel, 1988). Human beings also appear to have separate mechanisms for analyzing different features of auditory input, others designed for understanding spoken words, and still others designed for producing spoken words, and so on.

How modular is the mind? The jury is still out on this question. The most fruitful model might posit hundreds of separate processing modules; a smaller number of executive subsystems, or some hierarchical combination of overlapping modules and submodules (e.g., Kenrick, Sadalla, & Keefe, 1998).

Rather than thinking of modules in terms of spatially isolated suborgans in the brain, another approach is to think about the functionally separable types of information involved in different problems that humans have traditionally faced. What are the main categories of adaptive problems with which humans living in social groups have had to concern themselves? A number of researchers have reviewed literature relevant to this question, and there is some overlap in the schemes proposed (Bugental, 2000; Buss, 1999; Kenrick, Li, & Butner, 2003; Kenrick, Neuberg, & Cialdini, 1999). Our overview of this literature led us to postulate six domains: coalition formation, status, self-protection, mate choice, relationship maintenance, and offspring care. Our ancestors would have fared better generally in the game of successful reproduction to the extent that they:

- Belonged to a cooperative group that shared resources and skills
- Gained and maintained status within that group
- Protected themselves from threats from those outside and inside the group
- Chose a fit mate
- Hung onto that mate
- Cared for any children resultant from that mateship

Each of these problem domains is associated with a different fundamental goal, and a different set of decision-making biases, which we summarize in Table 2.1. They are also likely to be associated with different social geometries, as shown in Figure 2.2 (based on Kenrick, Li, & Butner, 2003).

Note, for example, that cooperative alliances are assumed to involve loosely overlapping groups, in which family members share resources with one another, while simultaneously developing alliances with members of other families. There is loose overlap because, given the power of inclusive fitness, if you help a

TABLE 2.1. Domains of Adaptive Problems, Fundamental Goals Associated with Each Domain, Examples of Evolved Decision Constraints, and Evolutionary Principles Underlying Decision Constraint

Social Problem Domain	Fundamental Goal	Evolved Decision Constraint
Coalition formation	To form and maintain cooperative alliances	Cooperation is more likely to the extent that others: (a) share genes, or (b) have shared resources in past.
Status	To gain or maintain respect from, and power over, other group members	Males take more risks to gain and maintain status.
Self-protection	To protect oneself and alliance members against threats to survival or reproduction	Potential threats or costs lead to reciprocal exchange of aggressive behavior, particularly among nonkin.
Mate choice	To obtain a partner or partners who will enhance one's own fitness	Males, compared with females, are generally more inclined toward an unrestricted mating strategy (i.e., multiple mates, shorter courtship before sex)
Relationship maintenance	To maintain a mating bond with a desirable partner	Males are inclined to break a bond if a partner is sexually unfaithful, or if there are physical attractive alternatives available. Females aree inclined to break a bond if a partner compromises resources, or if a high status alternative is available.
Parental care	To promote the survival and reproduction of individuals carrying one's genes	Familial provision of resources and care will follow the order: (a) self>siblings (b) own offspring>stepchildren

member of my family, you indirectly help me. On the other hand, self-protection stems from competition over resources between groups (and sometimes within groups). This conflict leads to the formation of barriers: If you hurt a member of my family or tribe, you thereby reduce my fitness, so you and those associated with you are relegated to the outgroup category with all the nasty implications that follow. Self-protection considerations promote the development of large groups, whereas mating alliances promote the development of dyads, given the male's concern with inadvertently raising offspring that are not his own, and the female's concern with losing the father's resources for her offspring. We review the evidence for these assumptions elsewhere (Kenrick, Li, & Butner, 2003). For the purposes of this chapter, we will note that the decision rules and social geometries associated with each of these domains have implications for connecting evolutionary psychology and complexity theory, as we discuss in the next section.

MINDING OUR MISCONCEPTIONS

In this section, we address two misconceptions about evolutionary psychology: That it is ultimately about isolating genes for particular behaviors, and that it is a perspective that most psychologists don't need to really think about. On the contrary, evolutionary psychology considers dynamic interactions at all levels, and has relevance for the whole field of psychology.

Reductionism Ain't What It Used to Be

Because evolution ultimately boils down to the survival of "selfish genes," evolutionary theorists have often proudly described themselves as reductionists (Wilson, 1998). But a given gene is only naturally selected if it fits within a network of other genes to produce individual cells that, in turn, must function in the context of the network of other cells that make up organs, and these in turn must function in the context of a network of other organs making up a complete organism. Furthermore, that organism must coordinate its behavior with a network of other organisms with which it shares an ecosystem. Evolutionary theorists are acutely aware of these interdependencies, as modern evolutionary biology has coevolved with the development of the field of ecology. Notions such as frequency-dependent selection presume that the success of a given strategy is dynamically linked to the success of other strategies. For example, the ratio of predators to prey is maintained in a delicate balance—too many predators and the prey will begin to disappear, which will in turn reduce the population of predators.

As noted, we believe that the coming paradigm will involve an integration of ideas in evolutionary psychology and cognitive science with those in dynamical systems theory (Kenrick, 2001). Dynamical systems theory is concerned with the study of complex multicomponent systems (ranging from the microscopic—

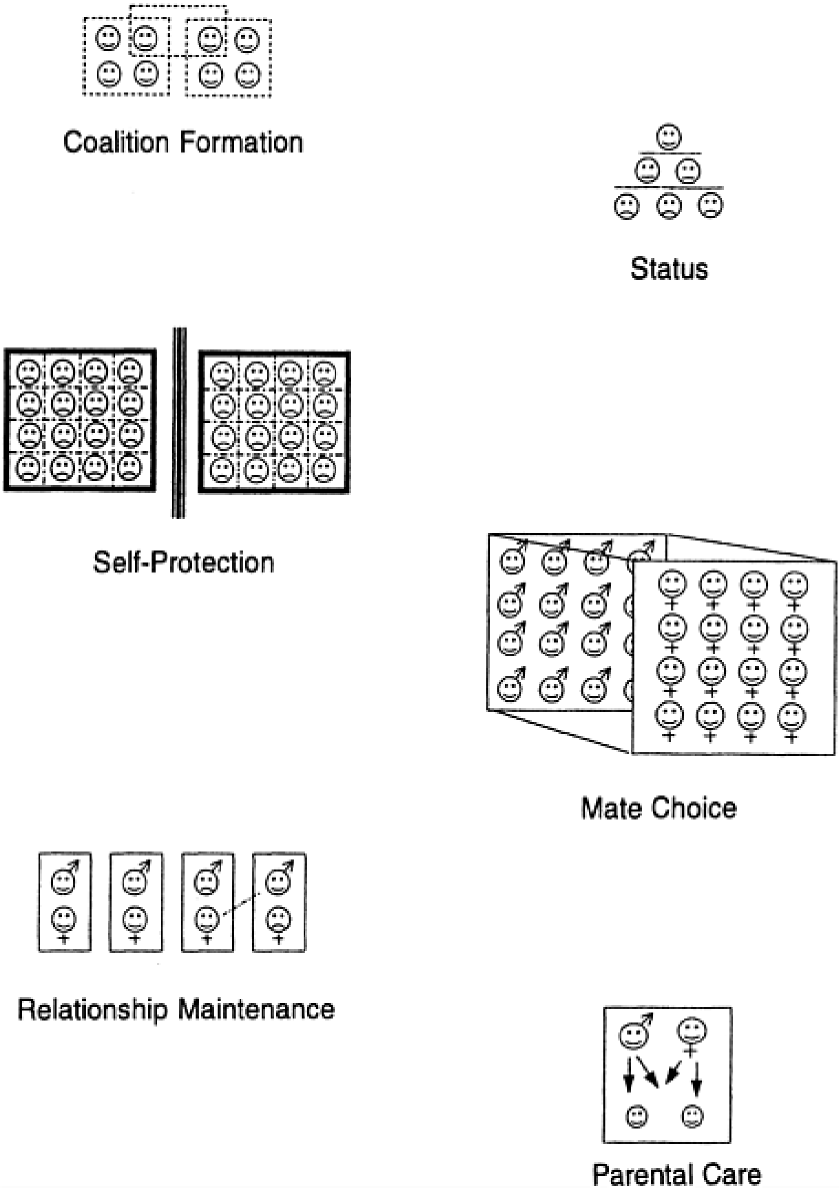


FIGURE 2.2. Different social geometries are associated with decision rules used in different social domains (from Kenrick, Li, & Butner, 2003).

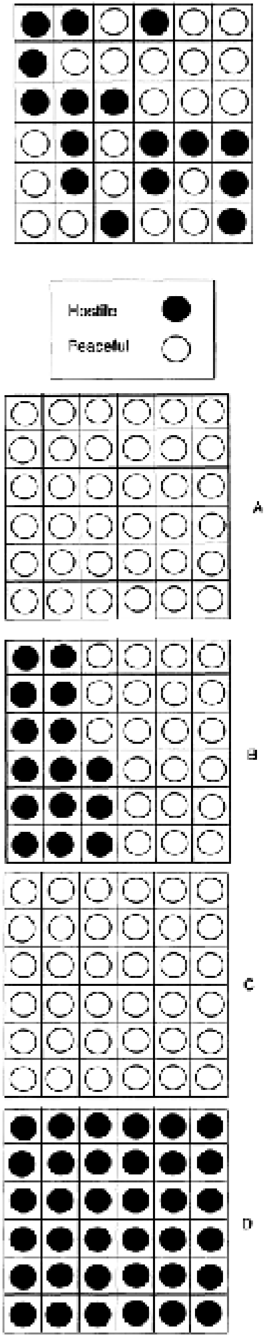
genes or neurons, to the macroscopic—all the animals and plants in a forest ecosystem, or all the millions of consumers in an economic market). Two key features of the dynamical approach are the study of *changes over time* and an emphasis on *nonlinear processes* (Nowak & Vallacher, 1998). A single snapshot is insufficient to understand the balance of predators and prey, plants and animals, parasites and hosts in a forest ecosystem, for example. And the changes that occur in ecosystems are often sudden rather than gradual. For example, the removal or addition of one key species in a coral reef or ponderosa pine forest can radically change relationships between the remaining species, and lead to a sudden and catastrophic alteration of the entire habitat (Wilson, 1992). The dynamic approach thus also emphasizes the *bidirectional causality* found in most natural systems.

Observations across a wide variety of complex systems have yielded a pair of critical insights. First, dazzling complexity can emerge from a few variables interacting according to a few simple rules (Holland, 1998). Second, complex systems at every level reveal *self-organization*—order emerging out of initial disorder (Latané, Nowak, & Liu, 1994). The top left segment of Figure 2.3, for example, depicts a group of individuals engaging in either cooperative or hostile behavior. When each individual attempts to match the majority of his neighbors, the neighborhood ends up completely peaceful after several rounds of interaction (Figure 2.3A, top right). When individuals influence their neighbors, communities often end up organizing into self-maintaining pockets over time, even when initial behaviors are random.

Complexity theorists have discovered broad principles that apply across different types of phenomena, and indeed, natural selection is an excellent example of dynamical self-organization. However, the success of any organism, and its dynamic relationship to the other organisms in its network, are ultimately linked to particulars. The particulars of the decision rules used by a group of organisms, and the particulars of individual differences between them, can have profound effects for network dynamics. As shown in examples B and D in Figure 2.3, one or two individuals with low thresholds for aggressive behavior could dramatically alter the outcomes for the rest of their community.

In another series of simulations, we examined how male and female differences in decision rules about restricted or unrestricted sexual behavior can result in very interesting group dynamics, and how random spatial placement and the possibility of migration can combine with such individual differences to result in the emergence of distinct subcultures (Kenrick, Li, & Butner, 2003). Using normal decision rules, for example, most communities of men and women end up in relatively restricted monogamous relationships. However, if the females in a neighborhood use slightly different decision rules (akin to those used by males, which are slightly more inclined toward unrestricted behavior), a neighborhood will generally become highly unrestricted, even though the males have not changed their decision rules at all. Again, relatively small changes in individual decisional rules can have much larger implications at the societal level.

To summarize this section, a dynamical evolutionary position hardly implies that all psychologists should begin to search for isolated genes. On the other



FIGRUE 2.3. The neighborhood at the left depicts individuals acting eigher ag- gressive or peaceful. If all attempt to match the majority of their neighbors, the neighborhood will self-organize into peaceful and aggressive clusters (or simply go completely over in one direction). If all individuals use a rule of attempting, to matcj the majority of their neighbors, thei initial pattern will stabilize into a totally peaceful neighborhood. If some individuals have very low thresholds for aggression (as marked), this can change outcomes for the whole neighborhood, depending on their location and random variations in their neighbors' behaviours (based on Kenrick, Li, & Butner, 2003).

hand, because dynamical processes at one level often emerge bottom-up from decision-rules affecting behavior at lower levels, this approach also counsels against a content-free holism that eschews any analysis of system components. Even single genes can have effects on the whole system of genes, and ultimately on the development and behavior of the organism (Ridley, 2000; Weiner, 1999). The field of psychology is probably best served by dogmatically embracing neither a reductionism that ignores emergent processes nor a holism that ignores the substrate out of which higher-level patterns emerge. Instead, a full understanding of adaptive problems faced by complex organisms living in groups of other complex organisms will come from simultaneous consideration of multiple levels of causality.

The Dynamical Adaptationist Map: Don't Leave Home Without It

The social sciences are littered with theories that would never have been advanced if their authors had understood the broader principles of evolution by natural selection. These include Freud's ideas about the Oedipus complex, the notion that adult sexual behavior would follow from consistently labeling someone as "male" or "female" or teaching them a particular "sexual script," the *tabula rasa* view of human nature, and the explanations of various sex differences in social behavior in terms of the norms of American culture (e.g., Daly & Wilson, 1988,1990). Furthermore, a number of psychological phenomena, such as the cognitive errors with which psychologists like to taunt their students, take on a completely different significance when considered in light of their value as possible functional adaptations (e.g., Funder, 1987; Haselton & Buss, 2000). Hence, we argue that it is essential, at least on occasion, to consider the broader evolutionary significance of the phenomena we study.

Besides helping us avoid blind alleys, a broader evolutionary perspective can also provide the intellectual satisfaction of seeing how the different roads flow together. Figure 2.4 provides an example of how a number of findings on social behavior can be linked with a broad set of evolutionary principles (from Kenrick & Trost, 1996). Adding just a link or two could probably bring you to any particular behavioral phenomenon of interest.

Just as there are topographic maps and road maps, there are different angles one can take on the increasingly interdisciplinary terrain of psychology. Table 2.2 provides another way to look at the space where an evolutionary psychological view interacts with a dynamical systems view (from Kenrick et al., 2002).

One dimension of the framework in Table 2.2 is equivalent to the proximate-ultimate continuum of causality. The other dimension divides questions into those dealing with the interactions of components inside the organism, and those dealing with interactions between organisms. Events at one of these levels are intrinsically linked to those at the other levels. The connections between attentional, memory, and emotional processes inside the organism are linked in one direction to events in the social environment, and in the other to ontogenetic and phylogenetic history. And each momentary interaction between individual and environment can be seen as one frame in the neverending dynamic story of natural selection. Mathematicians and biologists have begun to model such evolutionary dynamics using dynamic simulations of the sort we have used to study ongoing person-environment interactions (e.g., Killingback & Doebeli, 1996). These sorts of simulations provide tools to enhance the theory-building phase of science, but those tools are most useful when they are educated by experiments and naturalistic research that elucidate the underlying decision rules actually used by human beings making important decisions related to problems of survival and reproduction.

Consider that all the issues that could be raised about any given domain of social life have yet to be addressed for any level of analysis (e.g., the upper left box in Table 2.2). If you further consider that a parallel set of questions could be raised about nonsocial problems, and that the intrinsic interconnections also need to be explored, it becomes clear how ridiculous were the claims a few years back that we are nearing the "end of science." Indeed, when it comes to exploring the human mind, we are still near the beginning of the journey, but we are beginning to see the outlines of a map of the coastline.

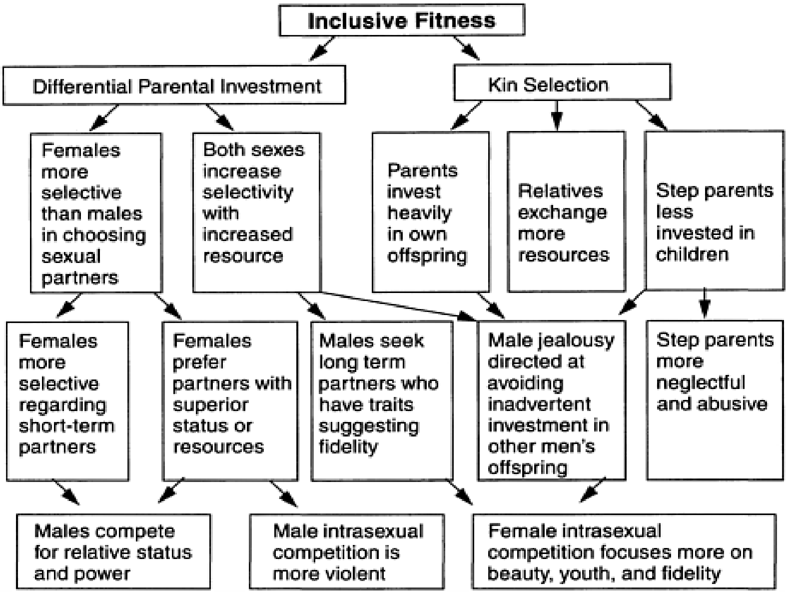


FIGURE 2.4. Patterns of social behavior across a wide range of animal species and human cultures may be linked via broad evolutionary principles. The theory of inclusive fitness is the assumption that natural selection favored animals with traits that led to behaviors favoring replication of their genes in their own offspring and other relatives. A number of midrange theories flow from this, and these can help connect a diverse network of empirical observations. Differential parental investment theory is the assumption that animals are more selective about mating to the extent that they invest in any likely offspring. Selectivity in one sex is presumed to increase competition in the other. Human males and females may both invest in offspring, but they invest somewhat different resources, and these differences affect mate selection criteria. Kin selection theory is based on the assumption that organisms aree selected for traits that favor the investment of resources in genetic relatives, and that disfavor investment of resources in nonrelatives (such as stepchildren). (Based on Kenrick & Trost, 1996).

Rather than being put out of business by opening passageways to evolutionary biology and general complexity theory, those paths help us realize the vastness of the new territories left to explore. And instead of being left on the dock, psychologists who study psychological mechanisms may yet have their day as head scouts in science’s most important expedition to date.

TABLE 2.2. A Framework For Organizing Questions about Dynamics and Evolved Mechanisms

Units	Short-Term Time Frame	Developmental	Evolutionary
Within the person	How do an individual's internal characteristics (motives, problemsolving strategies, hormone levels) interact across situations? *Example:* Activating a self-protection motive re-organizes attention, motivation, and perception.	How does the internal ecology of an individual's characteristics change over the lifespan? *Example:* The linkage between aggressive behavior and testosterone level changes at puberty	How do constellations or syndromes of traits coevolve? *Example:* Inherent connections between female reproductive physiology, parenting behavior, and attitudes about casual sex
Between people	How do the interactions between individuals in interconnected networks self-organize into group level patterns? *Example:* Mutual cooperation or conflict in a social dilemma situation	How do social groups organize and reorganize themselves over the lifespans of the individuals involved? *Example:* Dynamics between the sexes change at puberty.	How does the local population of types mutually constrain one another? *Examples:* Physical and behavioral characteristics of males coevolve with those of females.

Based on Kenrick, Maner, Butner, Li, Becker, and Schaller, 2002.

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3

Strategic Pluralism and Context-Specific Mate Preferences in Humans

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*A*s the chapters in this volume attest, evolutionary principles can explain and integrate much of what is currently known about human social be havior. This is particularly true of mating and parenting, two life tasks that are closely linked to inclusive fitness. At base, evolutionary approaches seek to understand human psychological design—the nature, organization, and operation of specific psychological mechanisms—through identifying plausible constraints imposed by past selection pressures. Human behavior is highly flexible and exquisitely sensitive to environmental stimuli and feedback. Nevertheless, as Tooby and Cosmides (1992) have argued, psychological designs that produce "plasticity" can be retained by selection "only if they have features that guide behavior into the infinitesimally small regions of relatively successful performance with sufficient frequeney" (p. 101). Consequently, in order to understand and explain behavioral flexibility, one must comprehend the psychological architecture that underlies social interaction.

Evolutionary theories of mating—especially Trivers' (1972) theory of sexual selection and parental investment—have clarified the major and slightly different barriers that males and females in many species must surmount to increase their inclusive fitness. Trivers' groundbreaking work, in fact, has served as the foundation for most current theoretical and empirical research on sex differences in human mating (e.g., Sexual Strategies Theory) (Buss & Schmitt, 1993), which primarily has sought to explain why women, relative to men, tend to be more discriminating when choosing mates and more "restricted" in their sociosexual behavior. This emphasis on sex differences, however, has been criticized for: (a) not explaining why there exists so much within-sex variation in mating-related behaviors (Gangestad & Simpson, 1990); and (b) not fully considering how women's control of resources could have affected the evolution of mating strategies in *both* sexes (Gowaty, 1992a; Hrdy, 1997).

In the present chapter, we discuss a new model of human mating—Strategic Pluralism (Gangestad & Simpson, 2000a)—that attempts to explain from an evolutionary perspective how and why *both* sexes utilize short-term and longterm mating tactics in certain environmental contexts and situations. Most current theories of human mating explain why men are more likely to engage in short-term mating than women, and most acknowledge or imply that both sexes *may* have evolved "mixed" (both short-term and long-term) mating strategies. Unfortunately, existing theories have not fully specified the conditions and decision rules that motivate the use of different strategies. Guided by Gangestad and Simpson's (2000a) model of strategic pluralism, we review recent evidence indicating that selection pressures produced *mixed* mating strategies in both sexes, with strategy choice being conditional on certain personal (dispositional) and environmental factors.

The chapter is divided into six sections. In the first section, we review some basic evolutionary concepts relevant to mating, including sexual selection, adaptations, trade-offs, and their adaptive roles. In the second section, we briefly summarize current theoretical accounts of sex differences in short-term mating in humans. We indicate how these accounts do not fully incorporate the important concept of "trade-offs" to explain the large amount of within-sex variation in mating tactics. In the third section, we elaborate on how and why genetic fitness and "good-genes" sexual selection ought to affect mating decisions and mating behavior. In the fourth section, we overview recent evidence suggesting that good-genes sexual selection might have operated on humans, a topic that has been controversial in evolutionary biology. In the fifth section, we present the major principles of Gangestad and Simpson's (2000a) model of strategic pluralism. In particular, we discuss the way in which "good-genes" and good-provider sexual selection should jointly affect how both sexes make tradeoffs in mating contexts and, thus, why individuals engage in different mating tactics. We also discuss how personal (dispositional) and environmental factors might influence the conditions under which short-term and long-term mating tactics are displayed by both sexes. Some concluding comments are offered in the final section.

EVOLUTIONARY CONCEPTS

Sexual Selection and Adaptations

Let us begin by defining some important evolutionary concepts. Sexual selection refers to differences in reproduction among individuals owing to differen tial advantages in mating, independent of advantages associated with differential survival. Sexual selection generates two types of adaptations (Andersson, 1994): (a) intrasexual competitive abilities; and (b) intersexual attraction cues (e.g., signals that most opposite-sex people find appealing in mates). In many species, the number of mates that males attract strongly impacts their reproductive success, whereas total mate number has less impact on the reproductive success of females (because the reproductive output of females is limited by gestation and lactation). In most mammals, therefore, females tend to be a “limited reproductive resource” over whom males compete. As a result, sexual selection pressures are believed to have operated more strongly on male intrasexual competitive abilities and male intersexual attraction cues than vice versa (see Cronin, 1991; Trivers, 1972). Evidence supporting these predictions has been marshaled for many species (for reviews, see Andersson, 1994; Trivers, 1985).

Theories of intersexual signaling have focused on two clusters of attributes: (a) signals conveying qualities of a good parent (or a “good provider”); and (b) signals that honestly advertise “good genes” (Cronin, 1991; Gangestad, 1993). Historically, theories of good parenting have been fairly uncontroversial. Those involving good genes, on the other hand, have been debated extensively. Because of this, good genes principles have not been applied to theories of human mating. Recent work, however, has suggested that good-genes selection could have operated on many species (Kirkpatrick, 1996; Møller, 1994), even those in which males provide substantial parental care (as is true of humans). This new evidence makes it possible to consider whether good-parenting and good-genes selection processes may have jointly generated mixed mating strategies within women and men.[1]

Adaptations are traits or behaviors that gave certain individuals a genetransmitting advantage over other individuals who possessed different variants of the traits/behaviors in the environment of evolutionary adaptedness (the EEA). Adaptations are identified by evidence of their “special design” (Simpson & Gangestad, 2001; Williams, 1966). A trait or behavior contains evidence of special design if it produces a particular beneficial effective (that, on average, should have enhanced inclusive fitness in the EEA) with a high degree of specificity, efficiency, and economy. Tooby and Cosmides (1992) have claimed that adaptive behavioral flexibility should be facilitated by domain-specific psychological mechanisms. They contend that these mechanisms should operate via specific decision rules that are triggered by specific environmental cues and that produce efficient, stable, persistent, and finely tuned responses (e.g., specific perceptions or behavioral reactions) that solved recurrent adaptive problems in evolutionary history (e.g., choosing or attracting mates). These decision rules are not necessarily conscious or premeditated; indeed, most may operate outside of awareness. For instance, the reasons why a person is attracted to a potential mate, suspicions about a mate’s possible infidelity, or interpretations of flirtatious glances are all responses to specific environmental cues that are likely to be governed by implicit, unconscious decision rules.[2]

Evolutionary Functional Analysis and Trade-Offs

Prior to the mid- 1960s, adaptations were viewed almost exclusively in terms of the benefits they bestowed on organisms without complete recognition of associated costs (see Cronin, 1991). Individuals must invest considerable time, effort, and energy to accomplish the major life-tasks that most directly affect their inclusive fitness: surviving to reproductive age, reproducing successfully, and then successfully rearing offspring. Decisions about how to invest time, effort, and energy are important not only because they can put individuals at risk (potentially decreasing their future reproductive success, despite short-term benefits), but because individuals could have used or allocated their resources differently. Hence, adaptations have "opportunity costs"—costs owing to lost fitness benefits that *could* have been gained by using resources differently. This suggests that benefits must always be evaluated in relation to costs to determine whether and how specific adaptations might have evolved (Parker & Maynard Smith, 1991). The primary objective of evolutionary functional analysis is to identify the cost-benefit "trade-offs" that motivated individuals to allocate their time, energy, and effort to activities in ways that typically should have increased their inclusive fitness in the EEA.

Trade-offs are particularly evident in the allocation of effort to parenting versus mating activities. Trivers (1972) defined parental investment as

> any investment by the parent in an individual offspring that increases the offspring's chance of surviving (and hence reproductive success) at the cost of the parent's ability to invest in other [including future] offspring. (p. 139)

Thus, even though parental investment can increase the probability that offspring will survive and reproduce (yielding greater fitness), it also carries costs in terms of the lost benefits of alternative investments (e.g., missed opportunities to invest in other offspring). Individuals who engage in high levels of parental effort could engage in other pursuits with their limited time and energy, such as devoting more effort to mating activities. As a result, costs include the lost benefits of potentially productive yet foregone activities (Gross & Sargent, 1985).

If fitness gains from mating effort tend to be larger for one sex in a species, selection pressures should lead that sex to devote relatively more effort to mating than parenting (Low, 1978). Because obtaining more mates (a component of mating effort) cannot produce large fitness gains for females in many species (Bateman, 1948), most females in those species should devote more time and energy to parental investment than the average male.[3] Conversely, most males should devote a larger share of their time, energy, and effort to finding and attracting mates than the average female. Trade-offs, therefore, are implicit in the notion that males and females should typically differ in the total reproductive effort they allot to mating. However, the notion of trade-offs also implies that males should *differ from one another* in the total amount of reproductive effort they devote to mating.

EXISTING THEORIES OF SEXUAL STRATEGIES

Sexual Strategies

Sexual strategies (or mating strategies) are integrated sets of adaptations that organize and guide an individual's reproductive effort. They influence how individuals select mates, how much effort they expend on mating, how much effort they expend on parenting, and so on. As discussed, sexual strategies are not necessarily consciously formulated or even accessible to awareness. They are conceptualized as genetically based programs (i.e., decision rules) that individuals use to allocate their somatic and reproductive effort to specific mating tactics in adaptive ways. Mating tactics are the specific actions and behaviors that individuals display when pursuing a given strategy. The use of a given mating strategy usually involves multiple behavioral tactics (Dominey, 1984). For example, males in many biparentally investing species invest heavily in offspring, yet remain open to low-cost mating opportunities with other females. According to Trivers (1972), this is a *mixed* mating strategy consisting of multiple behavioral tactics. If each tactic is elicited by specific environmental stimuli (e.g., the prolonged absence of a mate, having short-term sex only with mates who possess certain attributes, seeking short-term sex only when such efforts have been beneficial in the past), the strategy is a *conditional* one.

Considering the different and changing environments in which reproduction occurs, selection pressures probably did not produce a single "best" mating tactic for males and females in most species. Rather, selection should have generated a diversity of mating tactics. Informed by game theory (Maynard Smith, 1982) and research on evolutionary stable strategies (Dawkins, 1980; Parker, 1984), evolutionary biologists have recently documented that the use of alternative mating tactics tends to be associated with increases in reproductive fitness for females and males in many different species. Although research is just beginning to examine alternative mating tactics in humans, hundreds of studies have confirmed that males and females in a diverse array of species display alternative mating tactics reflecting conditional strategies (Gross, 1996).

Conditional strategies have five properties (Gross, 1996): (a) They involve different behavioral tactics that are either consciously or unconsciously chosen by an individual; (b) the choices between tactics are made in response to specific features or cues in the environment, often based on an individuals status or attractiveness relative to other individuals; (c) all individuals are genetically monomorphic (i.e., they are genetically designed to enact the same set of tactics); (d) during their evolution, the average adaptive values of different tactics representing specific strategies were not equal except at some "switch point" on a continuum of environmental input (e.g., individuals' relative status) at which the costs and benefits of each tactic balanced out; and (e) during their evolution, the chosen tactic yielded higher fitness for the individual than other tactics, given current environmental conditions. Environmental conditions, therefore, can moderate the fitness gains of using different tactics (e.g., exerting parental effort versus pursuing short-term matings), thereby affecting the optimal allocation of effort to different mating strategies.

Sex Differences in Mating Strategies

Until very recently, most evolutionary theories of human mating have focused on the use of different mating strategies by men and women. Influenced by Trivers' (1972) theorizing, Wilson (1978) suggested that, because of biologically based differences in how the sexes reproduce, males should have evolved to be *uniformly* aggressive, hasty, fickle, and undiscriminating in mating contexts, whereas females should be *uniformly* coy and should defer mating until males with the best genes are identified.

Sexual Strategies Theory (SST) (Buss & Schmitt, 1993) is based on similar notions. SST acknowledges that both sexes should have evolved tactics for obtaining long-term mates and investing in offspring. Similar to Wilson's theory, however, SST predicts that men should have evolved a strong desire for multiple mates:

> What specific adaptations should be expected in the evolved sexual psychology of men to solve the problem of gaining sexual access to a number of women? One first-line solution to the problem of number can be expected in desire: Men may have evolved over human evolutionary history a powerful desire for sexual access to a large number of women. (cf. Symons, 1979). [Buss & Schmitt, 1993, p. 208]

This prediction involves an implicit cost-benefit analysis involving tradeoffs between different activities that could have been optimal for both sexes in evolutionary history. According to SST, men on average experienced a greater net benefit than women for attempting to attract multiple mates, and this prediction has received empirical support across several studies (see Buss & Schmitt, 1993).

Although Buss and Schmitt do not state that all men possess a "powerful desire" for multiple mates, they never claim that that some men simply do not. Clearly, a mean sex difference—even a large one—does not imply a nearuniversal propensity in each sex. In fact, a mean sex difference could exist even if the modal response of each sex is similar. Sex differences in the desire for multiple sex partners and interest in short-term mating tend to be moderate in magnitude. Buss and Schmitt (1993) report that biological sex accounts for 16% of the variance in seeking short-term mates, 9% of the variance in the number of sex partners desired in a specified time, and 20% of the variance in the probability of consenting to sex after knowing an attractive, opposite-sex person for a given period of time. In a meta-analysis, Oliver and Hyde (1996) found that the mean sex difference in interest in casual sex accounted for 25% of the variance. In multiple college samples, Simpson and Gangestad (1991) found that sex accounts for 8% to 20% of the variance in expressed willingness to engage in sex without commitment.

Even though these effect sizes are reasonably large, substantial within-sex variation and overlap exists between the distributions of women and men. For example, despite the mean sex difference in the desire for sex without commitment (Simpson & Gangestad, 1991), approximately 30% of men express *less* favorable attitudes about casual sex than the median attitudes of women. Substantial overlap also exists for many other measures, including interest in sex with multiple partners and optimal time before having sex (Miller & Fishkin, 1997). General claims that women desire certain mating arrangements, whereas men desire others raise the questions "which men?" and "which women?" (Gowaty, 1992a). If the within-sex variation in mating strategies is adaptive, the environmental contingencies in the EEA that led men and women to selectively adopt short-term and long-term mating tactics must be articulated.

Cost-Benefit Considerations

If aggressiveness and interest in short-term mating is the "most profitable" strategy for men, why don't all men pursue short-term mates? As we have discussed, differential cost-benefit trade-offs should have led men to pursue more mates *on average* than women and, accordingly, men should dedicate more time and energy to mating effort *on average* than women. These principles, however, do not imply that the best strategy for most men would involve high mating effort and the best strategy for most women would entail high parental effort. A consideration of trade-offs suggests that the best strategy for men may not always— or even usually—involve devoting concerted effort to short-term mating (Kitcher, 1985). Moreover, the best strategy for women may not always be to focus less effort on short-term mating than the average male.

The idea that men, on average, evolved to engage in greater mating effort than women correctly assumes that some ancestral males *could* have increased their fitness by trying to attract multiple mates. However, the more stringent notion that the best evolutionary strategy for most men should have been to pursue multiple mates assumes that most men could have been successful at short-term mating. This is doubtful. Men's short-term mating success should have depended partly on which attributes women desired in short-term mates. Relatively few men may have been able to satisfy these preferences. For some men, therefore, one cost of engaging in short-term mating would have been the loss of opportunities to engage in parental investment (which, most likely, human males also evolved to do) (Alexander & Noonan, 1979; Lancaster & Lancaster, 1983). If short-term mating effort consistently generated poor pay-offs for men who did not have the attributes most women wanted in short-term mates, these costs should have outweighed the possible benefits of pursuing short-term mating. As a result, the most profitable mating strategy for the *typical* man may have been to dedicate most of his reproductive effort to enhancing the phenotypic quality of his offspring through heavy paternal investment, even though a small number of men with certain desirable attributes might have been more successful by pursuing short-term matings.

If the most profitable evolutionary strategy for men depended partly the attributes each man possessed (e.g., his ability to attract multiple, short-term mates), which mating strategy should have evolved in men? Most likely a conditional one. Most men should have profited from investing in a primary mate (or small set of mates) and in subsequent offspring. When men could obtain shortterm, opportunistic matings with women other than their primary mates, they could have profited by pursuing the sex-typical strategy described by Wilson (1978) and Buss and Schmitt (1993). When they could not, however, men should have allocated more time, energy, and resources to long-term mating, parental investment, and perhaps mate guarding in order to attract and retain one mate and derive the benefits of parental investment. Thus, although men may have "evolved over evolutionary history [*the capacity for*] a powerful desire for sexual access to a large number of women" (Buss & Schmitt, 1993, p. 208; our addition and emphasis), the desire for short-term mating should be expressed conditionally. The key adaptation should be a set of decision rules about when and how to allocate reproductive effort wisely and contingently, rather than a universal desire for short-term mating.[4]

What attributes have women evolved to desire in short-term mates? In the next section, we review evidence indicating that "good-genes" sexual selection may help to answer this question. These selection pressures may also have important implications for understanding variation in women's interest in shortterm mating.

GENETIC FITNESS AND SEXUAL SELECTION

Males provide little or no parental investment in many species. Yet even in these species, females are selective and consistently prefer some males over others as mates. "Good-genes" sexual selection (GGSS) can explain female mate preferences in species with low paternal investment (Cronin, 1991). According to models of good genes selection, females have evolved to prefer males who have indicators of viability and good physical condition, especially adaptive attributes that might be passed on to their offspring through genetic inheritance.

Genetic Variation in Fitness

In order for mate preferences based on heritable fitness to evolve, additive genetic variance must underlie fitness (i.e., fitness must be transmitted genetically across generations). GGSS has been controversial mainly because selec-tion tends to lower the heritability of fitness to zero (Kirkpatrick, 1985; 1986). However, new empirical and theoretical developments now suggest that

GGSS can and probably has occurred in a variety of species (see Kirkpatrick, 1996).

In recent years, it has become possible to estimate the amount of genetic variation in fitness (or fitness components) in natural populations. The most relevant measure of genetic variation is the additive genetic coefficient of variation (CV_a), the genetic standard deviation of a trait, standardized by the trait mean and multiplied by 100 (Houle, 1992). This measure indexes the amount of genetic variance for a given trait that is transmitted across generations. Houle (1992) has argued that the CV_as of fitness traits (e.g., longevity, fecundity) in natural populations are typically *greater* than those of ordinary morphological traits (e.g., height). Human fecundity, for example, has a CV_a that is four to five *times* larger than that of human height (Burt, 1995; see also Pomiankowski & Møller, 1995).

New theoretical advances have also clarified that two opposing forces affect the amount of genetic variance in fitness within a population: natural selection (which removes genetic variation), and forces that degrade an organism's fitness, such as mutations and environmental change (both of which increase genetic variation) (Fisher, 1958). If deleterious mutations and environmental change remain negligible, natural selection should keep genetic variance near zero. A substantial amount of genetic variance in fitness, however, can be maintained by mutations (cf. Charlesworth, 1990). Thus, mutations could produce more genetic variance in fitness than is observed in most ordinary traits.

Recent theoretical advances have also focused on phenomena that induce rapid change in the selective environments of organisms. One powerful set of selective forces operating on humans are pathogens. Because parasites evolve in response to their hosts' defenses, no set of defenses is evolutionarily stable; changing parasites is always challenging them. These rapid shifts in the selective environments of hosts ensure that hosts will never be free of maladaptations caused by parasites. They also ensure that hosts will vary in their ability to resist parasites. Accordingly, host-parasite coevolution can also maintain genetic variance in host fitness (Anderson & May, 1982; Hamilton, 1982; Tooby, 1982).[5] Because of these processes, both mutations and host-parasite coevolution can generate and maintain a sizable amount of genetic variance in populations, and they can explain why certain fitness traits have large amounts of genetic variance. Recent theoretical modeling, in fact, indicates that large amounts of genetic variance in fitness not only can but *do* produce GGSS (Kirkpatrick, 1996).

Markers of Heritable Fitness

Mildly deleterious alleles and lower pathogen resistance must be inferred from phenotypic markers (e.g., "advertisements") (Zahavi, 1975). In order to operate, GGSS must be based on "honest" signaling, which explains why only certain features serve as valid indicators of individuals' physical condition and, potentially, their genetic fitness (Grafen, 1990; Zahavi, 1975). If an attribute is a genuine marker of heritable fitness, sexual selection should favor new genes that simulate the valued feature, even among individuals who are less fit. In other words, "cheaters" (individuals who display the selected phenotypic traits or behaviors but do not have high fitness) should attempt to fake the valued attributes and invade the population. Over time, selection should gradually eliminate these attributes as valid cues of heritable fitness, and preferences for them should diminish. Under certain conditions, however, an attribute's link with heritable fitness can remain honest and stable across time. For example, an attribute can remain an "honest" advertisement if individuals who have deleterious alleles or less pathogen resistance cannot develop or maintain the attribute without incurring heavy personal costs. One set of attributes that meet this criterion are those that conditionally "handicap" individuals who have mutations or are less pathogen resistant.

Both mutations (Pomiankowski, Iwasa, & Nee, 1991) and pathogens (Hamilton & Zuk, 1982) divert an individuals energy and resources. Accordingly, honestly advertised traits are energetically costly to develop and maintain because such "handicapped" individuals cannot develop these traits without diverting valuable resources from other competing demands (such as sustaining their already taxed immune systems) (Folstad & Karter, 1992). In peacocks and other birds with extravagant features, costly handicaps include exaggerated sexual ornaments and colorful plumage (Zuk, Thornhill, Ligon, & Johnson, 1990). In mammals, they often include large size and increased musculature resulting in sexual dimorphism for size, particularly in polygynous species (Alexander, Hoogland, Howard, Noonan, & Sherman, 1979).[6]

Trivers (1972) proposed that intrasexual competitive abilities could have evolved to be valid cues of heritable fitness. Successful intrasexual competition requires developing costly attributes used in competition (e.g., muscularity, social dominance) and the expenditure of considerable energy during competition. Because males possessing higher viability should be more capable of withstanding the costs of "handicapping" traits than less viable males, they should be able to devote more energy to developing the physical tools necessary to succeed in intrasexual competition. Females, in turn, might have evolved to attend to the outcomes of intrasexual competitions in order to evaluate male fitness (Andersson, 1994).

Testing Good-Genes Sexual Selection

To test GGSS, direct indicators of individuals' genetic fitness ideally should be associated with their attractiveness as mates (particularly as short-term mates). Unfortunately, perfect indicators of fitness do not exist. Tests of GGSS, therefore, must rely on indirect, fallible markers of genetic fitness. The best available measure may be fluctuating asymmetry.

Fluctuating asymmetry (FA) reflects the degree to which individuals deviate from perfect symmetry on bilateral features (e.g., in humans, both ears, both feet; in other species, bilateral fins, bilateral tail feathers) (see Van Valen, 1962). Greater asymmetry indicates deviations in developmental design resulting from the disruptive effects of environmental or genetic abnormalities encountered during development (i.e., developmental instability) (Palmer & Strobeck, 1986; Parsons, 1990; Soule, 1982). Because these disturbances lower reproductive fitness, pronounced asymmetry reveals maladaptation (Møller & Swaddle, 1997). Recent meta-analyses and reviews have shown that greater asymmetry is associated with lower fecundity, slower growth, and poorer survival in many species (Leung & Forbes, 1996; Møller, 1997; Thornhill & Møller, 1997). FA should be a good marker of genetic fitness because an individuals degree of asymmetry should be affected primarily by: (a) mutations (that cause lower metabolic efficiency and imprecise development); and (b) diseases (that reflect an individuals pathogen-resistance; see Møller, 1992). Consequently, FA is a marker of both genetic and nongenetic variation in fitness. Because sexual selection should lead females to mate with males who possess phenotypic traits signaling greater fitness, the association between FA and mating success has been studied extensively. A recent meta-analysis has revealed that, on average, more symmetrical individuals have greater mating success (Møller & Thornhill, 1998).

GGSS, of course, is not the only form of selection that could explain these findings. In some species, more symmetrical males may provide greater material benefits (e.g., more direct and/or better parental care, more physical protection of young). In addition, females may find less symmetrical males less attractive to avoid contracting infectious diseases from them. However, three novel findings suggest that GGSS may have occurred in humans.

First, across many species, symmetry is partly heritable (Møller &Thornhill, 1997). Gangestad and Thornhill (1999), in fact, have estimated that the CV_a of developmental imprecision is approximately 15 to 20, about the same size as other fitness traits and much greater than ordinary morphological traits (e.g., height). If part of this genetic variance is related to fitness, the favored status of symmetrical males could reflect GGSS (Kirkpatrick, 1996).

Second, in some species in which symmetry predicts male mating success, more symmetrical males are favored as extra-pair mates, even when they pro vide few or no material benefits. For instance, even though male and female European barn swallows mate seasonally and rear offspring together, their rate of extra-pair paternity is approximately 35% (Møller, 1994). More symmetrical male barn swallows are the main beneficiaries of extra-pair mating, yet they do not provide material benefits that enhance the reproductive success of their female mates (Møller, 1994).

Third, in some species, more symmetrical males provide fewer material benefits to their primary mates. For example, more symmetrical male barn swallows spend less time feeding their offspring than do less symmetrical males, and they do not compensate with heightened feeding efficiency. In a review of 18 bird species, Møller and Thornhill (1997) have documented that when the rate of extra-pair paternity in a species is high (i.e., when males can benefit more from trying to attract extra-pair mates), attractive males perform a smaller proportion of offspring feedings compared to less attractive males. Exerting greater extra-pair mating effort should yield larger payoffs for more attractive males, which detracts from the time they spend providing direct parental care. Gangestad and Simpson (2000a) have proposed that this type of trade-off also occurs in humans. During evolutionary history, men who had indicators of genotypic quality should have experienced larger gains in fitness payoffs associated with short-term mating than men who lacked these indicators. Moreover, men should have evolved to conditionally "decide" to allocate more versus less effort to mating or parenting, depending in part on the degree to which they had these features.

EVIDENCE FOR GOOD-GENES SEXUAL SELECTION IN HUMANS

Mounting evidence is beginning to indicate that GGSS could have shaped the mating preferences and practices of humans. Much of this evidence suggests that fluctuating asymmetry (FA) is systematically associated with markers of health and mating success, particularly in males. Although FA is not the only possible marker of heritable fitness, it is the best marker currently available. This research has also identified some of the visible attributes (e.g., physicality, social dominance, intrasexual competition tactics) that may serve as the proximate cues "advertising" an individual's genetic fitness. We now review this evidence.

Fluctuating Asymmetry and Health

If FA is a valid indicator of health status, it should covary with markers of current and future health. Rhodes et al. (2001) have found that greater facial symmetry (which tends to be a weaker marker of developmental imprecision than body symmetry) predicts perceptions of better current health, but not actual long-term health outcomes in young adults. Recent studies conducted in regions where macroparasites abound and preventative medicines (e.g., antibiotics) are not commonly used have found more direct support for links between FA and health status. Studying the Ache in South America, Hurtado and Hill (2001) have found that less symmetrical individuals have higher levels of immunoglobulin IgE in their blood, which most likely indicates the presence of greater macroparasitic infestation. Studying people on the remote island of Dominica in the Caribbean, Flinn and Leone (2001) report that children with higher levels of water-borne parasites become less symmetrical over a 6-month period. This suggests that pathogen load may actually cause greater asymmetry. These two field-based studies offer the most direct evidence to date that FA may be a valid indicator of general health status.

Fluctuating Asymmetry and Sexual Behavior

If women evolved to prefer men with indicators of genetic fitness, more symmetrical men should have more lifetime sexual partners (see Perusse, 1993, 1994). Thornhill and Gangestad (1994) measured the symmetry of men and women on seven bilateral features (foot width, ankle width, hand width, wrist width, elbow width, ear width, and ear length) using digital body calipers. They then aggregated these measures (each standardized by feature size) to form a global index of FA. More symmetrical men reported more lifetime partners than did less symmetrical men. This effect remained significant when several potential artifacts (e.g., age, height, ethnicity, marital status) were statistically controlled. Similar effects have been documented in several other studies (see Gangestad & Thornhill, 1999). Studying young Mayan men in remote areas of Belize in Central America, Waynforth (1998, personal communication) has found that more symmetrical men have higher fertility than less symmetrical men. Supporting the notion that women should be less able to convert intrasexual competitive advantages into increased numbers of mates (Trivers, 1972), most studies have found no relation between women's FA and their number of lifetime sex partners (Gangestad & Thornhill, 1997a).

Fluctuating Asymmetry and Men's Extra-Pair Sex

Extra-pair sex (i.e., sex outside a current relationship) is one form of shortterm, opportunistic mating. If women evolved to desire men with greater heritable fitness (independent of the investment and resources such men might provide), more symmetrical men should have more extra-pair mates (Benshoof & Thornhill, 1979; Smith, 1984). Gangestad and Thornhill (1997b) found that more symmetrical men do report having more extra-pair sex partners, even when potential artifacts were controlled (e.g., social status, SES in family of origin, anticipated future salary). Neither men's social status nor their resources (indexed by their SES and anticipated future salary) predicted their frequency of extrapair sex.[7]

Fluctuating Asymmetry and Women's Extra-Pair Sex

More symmetrical men should also be preferred by women as extra-pair mates (i.e., they should be chosen as mates by women who are already involved in established relationships). Indeed, Gangestad and Thornhill (1997b) found that men's FA predicted the number of times they had been "chosen" as extra-pair partners by women who were simultaneously involved in other, ongoing relationships.

Fluctuating Asymmetry and Associated Cues

What personal attributes should mediate the link between men's FA and their number of lifetime sex partners? One possibility is physical attractiveness (Feingold, 1990), which is important to women evaluating short-term mates (Buss & Schmitt, 1993) and is particularly important to women choosing extrapair mates (Scheib, 2001). In fact, a man's attractiveness in short-term mating contexts is just as important to women as a woman's attractiveness is to men when men evaluate long-term mates. Hence, attractiveness might have been one indicator of genetic fitness in ancestral environments.

If preferences for attractiveness evolved in response to GGSS, attractiveness should be correlated with markers of heritable fitness (such as FA). Gangestad, Thornhill, and Yeo (1994) measured individuals' FA and then had coders rate their physical attractiveness from facial photographs. After controlling for potential artifacts (e.g., age, height), FA significantly predicted men's facial attractiveness, but not women 's. That is, more symmetrical men were rated as more physically attractive. This sex difference has been replicated (Thornhill & Gangestad, 1994), but has not been found consistently (Gangestad & Thornhill, 1997a). Across several studies, the correlation between facial attractiveness and body FA tends to be significant but rather small. Studies of the association between facial symmetry and facial attractiveness have also yielded mixed but generally positive results (e.g., Grammer & Thornhill, 1994; Kowner, 1996; Mealey, Bridgestock, & Townsend, 1999; Scheib, Gangestad, & Thornhill, 1999).

Because facial attractiveness mediates only a portion of the relation between men's FA and their sexual history, Gangestad and Thornhill (1997a) tested three other potential mediators, namely three sexually dimorphic traits involved in intrasexual competition: (1) *Body mass:* Humans show moderate sexual dimorphism, consistent with their purported ancestral polygyny (Alexander et al., 1979); (2) *Physicality:* A measure of men's muscularity, robustness, and vigor, as rated by both men and their romantic partners; and (3) *Social dominance:* A self-report measure based on the California Adult Q-Sort. A significant effect was found between men's FA and their number of lifetime partners. In addition, all three traits associated with intrasexual competition also correlated with men's FA. The indirect effects mediated through these traits accounted for over 70% of the total effect of FA on number of lifetime partners. Women's FA did not predict their number of lifetime partners and was not correlated with any of the three intrasexual competitive traits.

The fact that men's FA covaries with traits that should have facilitated intrasexual competition in ancestral environments is consistent with other research showing that more symmetrical men engage in more physical fights, particularly those they initiate (Furlow, Gangestad, & Armijo-Prewitt, 1998). Thus, in addition to affecting success in intrasexual competitions, these traits may also serve as cues that women use to evaluate men as potential short-term mates.

Men who have traits that facilitate intrasexual competitive success should benefit by directly comparing themselves with their competitors. Hence, Simpson, Gangestad, Christensen, and Leck (1999) predicted that, when competing for a date, more symmetrical men ought to use direct competition tactics (e.g., directly comparing themselves with, and derogating, their competitors). After measuring their FA, Simpson et al. had men compete with other men for a lunch date with an attractive woman. Each man was asked a series of questions over a video system by one of two female interviewers (actually videotaped experimental assistants) located in another room. After the interview was finished, the female asked each man to tell the "competitor" (ostensibly located in a different room) why she should choose him instead of the competitor. Each interaction was videotaped and coded for intrasexual competition tactics. Relative to less symmetrical men, more symmetrical men were more likely to directly compare themselves with and belittle the competitor. These results support the prediction that more symmetrical men should display direct intrasexual competition tactics, and they provide further evidence about the proximate cues that may "advertise" genotypic quality.

Finally, intellectual ability may also be a marker of developmental precision and health. In two studies, Furlow, Armijo-Prewitt, Gangestad, and Thornhill (1997) found that more symmetrical individuals (both women and men) score higher on a measure of fluid intelligence (e.g., the ability to solve novel problems). The size of this effect was not different for the sexes.

Women Preferring Symmetrical Men for Their Genes

The fact that more symmetrical men have more mates could be attributable to selection processes other than GGSS. More symmetrical men, for example, might be advantaged because of the superior material benefits they provide to their mates or because they are more successful in intrasexual competitions (Kirkpatrick & Ryan, 1991). However, additional evidence suggests that these alternative selection processes cannot fully explain the greater short-term mating success of more symmetrical men. This evidence indicates that women's preferences for more symmetrical men may be "specially designed" to favor acquisition of their "good genes."

Women's Olfactory Preferences Across the Reproductive Cycle. If women choose extra-pair partners for their good genes, women's extra-pair sex should occur during the most fertile time of their reproductive cycle, whereas their in-pair sex (i.e., sex with current, long-term partners) should not. Bellis and Baker (1990) documented this pattern in British women. To the extent that women's patterns of extra-pair sex favor the sperm of extra-pair mates, this effect might be mediated by women's preferences for specific attributes valued in extra-pair partners, particularly those correlated with symmetry.

Olfactory cues strongly influence mate preferences in many species (Alcock, 1993). Based on evidence that women's olfactory sensitivities and preferences change across the menstrual cycle (Grammer, 1993), Gangestad and Thornhill (1998b) hypothesized that women should find the smell of more symmetrical men more appealing during ovulation. Men were measured for FA and then asked to wear a nonscented T-shirt for two nights. Women smelled each shirt (blind to all other characteristics of the men), and rated how attractive they found the odor of each shirt. Women's fertility was estimated from their probability of conception when they participated in the study, based on medical data (Jöchle, 1973). Women taking the pill were excluded from the study. The shirts worn by more symmetrical men were rated as smelling better than those worn by less symmetrical men, but only among women who were in the fertile phase of their reproductive cycle. This effect has been replicated in at least four samples in Europe and the United States (see Thornhill & Gangestad, 1999). These results confirm a specific prediction derived only from GGSS, and they offer further clues about the proximate signals that may "advertise" male symmetry.[8]

In sum, although FA is only one potential marker of good genes, it systematically covaries with a constellation of other more visible cues that women appear to utilize when making strategic mating decisions. The relevance of male symmetry and the psychological mechanisms that guide female mating decisions are likely to be the products of different selective forces, but the two are not totally independent given the intrinsically dyadic nature of mate choice. Greater symmetry in men appears to be "honestly" advertised by certain visible cues that should be fairly reliable markers of good genes, and these cues in turn are what many women find attractive in short-term mates. Similar to a lens model, these cues in other words mediate the links *between* men's FA and women's short-term mate preferences. It is possible, of course, that some of the cues that correlate with men's FA might also covary with other (different) underlying indicators of good genes, and that other cues indicative of good genes do not correlate with men's FA.

Women's Long-Term and Short-Term Mate Preferences. Buss and Schmitt (1993) claim that the characteristics women prefer in long-term and short-term mates are quite similar. According to SST, women use short-term mating to evaluate men as potential long-term partners or for mate switching. Women's long-term and short-term mate preferences, however, are *not* identical. As mentioned, women place greater emphasis on men's physical attractiveness and physical prowess when evaluating them for short-term relationships (Buss & Schmitt, 1993). Moreover, Scheib (2001) has found that women place greater weight on physical attractiveness when evaluating men as possible extrapair partners.

Gangestad, Simpson, Cousins, and Christensen (2001) examined women's mate preferences by asking women to rate men who had been videotaped during an interview conducted by an attractive woman (see Simpson et al., 1999). After observing each man, women rated his attractiveness as a potential longterm mate and as a short-term mate (i.e., a one-time sex partner or an extra-pair mate). Women's sociosexual orientation (based on their SOI score; Simpson & Gangestad, 1991) was measured as well. Women with an unrestricted sociosexual orientation are more willing to have short-term relationships (i.e., they are more interested in short-term mating). Women with a restricted sociosexual orientation, in contrast, are less willing to have sex without commitment and emotional closeness (i.e., they are more interested in long-term mating) (Simpson & Gangestad, 1991, 1992).

Relative to restricted women raters, unrestricted women raters preferred more symmetrical men, particularly for short-term relationships. Correlations between restricted and unrestricted women's ratings of long-term mate attractiveness and men's symmetry, as well as correlations between restricted women's ratings of short-term mate attractiveness and men's symmetry, were all nonsignificant. These findings indicate that more symmetrical men have (or display) features that are preferred in short-term mates by those women who are most willing and likely to engage in short-term mating—unrestricted women. They also clarify how women's preferences may produce greater mating success in more symmetrical men. These results would not be expected if more symmetrical men offered superior material benefits in long-term relationships.

Recently, Penton-Voak et al. (1999) have found that women rate men with more masculine (i.e., androgen-facilitated) faces as more attractive, but only when they are ovulating and only as short-term mates (see also Penton-Voak & Perrett, 2000). Gangestad, Simpson, Cousins, Garver-Apgar, and Christensen (2003), therefore, had a new sample of women view the videotapes described earlier and rate men as both short-term sex partners and long-term mates. Observer ratings of each man's interview behavior yielded two behavioral factors: (a) the degree to which each man conveyed social presence (e.g., composure, confidence) during the interview; and (b) the degree to which each man adopted a superior, condescending attitude toward the male competitor. Before rating each man, women indicated where they were in their menstrual cycle. Similar to previous studies, women's fertility was estimated from their probability of conception when they took part in the study, based on medical data (Jöchle, 1973). Women taking the pill were excluded.

The results conceptually replicated Penton-Voak et al. (1999). When women rated the attractiveness of men as short-term mates, their ratings were significantly associated with the degree to which each man scored higher on the social presence and superior attitude behavioral factors, but *only* when women were in the fertile phase of their cycle. Women did not show this pattern when they were not ovulating. When women rated the same men as long-term mates, neither men's social presence nor their superior attitude were correlated with women's ratings, regardless of where women were in their cycles.

In sum, the degree to which women are attracted to men appears to be contingent on the social context (i.e., whether men are being considered as sex partners versus long-term mates), their own attributes (i.e., their current stage in the reproductive cycle), and the specific attributes of potential mates (i.e., the degree to which men exhibit “honest” cues signaling their underlying health/ viability).

THE STRATEGIC PLURALISM MODEL

According to Gangestad and Simpson’s (2000a) model of strategic pluralism, men’s allocation of effort to short-term mating in the EEA should have been contingent on their ability to satisfy the short-term mate preferences of women. Women’s short-term mate preferences, on the other hand, should have been shaped by GGSS. As a result, men’s tendency to engage in short-term mating should be a function of their genetic fitness (indexed by their FA), whereas men’s propensity to invest in exclusive, long-term relationships should be inversely related to their genetic fitness. In this section, we briefly review data supporting these conjectures.

Fluctuating Asymmetry and Investment in Exclusive Relationships

Gangestad and Thornhill (1998c) tested whether more symmetrical men typically invest less in long-term, ongoing relationships. Dating partners answered questions about their own and their partner’s behavior in the relationship and completed the Relationship-Specific Investment Inventory (RSI) (Ellis, 1998). The RSI measures acts pertinent to the partner and relationship maintenance: Being Expressive/Nurturing, Being Committed, Giving Time, Being Sexually Proceptive, Investing Money, Being Honest, Providing Physical Protection, Being Attentive in Social Contexts, Having a Good Relationship with the Partner’s Family, and Not Sexualizing Others. Some of these acts (e.g., giving time and attention to the partner) detract from mating effort outside the relationship. Therefore, these acts should correlate negatively with men’s symmetry.

Path analyses tested models in which both self-reports and partner-reports were treated as markers of men’s investment. After controlling for men’s resource potential (indexed by their anticipated future salary), women’s self-rated physical attractiveness, and women’s level of investment in the relationship, more symmetrical men were found to provide less investment than less symmetrical men. In particular, more symmetrical men were less honest with their partners, sexualized other women more, and spent less time with their partners. This pattern of lower investment should facilitate efforts to mate outside the current relationship.

The one component of investment that symmetrical men provided more of was physical protection, particularly their reported ability (as opposed to their reported willingness) to provide their current partners with greater physical protection. Because the ability to provide physical protection may involve attributes that also facilitate successful intrasexual competition, providing this form of investment is not likely to interfere with men's short-term mating. Thus, if men with good genes do compensate their partners with material benefits, the most likely candidate may be physical protection.[9]

Variation in Women's Mating Strategies

To this point, we have focused on adaptive variation in men's mating strategies and tactics. Women, however, also vary considerably in their willingness to engage in short-term mating (Simpson & Gangestad, 1991). According to Gangestad and Simpson's (2000a) model, the attributes that made men valuable as short-term mates in the EEA should have differed from those that made them valuable as long-term mates. Short-term or extra-pair mates may have offered genetic benefits that were typically unavailable from long-term mates, and long-term mates who did offer more genetic benefits may have provided fewer material benefits. Because of this, women should have differed in the extent to which they could have benefited from obtaining genetic versus material benefits. If some women could have improved their fitness by mating with men who offered better genetic benefits, these women should either have preferred long-term mates with markers of genetic fitness or they should have engaged in short-term mating with males who possessed these markers, even if they had to "trade-off" or risk losing the material benefits they could obtained from a long-term mate. Conversely, if other women could have enhanced their fitness by attracting mates who provided (or could provide) superior material benefits, these women should have pursued long-term mates who were able and willing to provide the material benefits they needed most. These women, therefore, should have refrained from extra-pair mating. Hence, the differential value of genetic versus material benefits across women could have generated adaptive variation in women's mating tactics.

Male Attributes Predicting Women's Mating

In a study described earlier, Gangestad et al. (2001) found that women with a less restricted sociosexual orientation (i.e., those more willing to have shortterm sexual relationships) found more symmetrical men as more attractive, particularly as short-term mates. These results indicate that variation in women's willingness to engage in short-term mating is associated with stronger preferences for indicators of male genetic fitness.

Two additional lines of evidence also suggest that women who engage in short-term mating prefer men with markers of genetic fitness. First, women who are willing to engage in short-term mating (i.e., unrestricted women) care more about men's physical attractiveness than do women who are less willing to engage in short-term mating (Simpson & Gangestad, 1992). Second, when given a choice between dating a romantic partner who is very attractive but not highly reliable/loyal versus one who is highly reliable/loyal but only average in attractiveness, women who are more willing to engage in short-term mating (unrestricted women) tend to choose the more attractive/less reliable male, whereas women who are less willing to engage in short-term mating (restricted women) tend to select the less attractive/more reliable one (Simpson & Gangestad, 1992).[10]

Environmental Factors and Mating Strategies

Gangestad and Simpson's (2000a) model has several important implications for understanding how environmental factors might influence the deployment of different mating strategies within each sex. At base, the strategic pluralism model contends that women should vary in their "exchange rate" between a partner's genetic fitness (indexed by FA) and his investment, whereas men should vary in their "exchange rate" between short-term mating effort and long-term parental investment. This implies that environmental factors should influence women's assessments of the exchange rate between a prospective male's parenting qualities and his heritable fitness. In environments where biparental care was critical to infant survival, male parenting qualities should have had more beneficial effects (Andersson, 1994). Conversely, in environments with many pathogens, male genetic fitness may have had more beneficial effects (partly because of the importance of pathogen resistance and the decreased marginal value of heavy parental investment when mortality rates are high) (Kaplan, 1996). If ancestral women repeatedly encountered these countervailing environments, they should have evolved to trade-off investment qualities and indicators of good genes, contingent on environmental conditions. Factors affecting this exchange rate could have differentiated: (a) populations of individuals, producing differences in mating systems *across* different groups of people; and (b) individuals within a population, producing differences in mating preferences and behaviors *within* groups of people.[11]

Environmental Factors: Between-Population Differences

Men's genetic quality. Several factors might have affected the relative value of men's genetic fitness, but pathogen prevalence should have been one critical factor (Hamilton, 1982). In environments where pathogens were prevalent, women should have benefited more from mating with men who had good genetic qualities that made them more pathogen resistant. Although men who had greater genetic fitness may have provided more direct material benefits when female demands for extra-pair mating were very low (see Gangestad & Simpson, 2000b), several lines of evidence suggest that women may have selected certain men as mates simply for the genetic benefits such men could transmit to their offspring.

First, aside from conveying information about female fertility, physical attractiveness may also contain cues about a person's health, pathogen resistance, or genetic fitness (Symons, 1979). Accordingly, Gangestad and Buss (1993) tested whether preferences for attractive mates might have evolved through parasitedriven sexual selection. Men and women from 29 countries throughout the world rated the importance of several mate attributes, including their preference for a"good-looking" mate (see Buss, 1989). Pathogen pressure at each geographical location was estimated from the prevalence of eight macroparasites. Across the 29 countries, both men and women in regions containing more pathogens placed greater importance on a prospective mate's attractiveness, even when latitude, geographical region, and mean income were controlled. In isolation, this finding does not confirm that host-parasite coevolution necessarily influenced sexual selection in humans. Attractiveness may have been more important in regions with more parasites, for instance, because of direct selection against mating with diseased individuals (Kirkpatrick & Ryan, 1991). Thus, further evidence is required.

Second, women in more pathogen-prevalent environments should be more willing to trade-off features predictive of exclusive parental care. Indeed, across the 29 cultures, parasite prevalence correlated negatively with the mean ranked importance of four attributes relevant to direct, exclusive parental care: "dependable character," "pleasing disposition," "emotional stability and maturity," and "desire for home and children" (Gangestad, 1993). In other words, individuals place less weight on these attributes in environments containing more pathogens. It remains unclear whether these findings reflect the increased value of physical attractiveness or the devaluation of these male attributes.

Third, in environments where pathogens are more prevalent, women should trade off indicators of good genes for exclusive paternal investment. This suggests that a higher degree of polygyny should be observed in environments containing more pathogens. Low (1990a) tested this prediction in nearly 200 societies of the Standard Cross-Cultural Sample (Low, 1988, 1990a; Murdock & White, 1969). Controlling for latitude and geographical region, she found that polygyny was more prevalent in societies where pathogens were more common. Viewed together, these results suggest that variation in parasite prevalence might be responsible for the variation in desired mate attributes and associated mating strategies across cultures.

Men's Parental Effort. According to the strategic pluralism model, the impact of men's parental effort on infant mortality and later reproductive success of offspring should have been influenced by several factors. In environments where the primary causes of infant mortality were infectious diseases (rather than deficient parental care), paternal effort should have affected offspring fitness less. When infant health was strongly dependent on biparental care (e.g., when women could not provide all of the nutritional needs for their children), however, paternal care should have impacted offspring fitness more.

Women's access to resources should also have influenced their need for and the value of male parental investment (Gowaty, 1992a,b). Given that parental investment has diminishing returns once basic care has been provided (Cashdan, 1993), men's resources may have been less important for women who already had sufficient resources. This suggests that polygyny should be more common in societies where women have greater access to resources. Examining nearly 200 cultures in the Standard Cross-Cultural Sample, Low (1990b) found this pattern.

Of course, we do not know how women's control of resources affected their mate preferences in the EEA. However, we do know that women's participation in current economies predicts the emphasis they place on men's physical attractiveness. In the 37 cultures surveyed by Buss (1989), women's mean preference for physical attractiveness in a mate was positively correlated with the percentage of women in a given culture who were involved in the economy. Women's preferences for qualities related to parental care, on the other hand, were *not* correlated with their economic involvement (Gangestad, 1993).

In a reanalysis of the 37 cultures data, Eagly and Wood (1999) found that women's preference for men's earning potential is predicted by a measure of women's "empowerment" (United Nations Development Programme, 1995). Specifically, as women's empowerment (indexed by their earnings, their representation in legislative government, and their involvement in professional positions) increases relative to men within cultures, women place increasingly less value on male earnings. However, women do not place less emphasis on men's physical attractiveness (nor do men place less emphasis on women's attractiveness) as women's empowerment increases. Eagly and Wood interpret these find ings in terms of domain-general behavioral processes and responses to gender roles. These findings, however, are also consistent with ecologically contingent, conditional mating strategies in which women tailor their mating tactics and preferences to current environmental factors (e.g., their control of resources).

Men's Tactic Choice. According to the strategic pluralism model, the way in which women evaluate men and make trade-offs should affect the mating tactics that most men adopt. In environments where male parenting qualities are needed and valued, women should be less inclined to engage in short-term and extra-pair mating. When the "demand" for short-term mates drops, men should devote more effort to parental investment, and variance in men's reproductive success should decline (i.e., a larger percentage of men should sire children). Conversely, in environments where men's genetic fitness is needed and valued, women should be more willing to engage in short-term, opportunistic mating. When the "demand" for short-term mating rises, men should devote greater effort to short-term and extra-pair mating, and variance in men's reproductive success should increase (see Low, 1990a,b).[12]

Environmental Factors: Within-Population Differences

Resources and cues of parenting. Several additional factors might impact the development and expression of mating strategies within populations. To the extent that women differ in resource holdings, within-population differences in mating preferences may develop, such that women with more resources may place greater emphasis on short-term mating tactics and good genes. Moreover, if some women encounter environments that require paternal investment whereas others do not, within-population differences in mating preferences and tactics may also emerge. Belsky, Steinberg, and Draper (1991) have proposed that the quality of parental care experienced during childhood may affect the development of adult mating tactics (see also Chisholm, 1996; Simpson, 1999). Experiencing insufficient or poor parental responsiveness during childhood (possibly because of harsh, demanding environments) should lead individuals to adopt short-term mating tactics in adulthood, tactics that may be better suited for future environments in which stable pair-bonds are not needed or anticipated. Experiencing adequate parental responsiveness, in contrast, may shunt individuals toward long-term mating tactics in adulthood, tactics that should be more appropriate for environments in which biparental care and stable pairbonds are both needed and expected.

Women's Phenotypic Qualities. Women's personal characteristics may also affect which qualities they value in mates. In many circumstances, the more investment an offspring has received, the less "adaptive" additional investments become (Cashdan, 1993). This might be particularly true when men's and women's investment in offspring is similar (rather than complementary) and women already have sufficient resources. Consequently, women may have evolved to focus more on men's genetic fitness when their own access to independent resources increased (Gowaty, 1992a), making them more inclined to pursue short-term mating. Some indirect evidence supports these conjectures. Unrestricted women (who are more likely to engage in short-term mating) tend to be more socially dominant, more extraverted, and less harm-avoidant (Gangestad & Simpson, 1990). Each of these traits is likely to facilitate the acquisition and maintenance of independent resources.[13]

CONCLUSIONS

Considering the demands for biparental care throughout human evolutionary history, the strategic pluralism model posits that both sexes should have been selected to enact long-term mating tactics and to invest heavily in their children. Both sexes also should have evolved to track ecological cues when making important mating decisions (see Gangestad & Simpson, 2000b). At the same time, however, both sexes should also have been selected to enact ecologically contingent, conditional mating strategies, dedicating some effort to short-term and extra-pair mating under certain conditions. Women may have evolved to trade-off evidence of a man's genetic fitness for evidence of his ability and willingness to invest in offspring. The specific mating tactics and preferences women adopted should have depended more on the nature of their local environments. If the local environment was difficult and demanded biparental care, women should have placed more weight on the investment potential of prospective mates and less weight on indicators of their genetic fitness. As a result, a larger proportion of women should have adopted long-term mating tactics almost exclusively. On the other hand, if the local environment contained many pathogens (or signaled the importance of obtaining genetic fitness for offspring), women should have placed more weight on indicators of male genetic fitness. In such environments, a larger proportion of women should have been willing to engage in short-term, extra-pair mating, allowing them to obtain genetic benefits from men who provided less parental investment at the risk of losing parental investment from their primary mates.

If most women in a local environment expected high paternal investment, most men (especially those who were less fit) should have offered more exclusive investment, dedicating more effort to long-term mating and less to shortterm mating. As a result, variance in men's mating success should have declined. When women's "demands" for genetic benefits increased, some men (especially those who were more fit) should have allotted more effort to short-term, extrapair mating, increasing variance in mating success within men. Only a small proportion of men (i.e., those who were most fit), however, should have been able to pursue short-term mating successfully at all times, regardless of the environmental factors to which women were responding.

Several caveats must be recognized in connection with the strategic pluralism model. First, FA is only one possible marker of good genes; additional markers need to be identified. Second, we currently know little about how women (and, in some situations, possibly men) make strategic trade-offs between viability and investment; current data on how trade-offs are negotiated is largely indirect. Third, we are only beginning to understand links between FA, health, and immune system functioning. It is important to note, however, that FA should have been associated with enhanced reproductive fitness in the EEA, not necessarily with longevity per se. Fourth, the relation between FA and individuals' ability versus willingness to invest in offspring remains murky. Despite evidence suggesting that women prefer more symmetrical men for their genes (in the absence of material benefits), more symmetrical men could have been more capable of garnering and providing resources in the EEA. Fifth, more needs to be learned about how other important variables affect mating decisions (e.g., self-perceived mate value, the strategies and tactics enacted by local same-sex competitors, etc.). Sixth, the strategic pluralism model focuses on mate choice and not necessarily on the factors that sustain romantic relationships. Although radical changes in a mate's viability or investment potential should have implications for whether a relationship is maintained, different biological or evolutionary processes may govern relationship maintenance. Seventh, the strategic pluralism model does *not* imply quality versus quantity trade-offs (see Chisholm, 1996). Short-term mating, for example, does not necessarily involve reductions in *total* parental effort; total effort may simply be distributed across a larger number of offspring. Eighth, the strategic pluralism model does not deny that operational sex ratios and related ecological factors (e.g., harshness of local environments) could affect the deployment of short-term versus long-term mating strategies. Ninth, we currently do not know whether good genes have diminishing rates of return. For example, evidence of men's good genes (e.g., FA) may be less "valuable" beyond some critical threshold. Moreover, women who possess markers of good genes may have less to gain by seeking mates with good genes. Finally, life history models of reproduction (see Belsky, Steinberg, & Draper, 1991; Chisholm, 1996) may interface with strategic pluralism principles. For example, women raised in environments where infant mortality is high may weigh honest indicators of men's viability more heavily than indicators of male investment when evaluating mates, and may have lower thresholds for finding men with honest viability indicators as "acceptable" mates.

Strategic pluralism remains a provisional model of human mating, one that addresses only some of the factors that might be relevant to the evolution of human mating. Nonetheless, compared to previous theories, the model more completely captures the plurality of mating tactics employed by women and men, it more fully explains *why* trade-offs are negotiated in certain ways by each sex, and it elucidates the ways in which environmental conditions should influence the use of different mating strategies and tactics within each sex.

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NOTES

1. Natural selection also has important implications for human mating. In many species, male parental care might have evolved to increase offspring fitness directly and, thus, could have evolutionary benefits (Clutton-Brock, 1991), especially in humans (Lancaster & Lancaster, 1985). Natural selection might also have motivated females to obtain the resources necessary to reproduce and compete for additional resources (Gowaty, 1992a; Hrdy, 1981). Moreover, mating can have benefits that are indirectly derived from reproduction and are not associated with sexual selection. Females, for instance, may induce paternity uncertainty by having multiple mates, possibly resulting in more protection or greater tolerance of offspring by different men (Hrdy 1981).
2. Not all environment-response linkages are evolved. Many are learned. Learning, however, can reflect a set of implicit, evolved decision rules (Mayr, 1974).

3. In some species, males engage in more parental effort than females, in which case sexual selection pressures may operate more strongly on females than males (Trivers, 1972; 1985). There also are circumstances in which having multiple mates can benefit females, even in species in which females tend to invest in offspring more than males (Hrdy, 1981).
4. This is not a novel idea. Conditional strategies are a major topic in behavioral ecology (Krebs & Davies, 1993). However, the possible role of conditional strategies in human mating has received little attention (Buss, 1998; Buss & Schmitt, 1993). Assuming that the signals indicating which strategy should be best are reliable, conditional strategies should evolve when the fitness difference between the chosen strategy and other possible strategies in a given environment is significantly larger than the costs of making a bad decision. For this to occur, the costs of using other (nonoptimal) strategies should be relatively high, the costs of using the chosen strategy should be low, and the benefits associated with the chosen strategy should be high.
5. It is important to emphasize that alleles, which are good genes at one point in time, need not be intrinsically better than alternate alleles. Host-parasite coevolution imposes changing selection pressures on host genes, maintaining heritable fitness in individuals. Consequently, an allele that is a “good gene” today might be selected against in future generations, and an allele that currently is selected against could become a “good gene” in the future. Because no gene is inherently better than alternate ones, the population does not become more fit through selection.
6. This type of selection can be understood in cost-benefit analysis. A costly trait confers certain benefits. For example, males who possess the trait may be preferred as mates, but having the trait may impose some costs given that the resources used to develop the trait could have been used for other purposes. At some point, the marginal gains of investing in the costly trait begin to diminish, and individuals should no longer be selected to invest in the trait beyond that point. The benefits of sexual selection can be maintained if males with different levels of heritable fitness maximize their benefits relative to their costs (i.e., maximize their outcomes) at different levels of the costly trait (Grafen, 1990).
7. Although Perusse (1993) has found that men with higher status report having more sex partners than men with lower status, this effect did not hold for married men in his sample. Thus, it is unclear whether men with higher status (e.g., wealthier men) have more extra-pair mates than men with lower status.
8. It could be argued that women find more symmetrical men more appealing because they are looking for long-term mates who will provide them (and subsequent offspring) with more material benefits. Even though more symmetrical men may be more capable of providing certain benefits (e.g., physical protection), the reproductive cycle studies indicate that women tend to find more symmetrical men more desirable *primarily* when they are ovulating (i.e., when gaining material benefits is not likely). From the standpoint of adaptive design, these data strongly suggest that women find symmetrical men more attractive than less symmetrical men for reasons beyond potential resource acquisition.
9. Hrdy (1981) and Smuts (1985) have claimed that physical aggression from men other than fathers may have been a major danger to offspring in the EEA. To reduce these threats, women may have mated with multiple partners (to confuse paternity) or developed male-female friendships, particularly with socially and

physically dominant men. Mesnick (1997) and Wilson and Mesnick (1997) have argued that women may have evolved to prefer protective males primarily to receive protection from other aggressive males, which could have enhanced women's fitness directly.

10. Another factor that should influence women's willingness to engage in short-term versus long-term mating ought to be the perceived risks of short-term mating. When the perceived risks are high, women should be less inclined to engage in short-term mating.
11. Because women also vary in their relative mate value, they should differ in the amount of benefit they receive from men. Certain highly valued women may be able to obtain both high material benefits and high genetic benefits. For most women, however, the genetic benefits and exclusive investment benefits that can be obtained from mates should correlate negatively within the most desirable set of mates a women can attract (cf. Simpson & Gangestad, 1992).
12. Low (1990a) has suggested that healthy men in pathogen-prevalent regions may have multiple mates because they can provide better paternal care. We suggest that men who have better fitness indicators should invest less in their offspring than men with poorer indicators.
13. Hrdy (1981) and Gowaty (1992a) have suggested that women's access to resources through cooperation, competition, and social influence should have been an integral component of their fitness in the EEA. These personality traits may have helped unrestricted women to acquire their own resources and, thus, to pursue shortterm mating tactics more successfully.

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4

The Intimate Relationship Mind

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*E*volutionary psychology (née socio-biology) was forged as a revolutionary movement in the 1970s, provoking the inevitable horrified backlash from dyed-in-the-wool defenders of the standard social science model. Evolutionary psychology, however, has continued to make considerable inroads into mainstream psychology. Empirical and theoretical articles now commonly appear in mainstream cognitive and social psychology journals, chapters adopting evolutionary approaches have become standard fare in psychology handbooks, and psychology textbooks routinely include evolutionary material. Yet the relations between mainstream psychology and evolutionary psychology continue to be strained. Many psychologists remain openly skeptical, even derisive, of the claims and programs of evolutionary psychology, and it remains true that one of the best ways of instigating verbal fireworks at a conference dinner table is to raise the topic of evolutionary psychology.

There are doubtless several reasons for the continuing furor over the status and proper role of evolutionary psychology. To begin with, occasional claims by its most fervent proselytizers that it represents nothing less than a new grand theory, destined to replace the unscientific fiddlings of mainstream psychology (e.g., Buss, 1995), tend not to go down well with most psychologists. It is true that evolutionary psychologists are generally happy to concede that social, developmental, and cognitive psychology should retain their own domains of study, which focus on the proximal-level psychological machinery rather than on distal causes from our evolutionary past. However, when explicating the links between social or cognitive psychology and evolutionary psychology, their emphasis overwhelmingly remains on what evolutionary psychology has to offer mainstream psychology, rather than vice versa (e.g., Buss, 1999; Simpson & Kenrick, 1997).

We focus in this chapter on the links between social psychology and evolutionary psychology and argue that the time is ripe for a more balanced and less evangelical approach, in which the efforts of mainstream psychological scientists and evolutionary psychologists should be viewed as a collaborative, equal opportunity enterprise. However, we will argue that there is a problem at the heart of an evolutionary psychological approach, that cannot be waved away as a misunderstanding, and that has been at the core of scientific misgivings about the proper application of evolutionary theory from Darwin onward; namely, the extent to which biological evolutionary theory can or should be applied to *Homo sapiens*.

The main aim of evolutionary psychology can be described as explaining the cognitive and emotional mechanisms of the contemporary human mind, through understanding how such mechanisms evolved via natural and sexual selection in the ancestral environments. Disagreements among proponents of such an approach are a dime a dozen, the closer one gets to the fine print of how such an aim should be unpacked. Nevertheless, there is widespread adherence to three basic principles among devotees of this approach. First, this approach is essentially cognitive, in that it attempts to principally understand how human cognition and emotion have evolved. Second, the human mind is viewed as a cognitive Swiss army knife, rather than as a general problem solver (termed the *assumption of modularity*). Third, an evolutionary approach is historical. It attempts to understand the selection forces that rendered particular mental modules functional within an environment that existed long ago.

Like evolutionary psychology, the key focus of social psychology is on the human social mind, and in particular how cognition and emotion interact with behavior, both within individuals and across individuals when they interact. In the study of intimate relationships, social psychologists study the structure and functions of the intimate relationship mind, and seek to model and explain how cognition, emotion, and behavior play out in terms of the phenomena of intimate relationships (e.g., love, mate selection, marriage, divorce, relationship violence, and so forth). Thus, the focus of social psychology is on explaining the proximal-level workings of the intimate relationship mind (Fletcher, 2002).

In this chapter we take two core features of the intimate relationship mind—love and mate preferences—and briefly describe and explain their nature and functions (drawing heavily from Fletcher, 2002). We then use these topics as a springboard with which to tease out the strengths and weaknesses of both standard social psychological and evolutionary approaches, and to analyze further how they might contribute to the science of the (relationship) mind.

LOVE

There is compelling evidence that adult (sexual) love is composed of three distinct and basic components that each represent evolved adaptations; namely, intimacy, commitment, and passion. These three components can operate independently or in concert. When every component is in full flood, but with passion occupying center stage, the result is usually termed *romantic love*. For relationships in which the passion and intensity are set to low, but that maintain comfortably high levels of intimacy and commitment, this form of love is often termed *companionate love.*

Adult intimate love has an array of features that mark it out as an adaptation (Fletcher, 2002). First, there is strong evidence that both romantic and companionate love are universals. Second, there exists a plausible evolutionary account (supported by comparative data) that specifies the functions that both forms of love have evolved to meet. Third, the capacity for love follows identifiable developmental pathways, being linked to the adult development of sexual biological systems. Fourth, sexual love between adults has some pivotal similarities with platonic love, especially adult-child love. Fifth, romantic and companionate love are associated with distinctive emotions and related physiological processes, including the release of specific bonding hormones such as oxytocin and vasopressin. Sixth, people think about love in ways that cognitively mirror the three components outlined (intimacy, commitment, and passion). Finally, the mate assessment module, to some extent, shuts off the search for alternative mates when love exists at high levels (especially for women). We expand on these points in the following.

An evolutionary psychological approach suggests that different emotions have evolved to deal with different problems, and therefore should have special (and universal) characteristics. Social constructionists and postmodernists have often (absurdly) claimed that romantic love is an invention of European culture. However, there is considerable evidence for both the antiquity and the universality of romantic love. Jankowiak and Fischer (1992) found good evidence (based on folk tales, ethnographies, evidence of elopement, and so forth) of romantic love existing in 147 of 166 cultures. This is a conservative figure, given that in 18 of the 19 love-absent cultures the ethnographic accounts were uninformative rather than definitive.

The standard evolutionary explanation for the origin of love runs as follows. Compared to other primates, humans have exceptionally large brains and thus heads, which necessitates them being born at an unusually undeveloped stage (for a mammal) in order to achieve egress though the birth canal. In addition, humans are dependent on their parents and other relatives for unusually long periods of time before attaining adulthood (compared to other animals, including primates) and also require a tremendous amount of informal and formal education from their parents to attain the social, cultural, and practical knowledge necessary for survival and reproductive success. Accordingly, as brain size and childhood length steadily increased over the last million or so years of *Homo* evolution, there were strong selection pressures toward the development of (relatively) monogamous pair bonding.

Thus, the function of the first stages of romantic love is to motivate sexual activity, whereas the function of the second, companionate stage, is to establish a firm emotional bond between the partners that will motivate them to remain together, at least for enough years to give the children a good chance of making it to adulthood (and passing on the genes of both parents). The existence of a stable monogamous couple in a hunter-gatherer lifestyle also allows for a potentially valuable division of labor, with the man the dominant provider and the woman the dominant caregiver (although in hunter-gatherer cultures both genders typically perform both functions). In short, in our ancestral environment, two parents were better than one.

This evolutionary account is consistent with the comparative data. For example, research with mammals has implicated the role of oxytocin and vasopressin (closely related neuropeptides) in bonding, both in terms of mother-infant and adult-adult bonding (Diamond, 2003). However, such hormones only appear to be present in the 3% of male mammals who are monogamous, expend considerable investment in the offspring, and bond with a female partner (Insel, 2000). In humans also, the same ancient hormones (and hormone receptors in the brain) are present in both men and women, are released by women when giving birth and when breast-feeding, and by both men and women during sexual intercourse and after orgasm (Insel, 2000).

A massive amount of developmental evidence supports Bowlby's pioneering thesis that adult attachment and love are prefigured, and developed on the back of attachment systems that are in place at birth (Simpson, 1999). Shaver, Morgan, and Wu (1996) list no fewer than 17 similarities between the two systems. For example, lovers often slip into motherese when they talk to one another (nauseating though it might be for the casual observer), using favorite nicknames, and slipping into singsong cadences. Lovers have a strong need to spend a lot of time together, often caressing and kissing one another. Lovers seem fascinated with each other's physical appearance, and engage in long bouts of prolonged eye contact. Lovers become distressed if they are parted for prolonged lengths of time, and are exquisitely sensitive to each others needs. And the list could go on.

The similarity between adult-adult love and adult-child love is exactly what an evolutionary approach would predict (Shaver et al., 1996). The attachment and caregiving behavioral/affective systems are ancient, present in all mammals that care for and protect their offspring for lengthy periods of time, and precede the emergence of primates and *Homo sapiens* by millions of years (Hrdy, 1999). These two specific affective and behavioral systems were clearly designed to increase the odds that vulnerable infants would survive to adulthood. If pairbonding and love between human adults (or adults of any species) was to emerge as an adaptation, then evolution would assuredly tinker with the serviceable attachment systems that were already in place—no need to invent something completely different. Thus, the basic commitment and intimacy components of adult human love are likely to have been directly lifted from the same systems that promote adult-infant bonding.

The three love components appear to be underpinned by two independent hormonal/physiological systems. Passion is associated with the sex hormones (e.g., testosterone) juiced up by the peripheral arousal systems (with hormones such as adrenaline). Commitment and intimacy, in turn, are promoted by the hormones oxytocin and vasopressin (Fisher, 1998). These hormones are produced during sexual intercourse and after experiencing orgasm by both men and women (Diamond, 2003), and also by women when giving birth and when breast-feeding (along with other mammals).

Social psychological research, which has examined how people think about love, both in terms of actual relationships and more abstract judgments, has revealed the identical cognitive dimensions of intimacy, commitment, and passion (e.g., Aron & Westbay, 1996). Tellingly social psychologists that have carried out this work have done so quite independently of either evolutionary psychology or the developmental work. Moreover, it is important to note that research participants in this work are not actually asked to group the items, or to report whether they think the individual items are related. These dimensions are products of *factor analysis,* a statistical procedure that does not require or depend on the research participants being aware of the underlying structure implicit in their ratings.

Social psychological research suggests these three components can have complex connections, and can operate quite independently (see Fletcher, 2002). The empirical evidence, in line with common sense, suggests the following patterns are common. In flings or one-night stands, passion can run high, whereas intimacy and commitment are probably low. However, in relationships that develop past a few dates, high levels of passion typically follow the development of intimacy, and high levels of commitment (such as in a marriage) lag further behind again. In many relationships, especially in full-blooded romantic affairs, the three components operate in unison. As relationships mature over several years, the passion component usually fades, often leaving behind high levels of intimacy and commitment, and this seems to be true in many cultures.

Commitment may also tail off for a variety of reasons, often leading to the demise of the relationship. However, individuals can and do maintain that they “love” their partner even though passion and commitment have both disappeared. For example, in marriages where one partner is an alcoholic, the passion (sexual attraction) and commitment (caregiving) modules may be running on empty, but the intimacy (attachment) module might still be up and running. Moreover, commitment can also attain astronomical levels, even in the total absence of behavioral interaction, such as in the (usually painful) cases of unrequited love.

Finally, a well-established finding in social psychology is that people who are in love or highly committed to relationships (especially women) tend to derogate alternative mates, and essentially stop searching for mates outside the primary relationship (e.g., Johnson & Rusbult, 1989). In short, the mate assessment module, to some extent, shuts off the search for alternative mates. However, it is important to note that derogation of alternative mates does not occur when individuals are dissatisfied with their current relationships (Johnson & Rusbult, 1989).

Problems

One might question how the account presented thus far squares with arranged marriages or the existence of polygyny in many cultures. It could also be objected that the bonding mechanism of love does not seem to function particularly well, given that extramarital liaisons are so common, and that divorce is so prevalent in Western countries and in many other cultures. However, none of these objections are fatal to the argument presented.

In traditional cultures that practice arranged marriages, brides (and grooms) are typically given some choice in the matter. For example, in arranged marriages in Sri Lanka men and women who like one another (or fall in love) usually let their parents know their choices in advance through indirect channels (de Munck, 1998). Moreover, families or matchmakers, it turns out, use very similar criteria in mate selection to those that the partners themselves would use if they had a completely free choice, including matching on attractiveness or socioeconomic status (Rockman, 1994). As Fisher (1992) concludes in her analysis:

> Taboos, myths, rituals, myriad cultural inventions coax the young around the world into arranged marriages. Yet where these marriages can be dissolved, as in New Guinea, on atolls in the Pacific, in much of Africa and Amazonia, people regularly divorce and remarry mates they choose themselves. (p. 74)

It is true that a whopping 84% of known cultures allow polygyny. However, it has been estimated that only about 5% to 10% of men in cultures that allow polygyny actually have more than one wife, the majority of marriages being monogamous (Fisher, 1992). In cultures that allow polygyny the wives often complain and suffer from bouts of jealousy, and, genetically speaking, there is not much in it for the women. They may certainly attain a share of the status or wealth of their husband, but will probably have to compete for such resources with the other wives. From the male point of view there is the distinct genetic advantage of siring more offspring, but on the other hand, considerable resources and wealth may be required to maintain more than one wife, and the task of ensuring spousal fidelity may become difficult, if not exhausting.

How powerful are the bonds of love? The divorce rate in the United States and other Western countries has steadily increased since the 1940s, but has leveled off in the last two decades in most Western countries. The United States still retains the lead, with current estimates that about 50% or higher of marriages are likely to break up. This rate is almost double that of most European countries, Australia, and New Zealand (Goode, 1993). The widespread perception that divorce rates have become outrageously high in the United States and other Western countries, compared to all other cultures, is a fallacy Traditional hunter-gatherer cultures, such as the !Kung, have comparable divorce rates to those now seen in Western countries, as do many Arab or Muslim countries, along with preindustrialized Japan.

The peak period for divorce in the United States is currently running at about 4 years after getting married, but Fisher (1992) has shown that this same peak period for divorce is common across cultures. Most individuals in Western countries, including the United States, who get divorced eventually remarry (80% to 90%). The same pattern is found across most cultures (Goode, 1993). In short, where divorce is relatively common in cultures, it is not replaced by celibacy or promiscuity, but by serial monogamy.

Extramarital sexual activity is common in Western countries, and its existence has also been documented in many cultures. Surveys in Western countries generally indicate that somewhere between 20% and 50% of people admit to having had extramarital affairs (Buss, 1999). Anthropological work in traditional cultures is more anecdotal, but suggests, likewise, that adultery is not uncommon (Buss, 1999; Fisher, 1992). It is not difficult to propose plausible evolutionary arguments for extramarital sexual activity. For males, it looks like a way of having one's cake and eating it too. Males can spread their genes around, with the hope that some progeny will make it to puberty while also ensuring that their own children are well cared for in the primary relationship. For women, extramarital sex can enable them to obtain some top-quality genes while also retaining the support of their husbands (who, hopefully, blithely assume fatherhood).

Of course, extramarital liaisons carry risks and costs. Normally they need to be carried out in a clandestine fashion, put the primary relationship at risk, and, if discovered, in many cultures carry legal penalties or socially sanctioned physical attacks from the sinned-against partner (especially by men against women). Moreover, it is not as if the neuroendocrinal, cognitive, and behavioral "love" systems turn off in extramarital affairs. Thus, sexual activity that goes beyond a one-night stand always carries the risk of developing into full-blooded (and potentially life-wrecking) romantic love. How often does it happen that a casual extramarital fling, carried out through boredom or simply because it becomes available, turns into a passionate affair, with oxytocin, vasopressin, and testosterone liberally flowing? We suspect, without any evidence apart from anecdotal to offer, that it is a common occurrence. Love is a dangerous emotion.

Thus, passion or sexual infatuation (like many evolutionary adaptations) does double duty. Operating independently, it can motivate both men and women to take the risks involved in adultery, in order to gain the genetic benefits. On the other hand, when it becomes entangled with commitment and intimacy, and develops into full-blown romantic love, then picking up genetic benefits can quickly switch to picking up a new partner.

Summary

To summarize, like almost every other monogamous species known, humans have an unfaithful streak. However, it is clear that humans (both men and women) are programmed via evolution to court and mate, fall in love, bond and commit to another person, nest, and raise a family. The divorce rates in Western countries can be seen as a glass either half full or half empty. From 50% to 75% of couples in most Western countries apparently choose to live together for most of their lives. This unadorned fact is inconsistent with the *Playboy* philosophy that humans are essentially promiscuous beings, squeezed and socialized into an ill-fitting monogamous straightjacket.

MATE SELECTION

In New Zealand, the United States, the Polynesian Islands, African huntergatherer cultures, and around the world, people focus on the same three categories in evaluating their potential mates: personality factors related to intimacy, warmth, and commitment; a second set related to passion, attractiveness, excitement, vitality, and sex; and a third set related to status and resources such as influence, age, money, position, possessions, and so forth (Buss, 1999; Fletcher, Simpson, Thomas, & Giles, 1999). Moreover, although there are characteristic gender differences in the importance attached to such categories, there is also remarkable agreement across both gender and cultures concerning which factors are most important in selecting mates for long-term relationships: The winner is warmth and loyalty, a close second is physical attractiveness and general vitality, and down the track is status and resources (Fletcher, 2002).

A series of factor-analytic studies by Fletcher and his colleagues have shown that sought-after characteristics of ideal mates fall neatly into the tripartite ideal structure described: *warmth/loyalty* (with items such as understanding, supportiveness, consideration, kindness, being a good listener, and sensitivity); *vitality/attractiveness* (with items such as adventurousness, a nice body, being outgoing, sexiness, attractiveness, being a good lover); and *status/resources* (with items such as having a good job, being financially secure, having a nice house or apartment, being of an appropriate ethnicity, successfulness, and dressing well) (Fletcher et al., 1999; Simpson, Fletcher, & Campbell, 2001). These results have proved to be the same regardless of whether the samples comprised men or women, or whether or not individuals were involved in sexual relationships. Moreover, the importance that people attach to these different categories of ideal mate standards predicts relationship cognition and behavior in predictable ways (as we will document in due course).

What these results show is that people do not just set high or low ideal partner standards—individuals set high or low expectations in ways that vary in a relatively independent fashion across the three kinds of mate characteristics.

Indeed, differences between men and women are considerably less extreme than differences within men and within women. Some people (both men and women) are essentially on the hunt for an exciting, passionate relationship with a gorgeous hunk; others care relatively little about passion and are preoccupied with the search for intimacy, warmth, and commitment. Yet others are prepared to sacrifice somewhat on the passion and intimacy front, if they can obtain a partner with considerable status and resources.

Why do people not want it all? Why is Jane's ideal partner not incredibly kind, beautiful, remarkably fit with a wonderful body—and rich? First, such people might be plentiful in TV soap operas, but in real life they are remarkably scarce. Second, even when Jane meets such a male paragon, he will probably not be interested in Jane (who is not a perfect 10 in every category). Third, even if Jane succeeds in striking up a relationship with such a catch, he may be difficult to retain, and Jane may find she needs to invest an exhausting amount of time and resources in maintaining the relationship. Different people favor different trade-offs and, hence, should differentially weight associated ideals— and they do.

Why do people in cultures across the world share the same core set of three concepts around which they store their mate expectations? Clearly, culture has a pivotal role in any origin account of ideal mate standards. However, not only are the same kinds of ideal standards universal, but also the way in which at least some of the mating criteria are cashed out. We consider the three basic mating criteria in turn.

First, there is a wealth of cross-cultural evidence that people everywhere categorize people in terms of personality categories such as warmth, loyalty, and trustworthiness (Church & Lonner, 1998). In contrast, although increased status and resources elevate mating value everywhere, the fashion in which this is done is amazingly variable. Where we live, in Christchurch, men can gain status by dressing expensively, driving a Porsche, hanging around cafes talking on a cell phone, playing professional rugby, living in a fabulous house with a view, and so forth. The key seems to be to provide evidence of the sort of ambition, drive, and ability that signals the probability that one is, or may become, a wealthy man, or a man who can forge social connections and win respect from the group.

In our human ancestral environments, men who wished to establish their status and resource-gathering credentials did not have cafes, cars, or cell phones. Moreover, in hunter-gatherer cultures, it is not easy to accumulate much in the way of resources, given the lifestyle and the need to travel light. No matter. Political savvy, hunting prowess, or fighting ability are respected and confer status in almost every culture. Indeed, the human desire for status and respect (especially for men) is one of the most powerful, yet most general, human traits known. The evolutionary reason is almost certainly because women (and perhaps men) find such a characteristic attractive in potential mates (or more accurately, did so during our ancestral past). Indeed, Miller (2000) has advanced the bold claim that men became locked into a mate-attraction arms race in which the need for men to be admired and respected (by women) for their mental agility, linguistic skills, musical ability, joke-telling skills, and so forth, became a crucial force in the evolution of human language and the human neocortex. Thus, according to Miller, the human mind is not a computer, but an entertainment center.

In contrast to status and resources, vitality and physical attractiveness are judged in very much the same way both across and within cultures. Individuals in Western and other cultures obviously differ (to some extent) in whom they find attractive. However, the research evidence shows that differences of opinion within Western cultures in standards of beauty and sex appeal are no greater than differences across cultures. Beautiful or homely faces are perceived the world over in much the same way (Cunningham, Druen, & Barbee, 1997; Cunningham, Roberts, Barbee, Druen, & Wu, 1995). The universally attractive female face (for men) has a relatively childlike appearance, with wide-set, large eyes, a small nose and chin, prominent cheekbones, high eyebrows, large pupils, and a warm smile (Cunningham, 1986). The story with men's faces is more complicated. Cunningham and his colleagues have found that the universally attractive male face (compared to a woman's face) has a relatively angular appearance, wide-set eyes, and a large chin—but combined with babyface features including large eyes, and an expressive smile (Cunningham, Barbee, & Pike, 1990).

What goes for faces also goes for bodies; namely, particular body prototypes are universally held to be attractive. Men who are muscular, athletic, and tall are sexier than flabby, out of shape, short men worldwide (Langlois et al., 2000; Singh & Luis, 1995). However, short, out of condition, middle-aged men with potbellies can still do surprisingly well in the mating stakes if they hold power and prestige, or have other qualities such as high intelligence. It is true that fashion models tend toward thinness, sometimes to the point that they resemble advertisements for famine relief. However, when ordinary men (not fashion mavens) are asked to rate body shapes, fashion-model thinness is not judged as physically attractive (Singh, 1994). Singh has shown that, although attractive stereotypes of a woman's body shape do vary in terms of overall size, what remains more or less constant is the favored ratio between the woman's waist and hip, which is about .70. Venus de Milo, Sophia Loren, or Marilyn Monroe might seem chunky to contemporary fashion-conscious eyes. However, all these women have (or had) waists that are close to 70% of the size of their hips (Singh, 1993).

But the critical question remains: Why are these three particular categories—warmth/loyalty, vitality/attractiveness, and status/resources—pivotal in mate selection? An evolutionary approach recently developed by Gangestad and Simpson (2000) answers this question by positing that each dimension represents a different route used to obtain a mate and promote reproductive fitness in our ancestral environments (also see Simpson, this volume). Gangestad and Simpson argue that individuals utilize two basic kinds of mating criteria: the possession of "good genes" and/or "good investment." By being attentive to a partner's capacity for intimacy and commitment an individual should increase his or her chances of finding a cooperative, committed partner who would be a devoted mate and parent—this is the primary "good investment" factor. By focusing on attractiveness and health an individual is more likely to acquire a mate who is younger, healthier, and more fertile—this is the primary "good genes" factor. And, by considering a partners resources and status, an individual should be more likely to obtain a mate who could ascend social hierarchies and form coalitions with other people who have—or could acquire—valued social status or resources. This last category is likely to represent a mixture of both good genes and the ability to invest in the relationship and the children.

It is more or less self-evident why a mate loaded with warmth and loyalty, along with high status and wealth, should make an effective provider and good parent. But why should attractiveness and vitality be associated with good genes? We so routinely perceive human faces and bodies as inherently attractive and beautiful (or the opposite) that it is difficult to step back and ask why particular arrangements of the human body strike us so forcefully and automatically as either beautiful or homely. Sexual selection theory provides the only plausible explanation; namely, that such features were associated in human ancestral environments with an increase in reproductive fitness. The standard example, and the one Darwin was obsessed with, concerns the peacock and his gorgeous tail. The most popular explanation currently for why peahens are so fixated on this feature when selecting mates, is in terms of the handicap principle. That is, large and gorgeously colored tails represent honest advertisements that indicate good health, a robust body, and high fertility (good genes).

For humans, the same kind of explanation entails that beautiful people with great bodies were healthier, more fertile, and bore healthier children in the past than those lower on the attractiveness scales (for both men and women). In support of this thesis, research in contemporary Western cultures has generally found positive relationships between physical *facial* attractiveness and levels of health (Langlois et al, 2000). Moreover, the classic hourglass woman's figure (associated with Singh's waist-to-hip ratio of .70) is associated with good health and high fertility in women (controlling for the effects of age) (Singh & Luis, 1995; Zaadstra et al, 1993).

The Gangestad and Simpson approach predicts the existence of substantial differences within men, and within women, in terms of what people are looking for in a mate—the name of the mating game is to do the best one can, in light of the available pool of mates, ones own perceived mate value, and other prevailing circumstances. Our would-be ancestors, who were unrealistic in terms of their ideal standards (both men and women), ended up being constantly rejected, were less likely to pass on their genes, and thus, were not our ancestors. Accordingly, a more refined and successful set of flexible mating strategies, in which expectations and ideals were tied to what individuals could reasonably hope to attain, was selected for (so the argument goes).

What then specifically causes people to attach different amounts of importance to different ideal categories? We believe one major factor is self-perceived mate value. In some recent research, Fletcher and colleagues have found evidence for this proposition. Individuals who rate themselves as superior in terms of warmth and loyalty attach more importance to the same ideal standards, those who perceive themselves as more attractive and vital give more weight to the equivalent ideal category, and, finally, those who believe they have more status and resources rate this ideal category as more important (see Fletcher, 2002). Moreover, these findings are not produced by a general tendency simply for those who have inflated views of their own mate value to rate any old ideal standard as more important. Rather, there is clear evidence that the mental links between self-perceptions and ideal standards are funneled through these three distinct ideal categories (Fletcher, 2002). It is beginning to look as if this tripartite structure of mate standards is carving the intimate relationship mind at its joints.

The model presented by Gangestad and Simpson exploits a pivotal concept; namely, that people flexibly alter their cognition and behavior (consciously or unconsciously) according to interactions between their stored relationship theories and a range of other factors, including personal goals, biological variables, and the relationship or social context. For example, a well-replicated finding is that women prefer the smell of T-shirts that have been worn by men who are more symmetrical (the women have no idea who actually wore the T-shirts). However, the intriguing caveat is that this kind of female preference is only found when the women participants are near ovulation (Gangestad & Simpson, 2000). In this phase of the menstrual cycle, testosterone (which tends to increase sexual desire in women) is generally at its peak of secretion just when women are also most likely to conceive. In the same vein, other research has found that women increasingly prefer more stereotypical male faces (jutting jaws, angular face shape, and craggy eyes) as they near ovulation in the menstrual cycle (Penton-Voak et al., 1999).

Evolutionary-based models of mate selection typically frame their predictions and explanations in terms of two different goals—the search for a shortterm sexual fling or for a mate who would make a suitable partner in a long-term committed relationship (see, for example, Buss & Schmitt, 1993). In short-term sexual liaisons, women need to invest heavily in any subsequent offspring resulting from such a union, but will not have the benefit of a lifelong mate and parent for the children. Thus, in this context, women should be mainly on the hunt for an attractive hunk (good genes) rather than for a sensitive and supportive mate. In short-term settings, men also should not be much interested in their mate's suitability as a long-term partner, but, if they have a choice, should go for the best genes (e.g., the sexiest woman in the bar). However, because the potential investment in subsequent offspring for the woman is vast, compared to the man flitting through town, the woman should be even choosier than the man in this context.

Research has generally affirmed this theorizing. Several studies have found that when men and women are asked about their minimal requirements in a mate for a one-night stand, men typically express more modest requirements than do women on factors associated with warmth, loyalty, intelligence, status, and so forth (see Buss, 1999). Given that men are generally more persuadable than women when it comes to rapid sexual conquests, women can afford to be much choosier than men in such a context (see Clark & Hatfield, 1989).

The standards that are maintained most steadfastly across short-term and long-term relationships are concerned with physical attractiveness, and this is true for both men and women (Buss, 1999). These findings are consistent with the theory that physical attractiveness and vitality form the primary "good genes" factor—in a short-term relationship all one is getting out of the deal (reproductively speaking) are (potentially) the other person's genes.

In the long-term mating scenario, women should be exceptionally picky in terms of the factors that make for a good parent and a supportive mate—that is, warmth/loyalty and status/resources. They should also be interested in good genes (attractiveness and vitality), but they may be prepared to trade off such characteristics against the presence of personal warmth and loyalty or money and status. Men should certainly be more interested in the woman's ability to be a supportive mate and parent than in the short-term mating context, and they should also maintain their search for a woman with good genes; after all, men make substantial investments in parental and mating effort in long-term relationships.

However, in evolutionary terms, the woman's eggs are more or less all in one basket—the success with which she can pass her genes on are dependent on her husband (and wider family). In contrast, the man has more options. He can continue to spread his genes around while he is married, and he will remain fertile with the ability to father children for many more years than women are able to muster. Thus, evolutionary logic (derived from Trivers' parental investment theory) dictates that a high level of investment by the man should be more important to the woman, than vice versa (although, in absolute terms, high levels of investment should be important to both genders in long-term relationships).

Again, there is a wealth of research that supports the existence of related gender differences. In long-term relationships men tend to attach more importance to attractiveness and vitality than do women, and women tend to give more weight to loyalty and warmth and to status and resources than do men (see Buss, 1999). These findings have been found in many cultures, and have been replicated consistently within Western cultures by research using standard rating scales, or by analyzing the contents of personal advertisements. An important caveat is that the size and significance of such gender differences are sensitive to both how preferences are measured and the cultural context.

Take, for example, the mate ideal category of status and resources. When the importance given to this category of mate characteristics has been assessed by standard rating scales, using student samples from the United States and New Zealand, some studies have found no gender differences (Buss, Shackelford, Kirkpatrick, & Larsen, 2001; Fletcher, Tither, Durrant, & O'Loughlin, 2002). This lack of a significant gender difference in the United States and New Zealand is consistent with Eagly and Wood's (1999) recent work in which they reanalyzed data from the 37 cultures surveyed by Buss in 1989. Eagly and Wood found that as women's empowerment (indexed by their earnings, representation in legislative government, and involvement in professional positions) increased relative to men across cultures, women placed increasingly less value on the earnings of a mate. These authors, in turn, explained this finding in cultural terms; namely, women rate resources as more important in mate selection than do men simply because men tend to possess more resources than do women.

The finding that relative access to power and resources in specific cultures influences the importance placed on such factors in potential mates by men and women does not, per se, damage an evolutionary approach. An axiom of evolutionary psychology is that the unfolding interaction between environmental contexts and various (genetically encoded) dispositions produces behavior and cognition. However, if coming close to equalizing power and resources in a culture (as perhaps is true for university student samples in egalitarian Western cultures such as the United States and New Zealand) succeeds in wiping out gender differences in mate ideals, then this creates a problem for any evolutionary-derived thesis for their existence. If evolution has built in genetically encoded sex-linked differences that tend to push men and women in different directions in terms of mate criteria, then residual gender differences should remain when the proximal cultural contexts are equalized for men and women.

One possibility that rescues an evolutionary argument is that because university students (men or women) do not have much in the way of status and resources it is hardly surprising that current wealth and power do not rate very highly when they choose mates. To test this particular gender difference, using a more sensitive and realistic measure, Fletcher and his collaborators altered the ideal rating scales assessing status and resources to include the phrase "or potential to achieve" after each status/resources item (e.g., good job [or potential to achieve]). Presto! This alteration in wording produced a dramatic gender difference in the ratings (a finding we had never previously obtained) with women students at the University of Canterbury now rating the importance of this mate ideal category as considerably more important than did the men (Fletcher, 2002).

Perhaps the most famous, and bitterly contested, gender difference commonly cited in psychological evolutionary circles is that men, to a greater extent than women, have evolved a sexual system and set of desires oriented toward obtaining short-term sexual relationships. One apparently knockdown argument often leveled at this thesis is that any reasonable scenario of life in prehistoric Africa would involve a very limited access to fertile women, if we accept the standard view that our ancestors lived in small family-sized groupings, in a largely nomadic hunter-gatherer lifestyle (Angier, 1999; Hazan & Diamond, 2000). Our mythical Don Juan would have needed to range widely, and be reasonably choosy in terms of selecting women of the right age and status. For example, many possible sexual partners would have been breast-feeding a child, which severely reduces fertility by suppressing ovulation (Van Look et al., 1999). Moreover, any given act of copulation between fertile, healthy couples has only about a 1% to 2% chance of leading to offspring (see Fletcher, 2002). The one-night stand approach does not look like a particularly successful reproductive strategy for ancestral men, especially given the risks involved to life and limb from jealous partners and outraged family members.

However, the human intimate relationship mind has been evolving for an awfully long time. It is true that proclivities of the intimate relationship mind that imposed serious reproductive costs (without countervailing reproductive advantages) would probably have disappeared over the last 200,000 years of human evolution. However, both costs and benefits need to be taken into account in calculating reproductive success. It is not obvious that an overall tendency for men to be more turned on by short-term sex and sexual variety than women would necessarily have imposed a net loss in reproductive fitness. The key is that humans (both men and women) have evolved flexible mating goals and strategies, and that such strategies have evolved in a complementary fashion across genders.

The tendency of men to desire—if not always to achieve—sexual variety may have also served women well over the millennia (in reproductive terms) by enabling them to easily dip into a (superior) gene pool when the prevailing circumstances suited them. For example, when fertile women outnumbered suitable men, then a woman could make do with a homely and unattractive man who was happy to invest time and commitment, and with some judicious extrapair mating pick up some good genes elsewhere. On the other hand, if the women were outnumbered by the available men, a woman could obtain a handsome and athletic mate, but also afford to hold out for, and insist on, high levels of commitment and investment.

Evolutionary approaches to mate selection proceed by borrowing theories developed from within sexual selection theory, such as parental investment theory, and using data from other species (from birds to primates) for illustrative purposes or as supporting arguments. There is nothing wrong, *per se,* with doing this—such an approach is extremely powerful. However, this work often falls into the trap of treating mate selection as a one-shot process in which men and women weigh up the mating criteria and choose a specific partner, specifically in relation to whether they are pursuing a short-term or a long-term sexual relationship. This focus, in isolation, misrepresents the nature of the human intimate relationship mind. What is needed is supplementation from a social psychological analysis.

The Multiple Functions of Mate Ideal Standards

Mate selection cannot be properly understood apart from mate deselection. In both cases the critical proximal-level forces that drive the development of the relationship comprise first, each individual's relationship evaluations, and second, the nature of the behavioral interaction between the two individuals. These two factors are the best predictors of relationship dissolution (Karney & Bradbury, 1995). However, ideal standards are not put to sleep after a mate is chosen, and continue to be pressed into action (often unconsciously) throughout the course of the relationship, helping drive both the behavioral interaction and the relationship evaluations of relationship participants (Simpson, Fletcher, & Campbell, 2001).

From a social cognitive standpoint, ideal standards comprise a set of expectations and beliefs that operate in that part of the intimate relationship mind that (potentially) predate local relationships. They should be constantly accessible mentally, and be readily triggered off by any event or stimulus related to sexual relationships. However, such ideal standards continue to be used after mates have been chosen and commitments forged by being rapidly (and often automatically) compared to relevant perceptions of the partner or relationship. The gap between mate ideal expectations and perceptions of reality then can be used to accomplish four pivotal major goals in intimate relationships—evaluation, explanation, prediction, and control.

Take Fiona, who places huge importance on passion and sex in relationships and, thus, places a premium on vitality and attractiveness in evaluating a mate. Fiona was very attracted to Charles initially, mainly because he was athletic and attractive. Two years into the relationship, Charles has gained a lot of weight, and he has lost interest in going to the gym. Fiona's evaluations of Charles are, as a result, on the slide, and she is having doubts about the long-term future of the relationship (the *evaluation* function). Fiona can use the gap between her ideals and perceptions to help provide her with an explanation of why she is dissatisfied with her relationship—Charles is letting himself go (the *explanation* function). Fiona can also use the gap between her ideals and perceptions to predict the future of the relationship—unless Charles takes himself in hand, the relationship is doomed (the *prediction* function). Finally, on the basis of her evaluation, explanation, and prediction, Fiona may actively attempt to change her partner's behavior; for example, by buying Charles a year's subscription to a health club for his birthday (the *control* function).

Fletcher and his colleagues have been carrying out a program of research concerning the nature and functions of these three main categories of ideal standards. This research has substantiated the theorizing about Fiona and Charles, along with a few twists. We have found that mate-ideal standards not only operate to screen out inappropriate partners who fail to meet minimum standards, but also are used to drive evaluative judgments of ongoing relationships. Thus, the smaller the gap between the perceptions of the partner and the importance attached to ideal standards, the more positively people will evaluate their relationships (the associated correlations vary from .30 to .60) (see Fletcher et al, 1999).

In one study we tracked 100 individuals across the first 3 to 4 months of their new dating relationships (Fletcher, Simpson, & Thomas, 2000). The closer the gap between individuals' perceptions of their partners and their ideal standards, the less likely the relationships were to break up 1 month later (not surprisingly, the breakup rate was relatively high in the early stages of dating relationships—over a 3-month period, 46% of the relationships broke up). Campbell, Simpson, Kashy, and Fletcher (2001) found that the higher individuals set their ideal standards, the more demanding they were in terms of how closely they expected their partners to match those ideal standards. Although this may seem paradoxical, it is understandable given that individuals with more positive judgments of their self-perceived mating value (e.g., on the vitality-attractiveness dimension) possess both higher ideal standards and less flexible ideal standards. For example, if Mary perceives herself as very fit and highly attractive, she can set high expectations for obtaining a partner who is also highly fit and attractive. Moreover, if the chosen partner subsequently turns into a "couch potato" and gains weight, then she is in a strong position to demand her partner changes or she will look for—and probably find—an alternative partner who meets her exacting standards.

We have argued that human mate selection does not start and stop after initial mate selection occurs. It is a continuous process, with essentially the same psychological processes at work throughout the course of the relationship. No other animal species (including primates) exhibits the same degree of flexibility, fluidity, and conscious control revealed in the development of human sexual relationships. Note that this claim is not inconsistent with the prior analysis concerning love, in which we pointed out that the mate selection module switches to "low" or even "off," when individuals are in love or in happy relationships. As love and happiness slide in relationships, then this tends to turn the search for an alternative mate on again.

Of course, intimate relationships develop along familiar trajectories, with substantial changes taking place in the nature and complexity of the associated (lay) local relationship theories. Nevertheless, the full-blooded resources of the intimate relationship mind are pressed into action from the time boy meets girl (or girl talks to boy in a Web chat room). Humans select both mates and relationships, and they never cease so doing.

IMPLICATIONS

How do the two approaches we have devoted the most attention to—social psychology and evolutionary psychology—stack up, and how do they fit together? Obviously, they offer complementary theories and models, with standard social psychological theories focusing on the proximal-level cognitive machinery and behavior, and evolutionary theories concerned with origin accounts, and associated evolutionary adaptations, of the intimate relationship mind. Humans are animals that have evolved in the recent past (in evolutionary terms) from other animals. Thus, it is hardly surprising that footprints from our evolutionary past can be seen all over the intimate relationship mind.

Asking the right questions is one of the main tricks in doing good science, and evolutionary psychology encourages psychological scientists to ask a key question. What are the functions of ordinary human cognition and emotion? Social psychology has sometimes failed to ask this question effectively, for example, by proposing that people possess a built-in drive to be happy, or to attain high self-esteem, as ends in themselves. The aims of being happy or having skyhigh self-esteem are not, on their own, sensible functions. The psychological reality is considerably more complex.

Evolutionary psychology and social psychology need each other. Perhaps because they have been the new kids on the psychology block, evolutionary psychologists have concentrated on persuading other psychologists, with evangelical fervor, that they should pay close attention to evolutionary psychology As evolutionary psychology steadily infiltrates mainstream psychology, and the missionary zeal softens, evolutionary psychologists should take on board the proposition that they pay close attention to the work of social psychologists (and others), which details how the intimate relationship mind (and behavior) works at the proximal level. One example is the social cognitive research on mate selection, which reveals the ways in which mate standards and expectations help drive relationship judgments and processes throughout the history of specific relationships, up to and including decisions to leave.

It is not just evolutionary psychologists who get into trouble through ignorance of the social psychological work on the proximal-level processes of the intimate relationship mind. The evolutionary biologist Steve Jones, in his recent book, *Darwin's Ghost,* ridicules the sexual selection argument for the human male's large penis on the (accurate) grounds that, in almost every known culture on earth, human genitals are hidden by clothing. Thus women do not get to view the size of their partner's penis (flaccid or erect) before choosing them. Quite so. But Jones falls into the fallacy of assuming that mate selection occurs in humans only at the beginning of a sexual relationship (like virtually all other species), whereas we know that mate selection in *Homo sapiens* is a continuous process that operates throughout the long (or short) history of a relationship. Eventually, couples disrobe and have sex, and the moment of truth arrives. The size and shape of one's partners genitals (in sexually aroused or in other states) becomes apparent, along with his or her sexual skills, proclivities, and appetites. Both performance and appearance are eventually judged and found wanting or otherwise.

How does a central assumption of evolutionary psychology—that the human mind is fundamentally modular—stand up? Ripping apart complex relationship phenomena and analyzing them in terms of independent modules is a valuable strategy. For example, it is clear that adult human love is composed of three components (commitment, intimacy, and passion) that are represented separately in lay cognition, are underpinned by different neurophysiological and hormonal systems, and are capable of acting independently. Viewed in this way, the assumption of modularity can be viewed as a useful heuristic for both theory and research. However, this kind of modularity is variable, with different units sometimes acting in concert and profoundly influencing one another. Moreover, humans have the ability to cognitively join together what biology and genes have originally kept apart as modular, unconscious, and instinctual processes.

What, if any, are the unique features of human mating and the intimate relationship mind? A lengthy list of purportedly unique characteristics of human mating has been proposed over the years, but many of these have turned out not to be unique at all. For example, it has been suggested that humans are special in that ovulation is hidden in human females, who are constantly sexually receptive. But female chimpanzees have sex regularly, even when not in estrous. Moreover, echoes of the ancient cycle of sexual receptivity tied to ovulation remain present in women's menstrual cycle, with fluctuating hormonal levels producing a peak of sexual interest around the time of ovulation. Women are certainly at the extreme flexible end of the biological continuum when it comes to sexual receptivity but it is a continuum rather than a yawning chasm that separates humans from other species in this regard.

Flexibility in mating is a hallmark of the human species, with mating selection and deselection processes operating throughout the course of relationships. Human males are unique among primates in the amount of care, support, and provisioning they supply their mates (although this kind of behavior is found in monogamous birds). Human males will also help care for the children of the women with whom they form sexual relationships, even though they know they are not the genetic father of the children (although they do not typically offer the quality of care that genetic fathers provide). This kind of behavior is in stark contrast to almost every other mammal, who at worst kill the offspring (they did not father) of new mates (like lions) or, at best, ignore them.

We say almost every other mammal, because there exists one remarkable exception to the general rule—the gibbon. Observations of gibbons in the wild by Palombit (1994) have revealed a pattern of mating and caregiving that is eerily similar to humans. Gibbons are monogamous, but both male and female gibbons will leave their mates if their relationships become problematic, especially if other available single gibbons of the opposite sex appear on the scene. Moreover, the new male in the gibbon nuclear family will adopt any unrelated infants and help care for them (e.g., by carrying and defending them). However, there are hints that (like humans) stepfather gibbons are not as solicitous as genetic fathers. To complete the analogy with humans, Palombit suggests that gibbons continuously monitor the quality of their mates, while taking into account the availability of alternative mates.

One feature of human mating that we have not mentioned thus far concerns the fact that women can live for decades after menopause, a feature unique among primates. The evolutionary reason for not living much past menopause is obvious—why bother extending life for decades when the ability to reproduce is gone? The consensus view has been shifting toward the proposition that this characteristic feature of women is an evolved adaptation, based on the evidence that grandmothers play an important role in many cultures (including hunter-gatherer cultures) in helping to raise their own grandchildren (Hrdy, 1999). Thus, the concept of inclusive fitness is drafted into action—helping one's close genetic relatives to survive and reproduce is the next best thing, genetically speaking, to successfully rearing one's own offspring.

To sum up, human mating behavior, desires, and goals are clearly at the flexible end of the reproductive spectrum compared to other species. However, comparisons with other species show that continuities are ubiquitous, and reveal our mammalian and primate evolutionary heritage. We return, then, to what is truly unique and most remarkable about the human species; namely, the development of the human cerebral frontal cortex, in conjunction with the ability to use language and the ability to learn and to plan ahead. These capacities have generated the development of culture and technology, which have enabled humans to take a step that no other animal has managed—to figure out how sexual reproduction works. This step potentially short-circuits the evolu tionary feedback mechanism of reproductive success as the lever of biological evolution.

Understanding the causes of reproduction does not turn off the sexual desires, goals, and emotions that evolution has programmed into humans. However, it does mean that humans can fall in love and have sex, but prevent having offspring if they so choose. The choice not to reproduce is open to humans— and is being taken up in droves in countries with high standards of living—but closed to other species.

CONCLUSION

The relationship between evolutionary psychology and mainstream psychological approaches that focus on proximal-level processes (e.g., cognitive, social, or neurophysiological psychology) has survived its baptism of fire, but continues to be strained and even stormy at times. However, we believe the relationship is worth nurturing and exploring. The research and theorizing on the intimate relationship mind, and the area of relationship science more generally, provide powerful case-studies for the advantages and mutual benefits to be gained from pursuing such an interdisciplinary enterprise.

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5

Evolving to Be Mentalists

The "Mind-Reading Mums" Hypothesis

RAEWYN BROCKWAY

Our human capacities to "read" the mind states of others are sophisticated and complex. There are few hypotheses about how (i.e., to what advantage) these capacities might have evolved. Two candidates are the Manipulation Hypothesis (MH) and what may be dubbed the Intervening Variable Hypothesis (IVH). I argue that the MH, or "machiavellian intelligence"— gaining Darwinian advantage through manipulation—falls short as an explanation. There are probably severe constraints on the frequency with which deception and other hostile manipulations may be used to one's own advantage. Just as importantly, in humans, such behaviors require quite complex, sophisticated mind-reading if they are to lead to lasting and decisive benefit for the perpetrator. The MH leaves unanswered the question of how the many simple components of such mind-reading might have been selected for (i.e., how they might have been advantageous in their own right). The IVH, also, seems to be an incomplete account, especially with respect to what adaptive problems may have created a selection pressure for more efficient or "smarter" behavior reading, and thence for greater mentalistic abilities.

This chapter hypothesizes that during hominid evolution there were strong selection pressures for smarter, more efficient mothering, and that even the simplest components of our current theory-of-mind capacities may have been useful for promoting the survival of offspring. I give six examples of rudimentary or partial mind-reading feats that may have been selected for in this context. Such a selective mechanism would escape frequency constraints, and accords better than machiavellian intelligence with observed gender differences (female superiority) in some mind-reading capacities in humans. It would also help account for why so much of our simplest, almost unthinking mentalism is deployed to help others or cooperate with them, rather than to compete with them. Truly expert deception and manipulation might be an exaptation from more primitive child protection rackets.

WHAT DOES MIND-READING INVOLVE?

Theoretical Orientation and Terminology (a Quick Romp)

Much has been written about whether any nonhuman apes have a full, or fullish, theory of mind. Some, the simulation theorists, have questioned whether we have one ourselves! To an extent, this chapter side-steps the important debate between theory theories and simulation theories. But the question of how, exactly, complex mind-reading is pulled off—whether by having theories or by running simulations—is distinct from the questions: Has it been selected for? and, If so, how? What, in its most primitive and rudimentary forms, was it good for? Simulationists and theory-theorists alike might be interested in these questions.

The present author holds that some kernel of theory is essential for sophisticated mind-reading, and finds it natural to use folk psychology terms such as “the belief that,” “wish,” “desire,” “knowledge,” “ignorance,” and so forth, which are often associated with the ‘theory-theorists.’ I also use the concept of orders of intentionality (as per Dennett, 1987, especially Chapter 7), as a useful terminology for describing how many layers of nesting are required for a particular mind-reading feat.

To briefly define this terminology, first-order intentionality means having beliefs, desires, and some basic rationality or set of rules which governs the way in which beliefs and desires influence one’s behavior. It means having a few brains, but not necessarily even knowing that you have them. Second-order intentionality (SOI) means being aware of others’ beliefs, desires, and rationality: understanding that others are themselves intentional systems or agents, who accordingly have a point of view and beliefs that might differ from ones own. SOI is the crucial step in mentalistic understanding: it requires that you realize that others do have brains; it requires that you represent their mental processes *as* mental processes.

With Dennett, I conceive of attributed beliefs very broadly, and allow them to include the attribution of knowledge or ignorance. My definition of intentionality is a little broader than Dennett’s, as I include awareness of others’ attentional states. This is in line with, for example, Tomasello and Call’s discussion (1997), where understanding attention intentionally—grasping that another animal is attending to something—is distinguished from merely following another’s gaze direction (which behavior might occur reflexively or through associative learning, and without any appreciation of the other’s subjectivity or intentionality).

Another recent discussion point in the literature is our need to get away from an all-or-nothing approach to whether an animal possesses a theory of mind. I follow Tomasello and Call (1997) and Whiten (1996) in urging that we should recognize degrees of mentalistic understanding. This can apply both to comparisons between species and to ontogenetic development: Chimpanzees might display more mentalistic awareness than donkeys, even if they cannot match human achievements; 1-year-old humans might understand something of another's desires, but still be a few years away from sophisticated perspective-taking, or passing false-belief tests. In this chapter, I argue that human mentalistic abilities emerged bit by bit phylogenetically, and I relate their usefulness to the fact that they also emerge in a gradual way ontogenetically.

Empathy and Its Relation to Mind-Reading

The ability to "read" and react to a conspecific's emotions is widespread among group-living mammals and probably has a long evolutionary history. The adaptive significance of emotional contagion and empathy has been discussed as far back as Darwin (1998/1872); he and others (e.g., McDougall, 1923/1908) saw it as important to facilitating and cementing the mother-infant bond, and thus important to successful infant rearing.

More recently, Hrdy (1999) has discussed the importance of primates' lengthy infancies and lactation to sociability and the capacity for compassion. She observes that primate mothers are instrumental in imparting knowledge of the group's social relations and in determining which individuals the infant will accept as friends or kin. The prolonged, close bond between a lactating mother and her infant would have

> promoted the social *relationships* most conducive to infant survival and to the mother's long-term reproductive success…. Prolonged association between mother and suckling young provided both the chance *and* necessity for 'social intelligence' to evolve. (p. 145, italics original)

However, there is a difference between having highly developed emotional empathy and complex social relationships, and having a theory of others' minds. Many mammalian species exhibit the former two, but few show evidence for the latter. Preston and de Waal (2002) observe that

> While the mother-infant bond is surely important for developing empathy, this does not allow automatic forms of empathy to be linked with cognitive forms, or explain why we experience empathy for nonoffspring. (p. 6)

In relation to the second question, they reject inclusive fitness as an explanation, arguing persuasively that empathy is just one result of a generally adaptive nervous system design. On their Perception-Action Model (PAM), animals, for reasons of cognitive efficiency and survival, have the closest of links between their representations of the environment and their motor responses to these representations. When mammals live in social groups, their conspecifics become an important part of their environment. Perceiving the emotions of conspecifics often leads automatically to a mirroring of the perceived emotional expressions or displays in the perceiver's motor cortex, and thence to a subjective experience of the same emotion (which may or may not be toned down or otherwise modulated). Many mammals thus know how a conspecific is feeling, because they can feel it too. But consciously knowing *that* the conspecific is feeling it, let alone having a cognitive theory about the workings of the other's mind, does not automatically follow. However, the PAM, if valid, suggests the possibility that animals might have a built-in predisposition to link their phylogenetically older emotion-reading with more recently evolved cognitive skills.

On the question of how the empathic mother-infant bond might "allow automatic forms of empathy to be linked with cognitive forms" (such as tailored helping), or how this special social relationship might have been the locus of the evolution of our theory of mind, the literature says very little. It is this subject that the present chapter seeks to address.

What is known about the factors that promote empathy? There is robust empirical evidence across many primate species (summarized by Preston & de Waal, 2002) that certain factors increase or intensify empathy. These factors are: (a) the subject animal's similarity to the observed animal (the object); (b) the subject's familiarity with the object (i.e., empathy is more likely to occur, and to be stronger, when there is a social relationship between the animals); (c) learning, and the subject animal having had past experience of what the object is currently experiencing; and (d) cue salience. Clearly, all of these factors are present in the primate mother-infant relationship: there is close genetic relatedness; the mother has known her infant all of his life;[1] the two of them are sharing many current experiences and (in more recent phases of our evolution) the mother will be able to draw on her own memories of childhood to enhance her understanding of her infant's experience; finally, she is heavily invested in the offsprings welfare so that she is attuned to his difficulties or displays of distress. He has her attention. These themes recur throughout this chapter, as I develop scenarios to show how the mother-infant empathic bond may have served as a springboard for cognitive forms of empathy and other mentalistic insights.

THE NEED FOR A NEW HYPOTHESIS

What adaptive advantage might there be in having mentalistic insights, rather than simply relying on past behaviors and/or simple associative rules to predict or explain the behavior of one's conspecifics? This is a serious question, for with any move to a more sophisticated cognitive machinery, there is the metabolic cost of more brain tissue (Aiello & Wheeler, 1995) and the risk of breakdown or error: Simple systems have less that can go wrong; new abilities to represent carry with them the risk of misrepresentation. Thus the challenge is to find clear advantages to an organism of having the cognitive machinery that permits insights about the mental processes of others.

The two most widely discussed suggestions about the raison d'etre of primates' mind-reading capacities are the MH, and the IVH.

The Intervening Variable Hypothesis

According to the IVH (e.g., Whiten, 1996), the attribution of mental states isadvantageous for reasons of cognitive efficiency. In the absence of such attributions, an animal cannot make predictions of conspecifics' behavior without observing and learning a bewildering array of inputs (circumstances) and outputs(behaviors). However, once one has a concept of another's desires and/or beliefs, one can predict that the other will tend to act on them consistently, acrossa variety of circumstances that may cause the same beliefs and desires to beexpressed in a variety of ways. Behavior that looks diverse on the surface may beexplained—or predicted—in terms of the underlying intentional state. This mightpermit the speedier learning of useful behaviors that involve one's conspecifics,and which require flexibility according to the circumstances. Such an ability totranslate from one set of circumstances to another would allow one to betterpredict behavior in novel circumstances.

A problem with this hypothesis is that it is difficult to find examples, even of true novelty, that really require the attribution of intentionality to others, rather than the application of rules of thumb. There is thus the worry that if such a distinction is rare in nature, it would be difficult for natural selection to act on it. For example, why would you need a concept of your parents' superior knowledge instead of a simple rule to do what they do and avoid what they avoid? Furthermore, if you start to wonder on a case-by-case basis whether Mum really does know what she's doing, you introduce the risk of getting it disastrously wrong, without any apparent compensating advantage.

In my view, there is also a bootstrapping problem inherent in Whiten's account of how an appreciation of mental states as intervening variables could have evolved in the first place. He suggests an analogy with behavioral psychologists who statistically tally and analyze the effect of a range of inputs on a range of output behaviors. After observing that circumstances A, B, and C each lead to increases in behaviors X, Y, and Z, the psychologist (or chimpanzee, or hominid) realizes that it all makes simple sense if A, B, and C are seen as inducing a certain mental state (e.g., fear), while X, Y, and Z are all the diverse ways in which the observed animal expresses, reacts to or deals with this fear. So, on this account, the attribution of a mental state results from a complex behavioral analysis that somehow must have been achieved *prior to* and without the help of knowledge of what it was all about. Mentalistic insights save us hard behavioral analysis—that's why we have them, they're very economical and efficient— yet each individual still has to do the complex, costly and error-prone behavioral analyses before being able to employ the easier, more efficient mentalism.

Whiten himself seems to sense this problem, as he states

> If mentalism conceived in this way owes its existence to cognitive economy, it may appear a paradox if our current working hypothesis that it

> is refined only in particularly clever species like apes…is confirmed. The answer may depend on distinguishing the process of *acquisition* of mental state discrimination from the *application* of mentalism on subsequent… occasions. (p. 287, italics original)

That is, once you've "got it," it's easy.

But on Whiten's account, the cognitive capacity for doing the behavioral analysis, and the performance of the analysis itself, is still needed in the ontogeny of each individual: We all had to start out very clever in order to become a bit lazier. Intuitively, this seems puzzling, for we automatically and effortlessly attribute mental states, and if we couldn't do this we would be like sufferers from autism, scarcely knowing where to begin with behavioral analyses of our conspecifics.

The notion of mentalistic insights as as a powerful, efficient way to organize one's behavioral observations and make predictions remains attractive and interesting. But it seems to me that any animal smart enough to infer mental states as intervening variables from behavioral analysis *alone* is a lot smarter than I am. For Whiten's argument to work, we surely need to posit some specialized cognitive machinery for this task (akin, perhaps, to our astonishing knack for language acquisition, which fades away after the first decade of life). But the need for specialized cognitive machinery, metabolically expensive and error-prone, during the vulnerable early years of life pulls us away from "cognitive economy" as a selection pressure. We are still left wondering what the mindreading was *good for* in survival terms—what adaptive challenges it solved—to make it worth our ancestors' while to become mentalists. Whiten's account seems incomplete.

The Manipulation Hypothesis

The MH (e.g., Whiten & Byrne, 1988a, 1988b) suggests that social intelligence is primarily for the manipulation of others' behavior: using deception, distraction, coalitions, and other tricks to make others' knowledge or actions further one's own purposes.

Dawkins and Krebs (1978) had earlier argued that because animals are selected to behave in ways that further their own genetic interests, to talk of "communication" between animals is misleading. It is in part a carryover from human communication. The sender of animal signals is not selected to give reliable information to the recipient (which is what we usually think of as happening when humans use language). Manipulation, not communication, should be the paradigm we adopt when studying animal signals. Dawkins' and Krebs' paper was also in part a rejoinder to earlier group selection theories that emphasized cooperation and thus information sharing.

> The other attitude, which we espouse, emphasises the struggle between individuals. If information is shared at all it is likely to be false information, but it is probably better to abandon the concept of information altogether. Natural selection favours individuals who successfully manipulate the behaviour of other individuals, whether or not this is to the advantage of the manipulated individuals. Of course, selection will also work on individuals to make them resist manipulation if this is to their disadvantage…. [In this case] an evolutionary arms race will develop. (p. 309)

A similar emphasis on manipulation or social gamesmanship was already present in the pioneering primate social intelligence papers of Chance and Mead (1953) and Humphrey (1976)—although not, I think, in Jolly (1966)—and this emphasis has been enormously influential in shaping our views of what social intelligence and mentalistic abilities in primates might be for or about. Jolly (1991) notes the paradox that deception in group-living, cooperative primates offers us our best chance of studying their mental calculations.

> Possibly much of chimpanzees' cooperative behaviour also involves a mental picture of others' emotions, but we cannot often be sure…. Sharing much coveted meat with begging friends could just be a forced or unconscious response to the others' gestures of begging—or it could involve accurate and very human empathy (pp. 244–245)

Our very fascination with machiavellian carryings-on among primates is *prima facie* evidence for such a strand in our make-up! But now that the old spectres of group selectionism and anthropomorphism are not quite so threatening, and a possible observation bias has been gently pointed out, it is time to consider whether the MH is a sufficient explanation for human (or indeed great ape) mentalistic abilities.

For one thing, there are probably quite severe constraints on the frequency with which deceptive behaviors can be used to advantage. Dawkins and Krebs (1978) observed that on the face of it, deception must be infrequent in order to succeed. But if deception is indeed advantageous, the 'gene for deception' should spread rapidly, and once this had happened, it would no longer confer an advantage. It is difficult to construct models for infrequent, highly advantageous deception as an evolutionarily stable strategy.

Subsequent commentators have generally agreed that there are severe frequency constraints on manipulation and deception. Cheney and Seyfarth (1991) discuss the basic need for mutual cooperation in group-living primates, and the important roles of the recognition of individuals and repeated encounters; past interactions may be remembered (or a sense of chagrin and distrust may linger on), so that the currency of duplicitous social overtures would be expected to rapidly lose value. There also may be physical and cognitive constraints on the deceiver's ability to come up with new, improved deceptive signals (whether in his own lifetime, or over evolutionary time). Moreover, a cognitive "arms race" between the perps and victims of deceit can only get going if the intended victim can afford to be skeptical. Johnstone (1997) also reviews the physical and strategic constraints on deceit, and includes the high risks of having one's bluff called in hostile encounters. And in fact deception and manipulation, although widespread among great apes, occur at low frequencies in the daily life of any individual (Byrne & Whiten, 1990; Whiten & Byrne, 1988a). There remains a question mark over whether behavior that must occur at low frequencies can suffice to account for ubiquitous mentalistic capacities.

I suggest that there might be a further general problem with the MH, arising from the fact that our "full" human theory of mind is a large achievement. Our intense interest in primate deception, sign language acquisition and tool use, our exploration of our kinship with the great apes, must not obscure the enormous gulf that exists between their cognitive abilities and our own. Primates other than ourselves occasionally use deceptive and manipulative behaviors, yet do not show convincing evidence for possessing a theory of mind (e.g., they do not convincingly pass false-belief tests. See Tomasello & Call, 1997 for a detailed discussion). Great apes' deceptions generally can be interpreted as behaviors that advance their own practical agendas—gaining food and mates, evading punishments—rather than necessarily being aimed at inducing false beliefs *per se* in their competitors.

Why were the mentalistic capacities that were good enough for other great apes not good enough for our own evolving ancestors? Why did hominids at some point need more? Dennett points out (1987, p. 51) that second-order intentionality is a rather loose cluster of perceptions about others, each of which likely evolved at a different time. He characterizes SOI as nothing but a bag of tricks (rather than being a single, seamless, stupendous achievement). Here, I wish to emphasize the corollary that full SOI is an *entire* bag of tricks, or, perhaps, a rope built up of several differing strands. For modern humans to gain any selective advantage by deploying it in the ways suggested by the MH, they must be able to make:

- Aaccurate attributions of feelings, attitudes, desires, and attentional states,
- Accurate attributions of belief (including states of knowledge and ignorance)
- Accurate attributions of basic rationality
- Correct predictions of behavior based on these attributions, and
- Judgments about when to utilize these abilities—how often, and against whom—and when it might be wiser to desist

It is not enough that this final package should be advantageous (if indeed it is, which I hold has not been conclusively proven by either the MH or the IVH). If several quite different perceptive and cognitive abilities are involved in SOI, each and every component must have developed, somehow, before the final package could be put together.[2]

To compound this difficulty, SOI, although too complex to be plausibly considered a single ability that could have arisen all at once, still does not seem sophisticated or clever enough to deliver decisive competitive advantage for modern humans. Even disregarding frequency constraints, many manipulative or deceptive interactions in humans rely on third-order (or even higher-order) intentionality (e.g., “I want {you to believe that [I don’t know of any mechanical faults in this wonderful car that I’m trying to sell you]}”: third-order intentionality. I know that you don’t want to buy a lemon: second-order. But you know that I know that: third order. I know you do: fourth-order.).

As I thought about deceptive interactions, trying to make machiavellian manipulations work properly as a driver of evolving mentalistic insights, I had difficulty imagining what relevant social complexity might have pertained among our ancestors to serve as an adaptive problem to be solved, over and above what pertained among other great apes. I also had difficulty thinking of very simple second-order cognitive feats or mentalistic insights that would both: (a) allow an early hominid to deceive or manipulate someone else to his own clear reproductive advantage; and (b) provide clear advantage over simple associative learning, thereby earning their keep in terms of metabolic and error costs.

These difficulties may, of course, signify a failure of my own imagination, and others might have more luck than I in filling in these gaps. Be that as it may, the simpler the examples of strictly second-order mind reading I could think of, the more likely they were to be used by humans in cooperative or even altruistic interactions, rather than competitive: teaching, information sharing, warning others of hazards that they don’t seem to know about, responding to perceived distress or want…. The most primitive and rudimentary mentalistic insights—which surely must be in place before full SOI, let alone higher orders of intentionality, can be deployed—seem more often suited to *helping* others than to doing them down, which of course is no good at all for the relative reproductive success of the organism who has the insights.

Unless, of course, the other who is being helped by your insights into their beliefs, desires and rationality, happens to be your own kin. This was the thought that led to the present hypothesis: Even the simplest and most rudimentary mentalistic insights—simpler even than SOI—might benefit one’s offspring *and* bring clear advantages over simple associative learning (behavioral rules of thumb), thereby explaining why we evolved to be mentalists.

This chapter tries to meet the challenge of showing how each and every component of our theory of mind could indeed have been useful, and therefore selected, in the context of rearing dependent infants. I try to develop an account of how these abilities could have evolved as simple extensions of behavior that was occurring anyway (whether instinctively or as simply learned rule-based behavior). Thus, they did not require any startling degree of intelligence, complex condition planning, or innovative genius on the part of great ape or hominid mothers.

As gratifying as it would be to heap lavish praise on our ancestresses' braininess, this chapter aims rather to develop a version of mind-reading that would have been obvious rather than esoteric, a version that would have been simple for a mother to apply, a version that fitted in nicely with her existing motivations (rather than requiring feats of superhominid willpower or self-denial), one that she could have applied dozens if not hundreds of times every day, rather than anything rare and special; one that happened also to be a matter of life and death for her infants.

THE EVOLUTIONARY CONTEXT OF COMPLEX MIND-READING: WHEN DID IT ARISE?

When and why did it became worthwhile for mothers to have a theory of mind? As observed, sophisticated theory of mind seems to be a uniquely human bag of tricks. But if the scenarios I develop later in this chapter are so nifty and childpromoting, why isn't sophisticated mind-reading ubiquitous in the animal kingdom? Why does it seem to be an evolutionary latecomer?

An obvious if facile answer is that mentalistic insights take a lot of brain power —more than is found in monkeys and lemurs, certainly more than is found in weasels or sheep. After all, the greater encephalization of primates, compared with most other mammals, is already pronounced in the more primitive species (prosimians and monkeys) (Jerison, 1973), which display no signs of mentalistic abilities. Although this explanation may be true, it could be accused of begging the question, so I make some broad suggestions about why mentalistic abilities may be evolutionary latecomers.

Historically, social intelligence hypotheses have suggested that large group size in primates promoted social intelligence as a means of coping with social complexity. In the process, as the players grew smarter, more social complexity arose. Social intelligence in turn may have driven the evolution of other cognitive capacities, through exaptation or through the general brain-building effects of nutting out social problems. This general view is quite well supported: For example, across all primates there is a positive correlation between neocortex size and group size (as discussed by Dunbar, 1998). But there are bound to be twists and turns in the social intelligence story. It would be absurd to expect social complexity or social intelligence to explain everything.

Several nonsocial intelligence hypotheses have been advanced to account for the higher intelligence of great apes compared to monkeys, which does not seem explicable on group size alone. For example, Mackinnon (1978) suggested that locomotor slowness and inefficiency in our common ancestor with the chimp, in the context of a challenging feeding environment, may have selected for more sophisticated mapping of the physical environment. Povinelli and Cant (1995) relate arboreal clambering to advance planning and metarepresentation of body movements. Specialized food extraction and hierarchically organized tool use are discussed by Byrne (1997).

It seems plausible that a comparable range of selection pressures was at work during hominid evolution, and that there were times when nonsocial intelligence, particularly where it involved technology, leap-frogged ahead and created selection pressures for social intelligence to develop still further. Technology and acquired knowledge can only benefit a new generation if they are well taught and well learned. It will be apparent that several of the rudimentary mind-reading feats suggested in the following will only be useful in a species that already has something worth teaching to an infant (that is, a species that has at least a simple culture and/or technology to impart). At some point in our ancestry, intelligence seems to have become an attractive and desirable trait; therefore, sexual selection may have been yet another factor in the remarkable and extravagant brainpower we have evolved. The mind-reading scenarios I offer in this chapter therefore should be seen as just one set of selection processes among several, in a lineage that was becoming ridiculously brainy over a short period, evolutionarily speaking, and probably for a multitude of reasons.

Of the new and different'factors at work in hominid evolution, bipedalism is perhaps the most interesting. It seems generally accepted that bipedalism evolved for reasons of locomotive efficiency and/or heat regulation, and that the explosion of intelligence in our ancestry occurred afterward.

There have been several intriguing suggestions about how bipedalism may have created conditions that accelerated our cognitive evolution. For example, bipedalism, by creating narrower pelvic outlets, may have led to less mature, more vulnerable infants. Savage-Rumbaugh (1994) suggests that once bipedalism arrived, infants could no longer reflexively cling, on account of their mothers now being upright (and also because of the infants themselves now having hind legs that were made for walking, and that didn't cling so well any more). Thus they had to be actively carried by their mothers, producing a selection pressure for mothers who could successfully monitor and juggle the infant's needs with other (e.g., food-acquiring) activities. Carrying infants may have preadapted the brain for carrying tools and weapons. Having to sometimes put infants down, and therefore having to remember to pick them up again, may have encouraged similar behaviors with respect to tools and weapons, leading eventually to the notion of permanent possessions that hominids kept with them against the possibility of future need.

Especially pertinent to the present hypothesis, Savage-Rumbaugh notes that bipedalism and the need to carry infants: (a) transferred monitoring responsibilities from the infant to the mother (or parents); and (b) brought about more face-to-face interaction between mother and infant, increasing the importance of facial signals and the ability of each to monitor the other's direction of attention.

Lengthy childhoods probably date from the time of *Homo erectus* (Smith, 1993; Walker & Shipman, 1996). I suggest that the immaturity of the *H. erectus* infant and his long period of dependence on the mother created conditions that allowed him to benefit from maternal *mentalistic* insights. Longer dependency of infants and juveniles meant a greater burden for mothers, which in turn provided a selective pressure for smarter mothering. At some stage of our evolution, perhaps with *H. erectus,* perhaps earlier, mothers had to safely get their offspring through a toddler stage characterized by rapidly increasing mobility but lousy coordination, ignorance of hazards, impulsiveness, and physical puniness. Toddlerhood may have been the decisive new mothering challenge that accelerated the selection of mind-reading. And perhaps efficient bipedal locomotion and two free arms also gave hominid mothers the physical means to translate mentalistic insights into decisive actions on behalf of their offspring.

Additionally, altricial development meant that the mothers themselves were successful graduates of a long infancy: Social learning was already important for them, and their lifetime habits of observation, prediction, and recall may have transferred quite automatically to their dealings with their offspring (see also Chisholm, this volume).

Therefore, I think there are reasons to place many of the following suggested mechanisms for the selection of mind-reading abilities no earlier than our common ancestor with the chimpanzee, and some of them later, subsequent to bipedalism and perhaps even subsequent to the arrival of the large, slow-growing *H. erectus.*

SIX VERY SIMPLE WAYS TO READ AN INFANT'S MIND (AND MAYBE SAVE HIS SKIN)

Note that these are more or less independent of one another. The scenarios suggest a usefulness, a reason for selection, for the *various* abilities that go to make up our sophisticated mind-reading. They are like the strands of the rope, so —the author hopes—if one or two get broken, the rope may yet hold.

1.

The Mother Realizes (or Fondly Imagines) that Her Infant Loves Her

This seems a good candidate for an evolutionarily early form of mind-reading: The mother does not have to consciously do anything, let alone anything intel ligent, in order for her infant to gain advantage from her insight into his attachment to her. She only has to attribute an attitude or feeling to her infant (a Dennett desire). Realizing that someone whom you love, loves you back, is a potent reinforcer of your bond with them, making you a more motivated and conscientious mother than one who doesn't realize any such thing. There is a new reward in it. Or at least, that's the way it seems to be with humans, so something of the kind seems plausible for our ancestors.

Of course, this presupposes that the mother feels something warm and fuzzy for her infant to begin with, and the infant feels something warm and fuzzy back, or at least behaves nicely enough toward his mother to fool her into thinking he does. These suppositions seem uncontroversial, it being in the interests of the offspring both that she be attached to him, and that he be nice to her.

This need not have been the first instance ever of a mammal or primate recognizing that a conspecific had feelings about her, or about some third party. I suggest merely that this was *an* example of early and simple mind-reading, and one that happened to lead automatically to a selective advantage.

In support of this being an evolutionarily early example of recognition of a directed feeling (i.e., a mentalistic insight rather than a merely behavioral response), I would mention the following. The intimacy of mother and infant (embracing genetic similarity and close familiarity), together with the intensity of the feelings on both sides, and the fact that these feelings were roughly reciprocal, mirroring each other (thus increasing cue salience), seem to be facilitating factors for a modest cognitive leap.

In addition, the mother has had the experience of being an infant herself, with *feelings of attachment toward her own mother.* Being a mother may trigger memories of having once been in the infant's place, including memories of affect and feelings. This is a process that certainly operates with intensity in human mothers.

There is some evidence that chimpanzees are capable of role reversal; that is, they have the ability to put themselves in anothers place. Povinelli, Nelson, and Boysen (1992) trained four chimpanzees to either: (a) show an ignorant human where some food was hidden, with a share of the booty as a reward; or (b) learn from a human's helpful gestures where the food was. Their roles in the interaction were then swapped over. Three of the four chimpanzee subjects (and, one gathers, all of the humans) were able to cotton on quickly to what they were meant to do in their new role. Rhesus monkeys in similar experiments show no such capacity for role reversal and have to learn their new role as though they had never "played the other part" (Povinelli, Parks, &: Novak, 1992). This helps dispel the possible objection that the chimps were simply transferring some learned behavior from one role to the other. Savage-Rumbaugh (1986) reported that language-trained chimpanzees do not tell others where food is hidden until they themselves have been in the position of ignorance and needing to be informed.

What chimpanzees can do, hominids could probably do even better. The mother-offspring relationship seems a likely one for the recognition of another's feelings *as* feelings, because every mother has once been a dependent offspring herself.

Direct investigation of this scenario across species would be difficult and probably inconclusive, but the "mind-reading mums" hypothesis would have to predict: (a) species that show evidence for mentalistic insight will tend to have more affectionate infants than other species; (b) within such species—including our own species, of course—mothers will be differentially responsive to their infants according to how affectionate those infants are (or are perceived to be); and (c) where the affectional bond is in good shape, the infant will do better than when it's not.

2.

The Mother Enhances the Infant's Learning Through Understanding the Process of "Paying Attention"

Galdikas (1995, pp. 367–369) describes how the orangutans of Tanjung Puting in Borneo eat more than 400 food types, many of which are patchily and unpredictably distributed in both time and space. They seem to have a detailed cognitive map of their environment, and only very occasionally visit some of their food sources. The role of prior observational learning is evident from their reluctance to experiment with anything new, such as bananas, which don't form part of their natural diet. For apes with such complex feeding environments, a single exposure to a new foodstuff sometimes suffices—and sometimes probably has to suffice—for learning. But to learn from observation, you must be paying attention at the critical moment. I suggest that a mother who realizes when her offspring needs to pay attention, checks for it, and if necessary does something to direct it, will be a more effective teacher than one who leaves it to chance.

Undoubtedly, instinct plays a major role both in gaining someone's attention, and in paying it. In modern humans, it is also a fact that these have become at least partly a matter of flexible decision making, and that we understand what paying attention means: We make inferences from a persons direction of gaze, facial expression, body language, etc., both to what they are perceiving, and with how much interest or alertness they are perceiving it. We understand also that attention is necessary for observational learning.

Certainly, the manipulation of others' attention is something that primates other than ourselves are capable of, and that can be used competitively (e.g., Whiten & Byrne, 1988b). There is also some evidence, although it is not conclusive, that chimpanzees have a degree of insight about what visual attention means. They will explore what a human experimenter must have been looking at, when that object is not visible from the chimp's original vantage point, and they will do this in favor of following the human's gaze direction toward an area that was only visible to the chimp, beyond a barrier that obstructs the human's gaze (Povinelli & Eddy, 1996).

My suggestion here is that during hominid evolution, flexibility and judgment in relation to visual attention may have been useful in rearing and teaching infants. As hominid nonsocial intelligence advanced, and with it primitive technology and culture, a point might have been reached when instinct no longer sufficed. Flexibility and judgment would have been called for, because, with a lot going on, a mother would have had to make decisions about competing claims on her infant's attention (and on his other resources). Should she direct his attention to this or to that? Was it more important for him to be shown something new, or to finish some current task or activity uninterrupted? Was a particular lesson as important as a feed or a rest? (Was his lesson as important as *her* feed or rest?) Conscious decisions might be particularly important where a mother had more than one dependent infant, a few years apart in age and therefore at different stages of learning, where the demands on her were heavy and it would be of advantage to them all, including her, if she could tailor her attention-gaining to the offspring's individual needs.

If we assume that such flexibility and judgment became advantageous, then having some insight about what was going on inside one's offspring's head was probably going to win out over behavioral rules. A purely behavioral account of flexible attention-gaining, which excludes any maternal concept or understanding of the offsprings mental states, would be difficult to construct. Such an account would involve the mother having to observe and remember whether the infant had displayed the behavioral manifestations of "paying attention," without her actually understanding what those behavioral manifestations meant; she would have to make judgments about whether something was novel or important for the infant without actually having a concept of "the infant's point of view." I suggest that a mother might more easily make the connections between her infant's gaze directions, facial expressions, and his success at observational learning, if she has the insight that paying attention is simply paying attention. As Savage-Rumbaugh (1994) has pointed out, bipedalism meant that hominid infants were carried instead of clinging ventrally, and the increased face-to-face contact would have meant that the mother could easily follow her infant's gaze direction, and see what he was looking at; this would have facilitated inferences about what he was interested in. I suggest that mentalistic insight about the nature of attention and its relation to learning could quite naturally have followed.

Such insight does not require full second-order intentionality. It requires the attribution of attentional states, and that the mother make *some* sort of link between paying attention and learning by observation; however, this link could be a quite unreflective rule of thumb. She need not attribute rationality, or make behavioral predictions any fancier than that failure to pay attention means that the infant will not learn, whereas if he pays attention, he sometimes will learn.

3.

Accarate Attributions of Knowledge and Ignorance Allow More Efficient Maternal Vigilance for Hazards

Pure instinct can track an infant's age or maturity. Instinctively, you might respond to a 2-year-old in one way and to a 5-year-old rather differently (e.g., how far you let them stray, or how long you let them out of your sight, or how long it takes before it occurs to you, "Things are suspiciously quiet in the playroom! What's going on?").

However, in a complex environment or a large home range, an infant's age may not be a reliable predictor of whether he has previously encountered some infrequent hazard (such as a poisonous berry that might not fruit every year, or a physically rugged part of the territory that is not often visited). The application of simple behavioral rules might offer some advantage over instinct alone: "Heighten your vigilance whenever Junior encounters a known hazard for the first or second time." But it seems to me that the mentalistic insight that Junior is *ignorant* of a hazard will be better still, for two reasons. First, behavioral rules have to be learned, and one of the teachers will have to be (Junior's) cruel experience. The mother who understands about ignorance will thus have more success in the reproductive stakes than the mother who has to learn behavioral rules from disasters and near-disasters. Secondly, the mind-reading mother will be aware that what is important is not how many times Junior has seen something, or how many times she's "told him," but whether he has actually learned— whether he now *knows* that something is hazardous.

This idea seems accessible to empirical investigation, at least in part. The hypothesis predicts that in primate species that show some evidence of having mentalistic insights (however partial), a mother's vigilance will increase whenever her infants are exposed to new hazards that she is familiar with, independent of their age, and even when there is no evidence that she has learned a relevant behavioral rule.

The mechanism depends on the mother having a good enough memory of what the infant has encountered in the past. From what we know about the memory feats of some great apes in relation to feeding, it seems plausible that such an event memory might be present among them, or else might have appeared quite early in the hominid line.

This trick is still a very simple and rudimentary form of mind-reading, falling well short of full SOI; it does not require the mother to attribute desires or rationality to her infant. But she is starting to make predictions based on his state of ignorance and his habitual behaviors.

4.

Mother Learns to Accelerate Operant Conditioning by Making Rewards and Punishments More Effective

Operant conditioning, teaching by means of snarls, cuffs, caresses, and cuddles, has a long evolutionary history and, as any reader of B.F. Skinner will know, it works, sort of, even if you abstain from having a theory of mind. The conditioner's own behavior can be molded by the effectiveness of the rewards and punishments she dishes out; this does not require a theory of mind, either.

But if a mother develops the insight that her rewards and punishments are more or less effective *because* the infant likes or dislikes a particular treatment—that is, they work because of the infant's desires—then she has a new possibility before her. She can now exploit the infant's spontaneously expressed preferences, adding them to her repertoire of rewards and punishments.

It seems worth looking among great apes for instances of mothers taking their cue from their infants, when they dish out rewards and punishments. Of course, this trick is common among human parents; they tailor their manipulations according to what their *kids* see as special treat or dire threat. Children, like other apes, show large individual differences, and these are exploited by the parents. (Its also easy for us to see the pitfalls of this approach. Might they arise because human children are smarter and more resistant to manipulation than little chimps or our hominid ancestors?)

This mechanism is still simple and does not require a full theory of mind. The mother needs only to make a rule-of-thumb link between an infant's desires and the success or otherwise of her attempts at behavior modification. She does not have to attribute rationality—that folks like to maximize their pleasure and avoid pain, and that this is why the system works as it does. But there is now an element of prediction and manipulation in the mother's behavior, and also a need for flexibility over time as the infant's preferences and desires change. The process therefore seems more complex than those previously mentioned, and may serve as a "practice arena" for yet more complex forms of manipulation.

5.

Tough Nuts to Crack: Mother Understands that Something Is Unfamiliar and Difficult for Her Offspring

So she simplifies, repeats, or slows down what she is demonstrating for him. Active teaching of the young is virtually universal among humans. It is not explicable by instinct, or in terms of behavioral rules; it depends on a desire that a child should *learn* something, and on an awareness of his present ignorance or incompetence. Good teaching utilizes an empathic awareness of the infant's point of view, both physical and psychological, and of the limits of his attention span and other cognitive and physical capacities.

Active teaching seems to be rare among apes, and never convincingly observed among monkeys. There are a few anecdotal reports of teaching among great apes (e.g., Fouts, Fouts, & van Cantfort, 1989, with a human-raised chimpanzee, and Patterson & Linden, 1981, with a human-raised gorilla), and observations of very occasional attempts at instruction of offspring among chimpanzee and bonobo mothers (Bard & Vauclair, 1984).

Of particular relevance to the present chapter, Boesch (1991) reports two instances of wild chimpanzee mothers in the Taï National Park (Ivory Coast) actively teaching their offspring the techniques of nut-cracking. This population of chimpanzees is unique in having this technology, which takes the infants about 10 years to properly acquire. In one instance, a mother (Salome) precisely corrected the position of a partly cracked nut on its "anvil." In the other case, the mother (Ricci) very slowly and deliberately demonstrated the grip needed for successful use of a "hammer." In both instances the offspring had been having difficulties before these interventions, but were then able to complete their tasks successfully. These maternal behaviors seem to require a realization of the infant's point of view, and of the way he learns by observation.

The rarity of active teaching among chimpanzees has sometimes been taken to mean either that it either never really occurs (i.e., that alternative explanations are possible for what has been observed), or that it is not terribly significant. However, in addition to these two possible instances of active teaching, Boesch observed many other behaviors of the chimpanzee mothers that were costly to them, but had the effect of stimuating or facilitating their offspring's experience in nut-cracking.

First, mothers with infants were more likely to leave their hammers by the anvils, with the infant, when they went in search of more nuts. (This carried the risk that another chimpanzee would steal her hammer.) This behavior peaked among mothers of 3- and 4-year-old infants, who were the most likely to pick up the hammer and practice or play at nut-cracking. Second, when infants were aged 3 or 4 years or more, the mother would frequently give or lend her infant her own hammer, that was generally a better tool than those which the infants had found for themselves. Third, with infants 4 years and older, the mothers often gave the infants nuts to crack.

In the two dyads where the mothers gave the most nuts, the infants were apparently *more successful cracking the donated nuts* than nuts they had found themselves. This suggests that these mothers (Ella and Aurore) were deliberately choosing "easy" nuts for their offspring. Ella's son took from her 43 of the 47 nuts that he opened during one nut-cracking session. Aurore's daughter was 20% faster when cracking mostly donated nuts, than when she was working at nuts she had found for herself (same hammer, same anvil). Salome and Aurore, and two other mothers as well, were also observed to leave intact nuts on or near anvils with their infants, when they went away to gather more nuts. Chimpanzees without infants were never observed to walk away from intact nuts. Salome (again!), Hera, and Zoe would even position the nut precisely on the anvil, with the hammer laid next to it, so that it was ready for the infant to crack. (Shades of, "Your dinner's in the fridge—all you have to do is microwave it for two minutes.') Every one of the 18 mothers observed engaged in some of these facilitating or stimulating behaviors, with their frequency tracking the age of the infant.

These chimpanzees were thus using a wide variety of strategies to get their infants competent at nut-cracking. As described, six mothers, or about half of the mothers of older infants, showed some degree of inventiveness and flexibility in the task. All of the behaviors—882 instances in just under 70 hours of observation of these dyads—involved an actual or potential cost to the mother. This is perhaps our best evidence that chimpanzee mothers have at least some appreciation of their infants' need to learn, and their difficulties, and are motivated to teach to the best of their (somewhat limited) abilities. Boesch's own view is that

> Conclusively, Taï chimpanzee mothers show a concern about their infants' apprenticeship in tool use and, in various ways, facilitate their attempts, even at some cost to their own performance. They seem to have the ability to compare their offspring's behaviour to their own conception of how it should be performed and anticipate the possible effects of their actions on those of their offspring. (p. 532)

The Taï chimpanzee mothers almost certainly do not have a full theory of mind. Many, most, or all of their behaviors might be explained without invoking any elements of second-order intentionality; the mothers might simply be irritated by incompetence or worn out by dependency, and thus desirous of seeing their infants "do it right" on a purely behavioral level.

I have described these observations in some detail not only because of their intrinsic interest, but because this justly famous paper is often mentioned only in connection with the two possible instances of active teaching. The point I wish to make is that when training or imparting knowledge or techniques, a full theory of mind is not necessary. But without any appreciation at all of an infant's intentionality, a mother's efforts to "get him to do it right" will be limited in their scope: stimulation, facilitation, and physically intervening to abort faulty attempts. We can perhaps imagine an early hominid mother acting in a similarly laborious and inefficient way to impart technology to her offspring. If, however, she has even an embryonic theory of her infant's mind, such as the ability to appreciate the infant's physical perspective on her demonstration of a tricky technique, or the ability to appreciate his perplexity and understand what is causing it, she will have much greater flexibility and scope with active teaching. In addition to checking for attention, she can now (consistently) shift her position so that the infant has the best view, she can demonstrate parts of sequences, repeat, simplify, use slow motion, check for accurate performance, etc.

Boesch's comments about the actual or potential costs to the chimpanzee mother of imparting technology remind us of Trivers' (1974) insight: The mother's and the infant's genetic interests overlap but do not coincide. Because the infant is fully "committed" to his own survival (related 100% to himself), but he shares only 50% of his genes with his mother, certain differences of opinion may be expected. What the mother expends on one infant, she cannot give to other or future offspring. To maximize her own reproductive fitness, she can be expected to try to limit her investment in each infant to a level that is below what the infant strives for. When resources, including maternal time and energy, are scarce, this universal logic to mother-infant transactions may give rise to wasteful conflicts. Such conditions may have obtained during hominid evolution, owing to the new problem of very costly infants (born immature and dependent, and having longer childhoods). More effective and efficient mothering—smarter mothering—may have delivered a win-win solution to this problem, ameliorating the effects of the unavoidable gap between the mother's and infant's genetic interests.

The present chapter suggests that as technology or culture advanced in our hominid ancestry, it created a selection pressure to impart it to offspring. This was most successfully and efficiently achieved through active teaching, which required the mother (or parents) to have some mentalistic awareness of their offspring's perspective, including both physical point of view—what the infant was seeing—and an appreciation of his difficulties and limitations.

In the active teaching of technology and culture, there is also an interestingly intimate connection between what is taught (and learned), and *how* it is learned (and taught); the learner's own psychological processes of gaining understanding and mastery are barely separable from the tool or process that is being learned. "Getting it" involves both a "getting" and an "it," in very close relation to each other. There thus seems to be the possibility of a positive feedback loop between nonsocial and social intelligence: The best learners (of technology) in one generation might become, by and large, better teachers (of young minds) for the next generation, and thus gain a selective advantage both ways.

6.

Re-enter the Intervening Variable Hypothesis

Recall that according to this hypothesis, behavior that looks diverse on the surface may be explained or predicted as arising from an underlying desire or belief. A mother's simple insights about dennettian desires might help her to protect her offspring, even without her having any need to make belief attributions.

Example 1. Individual differences between primates are quite great. Recognizing that a particular infant was especially curious, aggressive, clingy, stupid, oppositional, etc., would aid a mother in predicting his likely responses to different situations, even ones that were novel to them both. Recognizing that "Junior especially likes to investigate anything that's new and different" enables the mother to predict that he, more than her other infants, will approach strangers, put horrible things in his mouth, take things apart, get left behind in interesting places, rush ahead to explore more interesting places, and so on. She is thus better placed to protect her infants from whatever hazards are peculiar to them by reason of their particular dispositions. Earlier, I argued that complex situations, calling for flexibility and judgment, would give mentalistic insights an advantage over behavioral rules. The same can be said for novel situations, because they call for intelligent anticipation and adaptability. The mother-infant relationship abounds in novelty: The mother has responsibility for a unique individual who is constantly developing, and to whom the whole world is new.

Example 2. Mother becomes aware that someone does not like her adolescent son. (Perhaps she even understands why.) Once she has tumbled to this state of affairs, she can be alert to the variety of ways in which the animosity might be expressed, including attacks by the enemy's allies, or revenge attacks against her smaller and weaker offspring after her son has successfully beaten the other guy up.

THE REAL THING: SOPHISTICATED MIND-READING

Adaptive advantage has now been suggested for all the simplest components of our theory of mind: recognizing others' feelings, attitudes, attentional states, desires and interests, rationality, appreciating their points of view (both physical and psychological, and including states of knowledge and ignorance), and appreciating their problems and difficulties. Once these skills or insights had been selected for, it presumably would not have been difficult to combine them to achieve more complex feats of deception, manipulation, and prediction.

Again, the mother's care of her infants may have been an ideal context for the further development of mind-reading. She might use distraction to defuse conflicts, either between herself and an infant, or between her different infants. She might learn to hide her alarm in the presence of danger, if startling her infant would further increase the danger to him. She might make correct attributions of faulty reasoning and resultant faulty beliefs that might endanger her infant. This sounds complicated, but it just means realizing that toadstools are more dangerous if the infant already loves mushrooms, or that swollen creeks are more dangerous if the infant is already confident about crossing shallow streams.[3]

There may of course be the odd opportunity for a mother to deploy second-order intentionality against others, in the service of her offspring. She might hide food, for example, or use nonstereotyped deception or manipulation to draw danger away.

This brings us back to uses for social intelligence that are familiar to us from the Manipulation Hypothesis. I hope to show that deception and manipulation, as played out between unrelated adults, make more sense if they are seen as an exaptation from the care of infants. But before looking in more detail at the implications of the Mind-Reading Mums Hypothesis, I would like to extend the hypothesis just a little further by suggesting that, in addition to mere survival (which is not to be sneezed at), a mind-reading mother might confer two more general sets of advantages on her infant:

Nongenetic or Nurtured

The infant of an expert mind-reader is likely to have had more successful learning experiences, fewer failures and frights. His mother has been better at anticipating his needs, at responding to his anxieties, and grading his challenges. Thus the infant might enjoy nurtured traits of superior knowledge, selfconfidence, health, and nutrition that (especially for a male, perhaps) will enhance his dominance and reproductive success. Having a good mind-reader for a mother might also promote the infant's cognitive and social development through secure attachment. Meins, Fernyhough, Russel, and Clark-Carter (1998) found that infants who were securely attached did better on a false-belief task, and had mothers who were more likely to describe them in mentalistic terms—an intriguing finding that cries out for a similar study on adopted children, to explore whether the infants' facility with theory of mind was more inherited or learned. (See also Chisholm, this volume, for a detailed discussion of motherinfant attachment and the ontological development of theory of mind as an intersubjective process.)

Genetic

The scenarios suggested here for the usefulness of simple, rudimentary mindreading do not require any sophisticated reasoning by the mother. But there are clearly going to be some risks of error; for example, making the wrong calls about the infant's preferences (scenarios 4 and 6); mistakenly attributing knowledge about potential hazards (scenario 3). In these instances there will be selection for mind-reading abilities coupled with the relevant nous, and selection against mind-reading-plus-stupidity. The relevant maternal nous would include her alertness, observational skills, event memory, cognitive mapping of the environment including food sources and efficient routes, "folk physics"—the ability to see things coming in the physical environment and to judge hazards in relation to her infant's physical capacities as well as her own, and so forth. Thus the Mind-Reading Mums Hypothesis suggests mechanisms for how a form of social intelligence may have driven other cognitive achievements: When the context of application is the rearing of a dependent infant in a complex or hazardous environment, mind-reading on its own is not enough.

CONCLUSIONS

There are several differences between mind-reading in the service of one's offspring, and using it directly in pursuit of competitive advantages against other, unrelated adults. These differences tend to support the Mind-Reading Mums Hypothesis, and to lend weight to the idea that other instances of mind-reading may be exaptations.

1. The mother's long and intimate knowledge of her infant, as well as her genetic relatedness to him, should assist her accuracy in reading his facial expressions, vocalizations, and body language, and thereby his emotions, desires, beliefs, and knowledge states. She thus has a head start, if the pun can be forgiven, on someone who is trying to read the minds of unrelated individuals. Furthermore, in most of the preceding scenarios, the infant will have no motivation or means to *hide* his desires or attentional and knowledge states from his mother. This also facilitates her mind-reading tasks, and it contrasts with the

situation that holds in deceptive and manipulative encounters between unrelated conspecifics.

2. As described, even the most simple and rudimentary forms of mindreading imaginable can plausibly confer advantage through kin selection. In contrast, more complex and sophisticated mind-reading seems necessary, if one is to gain decisive advantages over unrelated conspecifics by deception and manipulation.

3. Gains achieved for one's offspring are more likely to stick. The infant will rarely have the motivation to retaliate against his mother's mind-readinginspired manipulations, and will be little able to do so anyway, because his knowledge, physical strength, and cognitive capacities are relatively underdeveloped. It's just not a fair match! At most, he may resist and protest against strictures. Human children start to become aware of others' attention and emotions from a very early age, 12 months or even earlier, but they do not evince accurate awareness of others' thoughts and beliefs, perspective, and knowledge states (i.e., their theory of others' minds does not emerge) until around 4 years of age (Wimmer & Perner, 1983). Even then, it is obvious that a 4-year-old child cannot yet match an adults mentalistic abilities. Chimpanzees also take several years to develop their full quota of social intelligence; e.g, Povinelli, Nelson, and Boysen (1990) and Povinelli, Rulf and Bierschwale (1994) performed the same experiments to explore chimpanzees' understanding of knowledge and beliefs, firstly on adult chimps and then on 4-year-old juveniles, with greatly differing results. (The adults did not convincingly *understand* others' knowledge and ignorance, but they at least learned to learn who was knowledgable. The juveniles could not do this.) By a process of joining the dots (what else can we do?) we can reasonably infer that hominid infants likewise took several years to develop whatever level of mentalistic abilities obtained in their mothers. In other words, by the time they could match their mother's mind-reading abilities, she would have already got them safely through the period of their highest dependency and highest mortality (see the following). In competitive mentalistic interactions between adults, by contrast, as posited by machiavellian intelligence, the playing field is more level: Counterdeception or some other retaliation is likely, so that any advantage achieved may prove to be temporary or trivial.

4. When caring for dependent infants, the situations dealt with will literally be matters of life and death. This really gives natural selection something to work on. Hrdy (1999) notes the unpalatable truth that, "In mammals, natural selection will almost always have its greatest impact on the vulnerable life phases: in utero, during infancy, and just post-weaning" (p. 106).

Studies of primate infant mortality highlight both the vulnerability of infants and the importance of competent mothering. Infant mortality rates among apes and monkeys tend to be high, and more so for firstborn infants with inexperienced mothers, where an actual majority of infants can die (Drickamer, 1974; Harley, 1990; Silk et al, 1981). Among howler monkeys, the mortality of firstborn infants can approach 100% (Glander, 1980). For young chimpanzees, the loss of the mother is catastrophic—invariably fatal to offspring less than 3 years of age, and still a grave risk in chimps aged 5 years (Hrdy, 1999, p. 408).

In human hunter-gatherer societies, also, there are high levels of infant mortality. For example, close to half of !Kung San women, nomadic foragers of the Kalahari Desert, die without leaving surviving offspring (Blurton-Jones, 1986; Draper & Buchannon, 1992; Shostak, 1981). Among the Agta foragers of the Philippines, 30% to 50% of children die before puberty (Hewlett, 1991).

The importance of the combination of maternal experience and resourcing for human infant survival is shown by a general tendency across many societies for mortality rates to be highest in firstborns and in infants born to older mothers (World Health Organization, 1976).

Hrdy (1999) discusses the evolutionary literature's relative neglect of variation in female reproductive success. There has long been an assumption that the reproductive success of females would be fairly uniform, and therefore of little significance to natural selection. Empirial studies are still woefully lacking. However, the studies available, including those noted in the preceding, are beginning to challenge this assumption and to shift attention toward female reproductive patterns and infant survival.

5. The mother-infant context for mind-reading escapes any frequency constraints that might apply to manipulative and deceptive interactions between unrelated individuals. The real, Darwinian competitors for the mother are other mothers—they are competing with one another to leave the largest number of viable offspring. But the mind-reading is taking place between each mother and her own offspring, and it is generally not competitive or deceptive or even manipulative, in the sense of getting someone to act in a way that serves your own interests as *opposed* to theirs. Because the locus of the mind-reading is different from the locus of Darwinian competition, ensuing behaviors that benefit your offspring can be as frequent as you like (and, quite likely, the more you practice the better you get, and the more advantageous it all becomes).

In connection with this, the Mind-Reading Mums Hypothesis certainly is consistent with human parents' all-consuming obsession with the simple—and not terribly interesting—contents of their infants' minds. Western parents at least simply seem unable to stop themselves from hypothesizing about their babies' and toddlers' thoughts, feelings, wants, needs, likes and dislikes, what interests or frightens or disturbs them. Conversations between young mothers about their infants seem to center not on what the infants look like, or weigh, or are actually doing, but rather on what they love and hate, what they are *trying* to do, what they have tantrums about, what they're learning or struggling with, whether they are happy, or bold, or shy. There is a relentlessly mentalistic focus that is reflected also in conversations *with* the infant, which seem to consist in large part of running commentaries on what the infant "thinks" of this or that, whether he likes a particular experience, what he wants.

6. On average, adult human females are slightly more accurate than males in recognizing emotions from facial expression, vocal cues, and body language; the

situation in childhood is less clear-cut, suggesting that social learning plays a part in acquiring these abilities (e.g., see Brody's 1985 review; Brody, 1999). Gender differences in empathic expressions and behaviors emerge early in life, with infant girls showing greater attention to distress (Sigman & Kasari, 1994) and more comforting behaviors (Zahn-Waxler, Radke-Yarrow, Wagner, & Chapman, 1992; Zahn-Waxler, Robinson, & Emde, 1992). These observed gender differences are in line with what differences, if any, the Mind-Reading Mums Hypothesis would predict, and the accurate recognition of emotions seems central to possessing a belief/desire psychology.

I have wondered if my hypothesis might be taken to imply that only mothers are cognitively equipped to cope with infants! This does not follow, because male infants would presumably inherit mentalistic abilities from their mothers, just as females often inherit large hands and unfortunate noses from their fathers. When genes are autosomal, they get shuffled and dealt to males and females alike.

However, my hypothesis does have one worrying, or at least unflattering, implication: To the extent that primate infant survival was favored by a mother's ability to read her infants' minds and predict their behavior, her infants (including our ancestors) would have been selected for their transparency and predictability. Incidentally, it seems to me that these two selection pressures (for mentalistic insights and consequently for transparent infants), acting in concert, may have allowed quite rapid evolution of our theory-of-mind abilities.

7. "May I help you?" Some have thought that the MH offers an all too cynical or pessimistic a view of human social intelligence. The Mind-Reading Mums Hypothesis draws our attention to the commonness of simple and noncompetitive mind-reading in humans, such as teaching, counseling, and volunteering help when someone is distressed or in danger or looks lost. Also, businesses compete with one another, but they do so, in part, by mind-reading what the customer wants and anticipating his needs—looking after him—in a way that parallels a mother looking after her own infants while competing, in Darwinian terms, with other mothers. Thus my hypothesis may help explain the pervasiveness of helpful and cooperative mind-reading in our species, by viewing it as a generalization from a deeply rooted kin-selection mechanism.

8. The topics explored in this chapter might offer a further reason why so much animal signaling looks like honest communication, the arguments of Dawkins and Krebs (1978) notwithstanding. Their distinction—animals manipulate; only humans communicate—has its own value, but somehow some "manipulating" animals evolved into "communicating" humans. I suggest that the manipulations of mothers and dependent infants need to include a large measure of basically honest communication in both directions. Infant signals of fright, pain, confusion, hunger, tiredness have to be readable as such by the mother. Even if infants—heeding Trivers—are inclined to exaggerate their requirements, the species of each signal must still be clearly recognisable. Otherwise exaggeration would defeat its own purpose. Likewise, a maternal 'No!' has to

mean, precisely, “no!” “here!’ mustn’t be interpretable as,“that-a-way,” and, “thus,” shouldn’t be taken to mean, “that-a-way,” either. Etcetera. Because there is a heavy requirement for honest (even if exaggerated) communication during a primate’s long formative years, it may not be so easy to slip into deceptive habits later on—especially if parenting is on one’s future agenda, with its requirements to once again communicate truly in the course of manipulating. One’s “native language” of basic honesty might become to some extent hardwired, providing another constraint on the use of deception.

In fact, could language itself have evolved within the hominid motherinfant relationship? Savage-Rumbaugh (1994) has speculated about this, relating this possibility to face-to-face contact, joint regard and joint visual attention, and the emergence of self-concept; glances and gestures might communicate both the mother’s and the infant’s interests, paving the way to language. My reasons for speculating on this question are slightly different, but complementary. I hope I have made my arguments for the benefits of information exchange in the hominid mother-infant relationship. Language must primarily be for communications *between* individuals, so kin selection presumably must have figured importantly in a knack that is both cooperative and metabolically expensive. First-degree relatives are the obvious candidate dyads for the locus of language evolution, and the mother-infant relationship would seem a prime candidate, for many of the same reasons advanced in this chapter in relation to mind-reading. Humans learn to understand language early, and at roughly the same time that they become toddlers and thus more mobile and troublesome— a circumstance which should have favoured the evolution of a remote control device in mothers, to go along with their mentalistic abilities. And females are quite a lot better at talking than males! QED?

9. Several writers have urged a departure from an all-or-nothing view of theories of mind, in favour of a recognition that there can be degrees of mentalistic insight (e.g., Tomasello & Call, 1997; Whiten, 1996). Indeed, for us to label our own theory of mind as a “full” one is slightly absurd. What we have today works well enough for us, because we *are* us. But what if some highly intelligent aliens happened along, and put us humans through their bewildering hoops? They might conclude that, relative to them, we were pretty clueless; in fact, we didn’t even “get” what it was we were supposed to be “getting.” (And what could stop these arrogant aliens from receiving *their* come-uppance at the tentacles of a still more intelligent species?) In this paper I have suggested ways in which our present mind-reading abilities, such as they are, could have emerged bit by bit and untidily, which is the way of evolution.

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NOTES

1. I abandon political correctness and accuracy in the interests of clarity. Because mother has to be a "she," her infant of either sex is described as a "he."
2. No doubt once a few of the strands of this "rope" became firmly established in an animal's way of thinking, more strands could be added with progressively more ease; that is, the design, metabolic and "breakdown" costs might be lower for the later strands, and therefore the necessary adaptive advantage could also have been less. But it still seems necessary to acount for at least some of the strands.
3. Sue Savage-Rumbaugh (Savage-Rumbaugh & McDonald, 1988) tries frantically to protect the chimpanzee Kanzi: She knows that he knows mushrooms, and loves them; therefore (concludes she, as do all good human "parents" in like circumstances), as he has probably made an unwarranted inductive leap from some mushrooms to all mushrooms, he is likely to be in big danger if he finds a poisonous one. Consequently, she knows to be extra-vigilant—and as it turns out, she has to be extraordinarily vigilant, for Kanzi conceals mushrooms on his person, hides behind trees to eat them, and goes through infuriating "catch-me-if-you-can" routines.

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6

Uncertainty, Contingency, and Attachment

A Life History Theory of Theory of Mind

JAMES S.CHISHOLM

> *In politics, as in family life, be careful not to win too much, because you may win the point and lose the game.*
>
> Shimon Peres[1]

IS THEORY OF MIND AN ADAPTATION?

One hallmark of evolutionary psychology is the proposition that the mind is largely the product of natural selection and so is essentially a set of adaptations for solving the most enduring adaptive problems encountered by our ancestors. Such adaptations are widely referred to as modules, which are conceived as innate, informationally encapsulated, domain-specific brain mechanisms for processing the information logically required to solve particular adaptive problems. Another hallmark of evolutionary psychology, however, is fierce debate about the extent to which modules are informationally encapsulated and domain-specific, how many and how significant they are, how they develop, and what adaptive problems they're supposed to solve. In this chapter I hope to advance our thinking about modularity in general through an evolutionary theoretical critique of one specific candidate module, the theory of mind module.

Theory of mind is "the capacity to attribute mental states to oneself and to others and to interpret behaviour in terms of mental states" (Baron-Cohen, 1995). Based on earlier conceptual work on modularity (e.g., Fodor, 1983), Baron-Cohen, Leslie, and Frith (1985) proposed the existence of a specific theory-of-mind module to explain why autistic children were less able than nonautistic children to pass false-belief tests, which necessitate taking another person's point of view. Theory of mind is supposed to do just that: It is the allegedly innate capacity to understand people's behavior in terms of their beliefs, desires, and intentions. If it were otherwise,"…how on earth [could] young children master such abstract concepts as belief (and false belief) with such ease, and roughly at the same time the world over?" (Baron-Cohen and Swettenham 1996:158)

Existing evolutionary arguments for an innate theory-of-mind module are plausible but indirect. First, both the "evolvability" and "frame" problems make modularity *per se* an attractive idea. The evolvability problem holds that it is easier to evolve by natural selection when better genotypes can be achieved in small, cumulative steps. This is because small, step-by-step improvements are less likely to compromise previous adaptive achievements. Therefore, it is argued, selection will tend to favor complexes—i.e., modules—of functionally related characters that are genetically and developmentally integrated independently of other such modules. Modularizing adaptive functions is believed to foster evolvability: (a) by providing insulation from pleiotropic effects;[2] and (b) providing a sort of ratchet mechanism whereby tried-and-true solutions to the most enduring adaptive problems are compartmentalized against novel selection pressures. Modularity increases evolvability by making it easier for complex organisms with complex ontogenies to make their way from one adaptive peak to another (Bonner, 1988; Wagner, 1995, 1996). The frame problem holds that for behavior to be adaptive it must be controlled, for random movement is no more likely to be adaptive than random mutation. For behavior to be controlled adaptively it must be focused on, directed at, or somehow *about* a moreor-less specific adaptive problem—i.e., it must be framed. Therefore, goes the argument, the mind must be more like a Swiss army knife, with many specialpurpose blades, than a jack-of-all-trades jackknife with just one general-purpose blade. And as far as it goes this must be right: After all, everyone accepts that the mind contains a large number of peripheral modules that are dedicated to processing very narrow kinds of sensory information. On this view theory of mind evolved as a special-purpose "blade" for controlling our behavior by framing it on the beliefs, desires, and intentions that naturally underlie everyone's behavior, most importantly their social exchange behavior (Leslie, 1994; Tooby & Cosmides, 1992).

Second, because of theory of mind's apparent deficit in autism, which has been characterized as a specific "defect of empathy" (Brothers 1989) and may be localized in a specific brain area (Baron-Cohen et al., 1994; Goel, Graffman, Sadato, & Hallett, 1995; Preuss, 1995), and its apparent ease and regularity of development, and because no nonhuman primate has been shown unequivocally to pass a false-belief test, theory of mind is widely conceived as a species specific, automatic assumption that behind every behavior there is an agent who means or intends something by his or her action. This capacity is thought to have evolved uniquely in humans because we are so intensely social and rely so much on social exchange and really do have more-or-less good reasons for *doing* (action or intention to act) what we *believe* will achieve what we *desire.* Consequently, improved knowledge of other people's beliefs, desires, and actions/intentions (especially of cheating)[3] would have fostered increasingly comprehensive and effective social exchange, and therefore been an enduring adaptive problem throughout our evolutionary history (e.g., Cosmides, 1989; Cosmides & Tooby, 1989, 1992, 1994; Byrne & Whiten, 1988; Whiten & Byrne, 1997). This natural, biological association of belief, desire, and action/intention is assumed to have placed such consistent, invariant demands on the neural processes involved in their modeling that over time they became modularized,[4] special-purpose mechanisms that take as their input any two of this natural belief-desire-action/intention triad, from which they infer the remaining unknown and produce as their output a prediction or explanation of other people's belief, desire, or action/intention.

Despite the dearth of direct evidence, therefore, the idea of a theory-ofmind module seems to have evolutionary plausibility. Theory-of-mind skills seem to constitute an epistemically coherent package, to be adaptive, and limited to our species; they seem to distinguish autists from nonautists, and to be neurologically localized; and a theory-of-mind module would seem capable of solving both the evolvability and frame problems. But is theory of mind an adaptation? There are many reasons for questioning theory of mind's status as an adaptation (e.g., Fonagy & Target, 1997; Gerrans, in press; Heyes, 1998), but the tack I want to take here is to suggest that there are good evolutionary theoretical reasons for thinking that theory of mind is not the adaptation it may seem.

Theory of mind's status as an adaptation is critical, for modules are supposed to be adaptations. But as all evolutionary biologists know, "Evolutionary adaptation is a special and onerous concept that should not be used unnecessarily, and an effect should not be called a function unless it is clearly produced by design and not by chance" (Williams, 1966, p. vii). The value of the modularity concept is that it provides another bridge between our only scientific theory of life and the life of the mind; a bridge between the processes of adaptation by natural selection and development that produce the mind's design and the way the mind does the adaptive jobs that it was designed to do. Although theory of mind makes a plausible module, its precise biological nature has never been made clear, nor how it was first exposed to selection and thus capable of evolving. If we don't understand how modules evolve, the adaptive work they're designed to do, or how they do it, then the bridge between evolution and the mind will remain rickety. (On the other hand, we do have some understanding of how theory of mind develops, which, as I show toward the end, sheds light on theory of minds nature and evolutionary history.) If modules are adaptations and theory of mind is a module, then theory of mind is an adaptation and should have the characteristics of an adaptation. As an exercise in bridge-building, therefore, I want to evaluate theory of mind in terms of life history theory, which in my opinion provides our best understanding of the nature of adaptations. Seen this way, I will argue, there are two things that theory of mind is *not:* (a) it is not a module in the usual sense of innate domain-specificity or informational encapsulation but becomes more-or-less modularized during ontogeny through expertise or overlearning (much as we learn to tie our shoes without thinking); and (b) it is not an adaptation but an *exaptation* of two phylogenetically earlier modules, a "contingency detection module" and the mammalian "infant-mother attachment module." Exaptations, remember, are "features that now enhance fitness, but were not built by natural selection for their current role" (Gould & Vrba, 1982, p. 4).

ADAPTATION

If modules are adaptations (or exaptations) then the modularity concept should shed some of its slipperiness by accommodating to the nature of adaptations (and exaptations) as conceived by life history theory. Life history theory is developmental evolutionary ecology (Chisholm, 1999). Evolutionary ecology is the study of organism-environment interactions in an evolutionary context (e.g., Pianka, 1988); life history theory is the study of organism-environment interactions *throughout development* in an evolutionary context. All evolutionary theories hold that adaptations are *for* solving adaptive problems posed by the environment, but life history theory focuses at the same time on the *history* of adaptations—on the often subtle and nonobvious nature of adaptive problems and solutions as they ramify through and between individual life cycles and lineage evolution, and on the ways in which life cycles themselves are for solving adaptive problems (e.g., Stearns, 1992, 2000). In this approach, if theory of mind is an adaptation/exaptation, then it too should be *for* something and *historical.* Ostensibly, theory of minds output is for controlling an agent's behavior as a function of its inputs, predictions, and explanations of other agents' behavior in terms of their beliefs, desires, and intentions. In a word, theory of mind is for solving the "social unpredictability" problem. But is predicting behavior really this unsubtle and obvious? Is everyone's behaviour equally important? What is the evolutionary ecology of unpredictable social relations? What precisely is at risk in social uncertainty? Precisely how is this uncertainty experienced developmentally and represented phenotypically? Exploring the ontogenetic and phylogenetic histories of theory of minds function(s) simultaneously may help us see it in a new light.

The Adaptive Problem: Predicting the Future

Current evolutionary ecological models suggest that environmental risk and uncertainty are among the most pervasive and enduring adaptive problems fac ing all organisms (Seger & Brockmann, 1987; Stearns, 1992, 2000; Charnov, 1993). Complex adaptive systems theory helps to show why.

Complex adaptive systems theory grew out of the realization that there are more kinds of adaptation than adaptation by natural selection. It is now accepted that the process of adaptation has certain formal properties that can be identified in complex adaptive systems generally, be they evolving species, the immune system, ecological and economic systems, developing embryos and brains, or virtual species in computer simulations of evolution. Foremost among these formal properties is the capacity to form what John Holland called "internal models" for predicting the future. According to Holland, the father of complex adaptive systems theory, internal models are the "fundamental attribute" of all complex adaptive systems because they allow the system

> to look ahead to the future consequences of current actions, without actually committing itself to those actions. In particular, the system can avoid acts that would set it irretrievably down some road to future disaster ("stepping off a cliff"). Less dramatically, but equally important, the [internal] model enables the agent [the system] to make current "stage-setting" moves that set up later moves that are obviously advantageous. The very essence of attaining a competitive advantage, whether it be in chess or economics, is the discovery and execution of stage-setting moves (Holland, 1992, p. 25).

In sum, complex adaptive systems have the remarkable capability of continuing, descending through time, with modification, indefinitely. This entails solving two extremely basic and enduring adaptive problems. Continuing requires, first, not stepping off a fitness cliff. In the terms of evolutionary ecology this means avoiding death, or more precisely, failure to reproduce. Second, continuing requires setting the stage for future good fitness moves, which means investing in the future, preparing for the future, foregoing short-term benefits in return for delayed, but greater benefits in the future. Setting the stage for future good fitness moves is, in Holland's words, "less dramatic" than avoiding fitness cliffs because the cost of stepping off a fitness cliff is immediate, severe, and certain, whereas the benefit from setting the stage for the future is delayed, and to that extent devalued, relative to the present, because the future is always uncertain because no environment is ever absolutely without risk. This is surely why the probability of lineage extinction is more sensitive to having few offspring than it is to having many—that is, why *reducing* number of offspring by a certain amount *increases* the probability of lineage extinction more than vice versa, more than *increasing* number of offspring by the same amount *reduces* the risk of lineage extinction (Ellison, 1994; Keyfitz, 1977). In other words, continuing in evolution puts a premium on downside risk protection. This is why risk-aversion itself is another good candidate for modularity. As Robert Frank put it, risk-aversion is "one of the most robust regularities in experimental psychology [and] is apparently part of the hard wiring of most animal nervous systems" (1992, p. 279).

Henry Plotkin (1994) refers to this most generic of adaptive problems as the "uncertain futures problem." Solving this problem requires predicting the future; it requires adaptations, first for looking ahead for cliffs (risk and uncertainty) and then, if none are found, looking ahead for stage-setting moves. Given our species' extreme sociality where we stand between these cliffs and stagesetting moves is largely determined by other members of our species, especially when we're young, when, as mammals and highly altricial, slow-developing primates, we require so much parental investment. And as I will explain more fully later, we perceive where we stand in relation to other people largely in emotional terms, with social cliffs experienced as fear or insecurity and social stagesetting moves represented subjectively as feelings of hope or security. From the perspective of life history theory, therefore, I believe that we can say more about theory of mind than simply that it is for predicting and explaining behavior. This is not so much wrong as unsubtle. I will argue that it makes more evolutionary sense and is more productive to view theory of mind as a developmental exaptation of a "contingency detection module" and the basic mammalian "infant-mother attachment module" that was originally (both phylogenetically and ontogenetically) for predicting and explaining *mother's* behavior. To set the stage for this argument it helps to see how animals in general have solved the problem of predicting and explaining behavior.

The Adaptive Solution: Detecting Contingency

If the "fundamental attribute" of complex adaptive systems is predicting the future then, as complex adaptive systems *par excellence,* animals should have this capability. In early, relatively simple animals this capability is mostly "implicit." Holland notes

> that we rarely think of anticipation, or prediction, as a characteristic of organisms in general, though we readily ascribe it to humans. Still, a bacterium moves in the direction of a chemical gradient, implicitly predicting that food lies in that direction. The butterfly that mimics the foul-tasting Monarch butterfly survives because it implicitly forecasts that a certain wing pattern discourages predators. (1992, pp. 24–25)

In later animals, like mammals and primates, with big brains and complex social organizations, this capability is more sophisticated, reaching its apparent zenith in human theory-of-mind skills, which are arguably the foundation for culture (e.g., Tomasello, 1999) and thus science, our best, most explicit predictive capability. Thompson and Derr (2000) observe, however, that even with nary a hint of theory of mind many animals routinely predict the social future by inferring what other animals "want." They suggest that animals have this capability because behavior in general is governed by control systems and that natural selection, therefore, is expected to favor cognitive mechanisms for detecting the operation of behavioral control systems.

A control system is a system for controlling the output of another system through feedback processes so as to maintain that output within certain reference values. Thompson and Derr use the familiar example of a thermostat controlling the output of a heating system so that the temperature of the room being heated stays within a few degrees of its preset temperature, or set-goal. This set-goal is an instruction that has been programmed into the thermostat. It is information about what the temperature of the room should be. To do its work the thermostat has to carry out three operations. First, it must assess the room's actual temperature. It does this by assessing the curvature of a bimetal-lic strip, which is correlated with or cued to room temperature. The position or shape of the bimetallic strip is thus the thermostat's "cue variable". Second, it must compare the room's actual temperature with its set-goal, the information about what the room's temperature should be that was programmed into it. Third, it must use the output of this comparison to approach its set-goal, signaling the furnace to generate heat if the room temperature is less than its setgoal, or to cease generating heat if the room temperature is greater than its set-goal. In effect, a set-goal is an intention, an "image of the future that motivates action." By observing the relationship between the thermostat's environment (room temperature) and the furnace's behavior (on or off) we could determine the thermostat's set-goal. We could then predict or explain the thermostat's behavior in terms of this set-goal. We might even say, figuratively, that the thermostat "wanted" the room to be a certain temperature.

Control systems predict the future by "triangulating" on it with two kinds of information: old and new. Although "an image of the future that motivates action" is about or "points at" the future, it is in fact a product of the system's *past* and its *present;* it is the output of a mechanism for combining old information that was programmed into the system when it was designed (or last redesigned, e.g., by re-setting the thermostat) and new information about the system's current environment. In comparing where it should be (old information about design history) with where it is (new information about its present circumstances) the control system creates an image of the future by drawing a line, so to speak, from the past to the present and then projecting it into the future as a plan of action. Any discrepancy between the old and new information constitutes an image of the future that moves the system to action (e.g., turning the furnace on or off).

Control systems are ubiquitous in biology. In humans, for example, they maintain body temperature at 37°C, blood sugar at 90 mg of glucose per 100 mL of blood, and blood pressure at around 120/70 mm Hg. These set-goals are old information from our evolutionary past about what our temperatures, blood sugar levels, and blood pressures should be. When this old information is juxtaposed to new information about current body temperature, blood sugar level, and blood pressure any output of discrepancy constitutes an image of the future that triggers biochemical feedback processes that move the relevant systems toward their set goals. It is important to emphasize that neither the old nor the new information is sufficient; adaptive homeostasis—control—requires both.

Biological control systems control behavior as well as biochemistry. For present purposes the most relevant human example is the attachment system, which Bowlby (1969) saw as an evolved mechanism for maintaining optimal infant-mother proximity. The attachment system has two set-goals; it embodies two kinds of old information from our evolutionary past: security (confidence, hope; good) and insecurity (anxiety, fear; bad). When the infant's subjective experience of his current environment (the cue variable) is one of security then the attachment system matches this new information with old, innate representations of security and outputs an image of the future that motivates the child to explore its world; in the language of classical ethology this image "releases" exploratory behavior. Exploration entails the child's separation from his "secure base." Sooner or later, therefore, as a function of time or distance from mother, or encounter with some frightening stimulus, the child's subjective experience will be one of fear. When this happens the attachment system matches this new information with old information about fear and anxiety and outputs an image of the future that motivates the child to seek proximity to mother (his "safe haven"); it "releases" proximity-seeking behavior.[5] As a function of re-established proximity to mother and her positive, sensitive, responsive acceptance of him, the infant's anxiety is likely to be only transient. With security renewed he is motivated again to engage the world, so beginning another iteration of the attachment cycle.[6] By observing the relationship between child and mother (how long, how far, and by what events they are separated and reunited) and the child's behavior (e.g., play, explore versus approach mother), we could determine the child's set-goal. We could then predict or explain the child's behavior in terms of this set-goal. We could then say, quite literally, that the child wanted to be a certain distance from his mother, or to have a certain relationship with her.

Thompson and Derr's main point, however, is that just as we could say— but obviously don't have to—that the thermostat "wanted" the room to be a certain temperature, neither do we have to predict or explain the child's behavior in terms of what he wants. Just as predicting or explaining the output of a thermostat does not require the attribution of mental states to a wiring system, neither does predicting or explaining the output of *any* control system. This is because it is possible to predict the future by detecting contingencies in the relationship between the output of a control system (furnace off or on; approach or leave mother) and its environment (the cue variable: shape of bimetallic strip; subjective experience of distance from or relationship with mother). Thompson and Derr's point is that theory of mind is not necessary for predicting or explaining the output of control systems, even those controlling behavior; it is sufficient to note changes in behavior that are contingent on changes in environment.

György Gergely, Gergely Csibra, John Watson and their colleagues (Gergely et al, 1995; Gergely & Watson, 1996, 1999; Gergely & Csibra, 1997; Csibra, Gergely, Biró Koós, & Brodebank, 1999; Watson, 2001) have reached precisely the same conclusion, inductively and independently, without Thompson and Derr's insight about the "intentionality" of control systems. They argue that by at least 9 months infants[7] can adopt the "teleological stance," which is to explain behavior with reference to future goal states—i.e., to predict the future. In a series of experiments using an habituation paradigm, they found that 9-monthold infants demonstrated surprise and heightened attention when computergenerated geometric shapes seemed to act "irrationally" by not choosing the optimal ("rational") act with regard to achieving an apparent goal. Moreover, these attributions of intentionality to geometric shapes were not dependent on their apparent self-propulsion or categorization as "agents." They concluded that

> …teleological interpretation is driven by stimulus conditions that can be derived directly from the core principle of reasoning of the domain of naive psychology. The principle of rational action requires that behaviours directed to the same goal be adjusted in relation to the relevant aspects of the environment in which they occur. Consequently, the perception of behavioural adjustment that is a function of situational constraints may serve as the triggering condition for analysing the behaviour as goal-directed. (Csibra et al., 1999, p. 264)

To perceive that an event or action is "adjusted in relation" to "situational constraints" is to note that it is contingent on its environment. Gergely and Watson (1996,1999; Watson 2001)[8] make a convincing case that this capacity to infer goal-direction (predict the future) from the perception of contingency relations represents the action of an innate "contingency detection module" (henceforth CDM) (Gergely & Watson, 1999, p. 102). On this view the CDM is essentially an adaptation for predicting the future, and works by detecting the objective conditional probabilities connecting events or actions in time, space, or sensory intensity. When the events or actions involve social behavior the CDM is for predicting the social future and works by solving the logical problems of social contingency analysis. This entails discovering the cue variable on which a certain behavior is contingent. When we observe that this behavior has a high conditional probability of preceding or following particular changes in the cue variable we may feel justified in saying that we can predict or explain the agent's (or control system's) "intention" vis-à-vis the cue variable.

Recapping briefly, everyone seems to accept that ToM is for controlling an agents behavior by solving problems of social contingency analysis—i.e., predicting and explaining other agents' behavior in terms of their beliefs, desires, and intentions. But isn't this just what Gergely and Watson's CDM is supposed to do? So it would seem, which is another reason for questioning whether theory of mind is a module in the sense of innate domain-specificity or informational encapsulation. Following Thompson and Derr and Csibra, Gergely and Watson, I propose instead that theory of mind originates partly in the CDM, which is innate, and then *becomes* modularized ontogenetically, during the attachment process, through expertise or overlearning.

From the perspective of life history theory, this sort of combined phylogenetic/ontogenetic analysis is indispensable. Because selection operates on phenotypes, not genotypes, all adaptations/exaptations must be exposed to selection—they must develop—before they can evolve. Adaptations/exaptations are thus historical phenomena, and to understand them we have to describe two kinds of histories: the ontogenetic and phylogenetic processes that produce organisms (Bateson, 1976, 1982; Chisholm, 1999; Griffiths & Gray, 1994; Lickliter & Berry, 1990; Oyama, 1985, 2000; Stearns, 1982, 1992, 2000; Sterelny & Griffiths, 1999). David Marr once wrote that "an algorithm is likely to be understood more readily by understanding the nature of the problem being solved than by examining the mechanism (and the hardware) in which it is embodied" (1982:27). But if "algorithms" (modules) are adaptations/exaptations then they must have phylogenetic and ontogenetic histories. To understand modules surely it would help to understand the historical processes that produce them. But to study these histories you must have a *mechanism* to study. Understanding the nature of an adaptive problem is critical, of course, but no more critical than understanding the origin and potentially changing nature of the biological mechanism that does the adaptive work. By themselves, statements about "a module for X"are no more illuminating than statements about "a *gene* for X"; both modules and genes are nothing without mechanisms of adaptive action. If theory of mind is a module it had to be exposed to selection before it could evolve: "A *cognitive module is an evolved mechanism with a distinct phylogenetic history.*" (Sperber, 1994, p. 42; original emphasis)

To my knowledge, however, there are no convincing accounts of how theory of mind might have evolved. The consensus seems to be that because of its apparent absence in great apes it more-or-less just gradually appeared in *H. sapiens*. But we *do* have a place to start: If theory of mind is a module its phylogenetic precursor had to *develop* before theory of mind could evolve, for, like any *bricoleur,* evolution works only with what it already has: phenotypes. Moreover, I believe that principled ideas about what theory of mind is for and how it works provide a basis for informed speculation about its phylogenetic origin. I maintain further that empirical evidence about theory of mind's ontogenetic history is consistent with my speculations about its phylogenetic history and my overall conclusion that theory of mind is not an innate, domain-specific module. I review this evidence after exploring theory of minds phylogeny: How was theory of mind exposed to selection?

A PHYLOGENY OF THEORY OF MIND

Everyone agrees that solutions to the most enduring adaptive problems are the ones most likely to have been modularized during evolution. Take object per ception: Everyone seems happy with the idea of an "object perception module," for light has been bouncing off objects into vertebrate eyes for 500 million years (Stahl 1985; Young 1950), giving selection plenty of time to organize edgeand surface-detecting neurons for computing objects. On the other hand, no great ape seems to have theory of mind. The hominids split off from the apes about 5 million years ago and anatomically modern humans appeared at the earliest only 200,000 years ago. This means that selection would have had only l/ 100th to l/2,500th as long to modularize theory of mind as it had to modularize object perception.[9] But even if this was enough time, the more important question is, if theory of mind was modularized, how was it exposed to selection in the first place? From what ontogenetic source did its phylogenetic history spring?

There are three reasons why Gergely and Watson's CDM seems a good candidate for theory of mind's phylogenetic origin. First, the first principles of life history theory suggest that theory of mind is for predicting the social future and works by solving problems of social contingency analysis. This is what CDM is for and does. Second, the first principles of life history theory hold that theory of mind has a phylogenetic history and that knowing this history will help us understand theory of mind and its ontogenetic history. CDM must be phylogenetically much older and more widespread than ToM; surely one of the reasons that vertebrates evolved eyes 500 million years ago was to detect objects that *moved.* And surely an enduring reason for detecting movement was to reduce uncertainty through contingency analysis by detecting the "intentionality" of behavioral (movement) control systems. It seems wrong to suppose both CDM and theory of mind when CDM alone will do, especially because it is surely much older. Third, the first principles of life history theory suggest that theory of mind also has an ontogenetic history and that knowing this history will help us understand theory of mind and its phylogenetic history. There is good evidence that theory of mind (and therefore CDM too, to my way of thinking) is an intimate part of the of the ontogeny of infant-mother attachment. This suggests that one (the original?) phylogenetic context in which uncertain social relations exerted selection for increased capacity for analysing social contingency in hominids may have been the ontogenetic context of parental investment and parentoffspring conflict—*because this is what attachment is all about.* Attachment is not for achieving love, happiness, or even security but: (a) for offspring to identify likely *sources* of parental investment (energy, nutrients, protection, time, information, etc.), and then to *elicit* these resources; and (b) for parents to identify the best *recipients* of their limited resources and then to invest them *wisely.*

What makes things interesting is that parents and offspring are expected to differ about what constitutes "wise" investment from parents. Trivers' (1974) theory of parent-offspring conflict holds that because mother and offspring are not genetically identical they have different fitness interests and so are expected to disagree about how much or how long mother should invest in each offspring.[10] The issue, in a nutshell, is that although the mother has a 50% genetic interest in all of her offspring, each offspring has a 100% genetic interest in itself but only a 50% genetic interest in existing or future siblings (even less if they have different fathers). All else being equal, offspring are therefore expected to "want" more investment from mother than she is "willing" to provide.[11]

Accordingly, social contingency analysis—CDM, for example—is exactly what we would expect to find in parent-offspring relations: as Trivers put it,

> socialization is a process by which parents attempt to mold each offspring… while each offspring [is expected] to resist…and to attempt to mold the behavior of its parents. (1974, p. 260)

The attachment theorist, Peter Marris, sees the same Machiavellian potential:

> the balance between assertion and compliance in a child's experience of attachment represents a fundamental learning of the nature of order and security…. Learning to manage the attachment relationship therefore is learning to understand order and control (Marris, 1991, p. 79)

Because parent-offspring conflict flows inexorably from the logic of sexual reproduction and parental investment it is inescapable and would constitute an enduring evolutionary "arms race" between mothers and offspring. Because survival, growth, and development, and adult reproduction in mammals are so thoroughly dependent on parental investment (e.g., Gubernick, 1981; Klopfer, 1981; Polan & Hofer, 1999), selection for skilled social contingency analysis would have been unremitting in some mammalian orders (see the following) since the origin of mammals 180 to 230 million years ago, giving plenty of time for CDM to be modularized (if it hadn't already been modularized some time during the 400+ million years since the origin of the vertebrate eye). What's more, this parent-offspring arms race would be expected to escalate if offspring evolved to require more parental investment. This, I propose, is just what happened in hominid evolution. Before getting to hominids, however, I need to set the stage with a quick look at the evolutionary ecology of attachment in mammals.

In very simple terms, "attachment" refers to the emotional attraction that mothers and offspring hold for each other. As a result of this attraction offspring are moved to seek out mothers and attempt to be close and obtain body contact, warmth, food, etc. and mothers are moved to nurture offspring.[12] The evolutionary roots of infant attachment are thought to lie some 350 million years ago in the neurobiological mechanisms mediating place attachment (recognizing and wanting to return to home) and thermoregulation in reptiles; maternal attachment (nurturance), in turn, seems to emerge ontogenetically (with environmental "scaffolding") from the evolved brain systems promoting infant attachment (Panksepp, 1998). Reptilian protoattachment subsequently took two major evolutionary directions, toward imprinting in precocial ("fast-developing") birds and attachment in mammals. Mammalian attachment patterns are vari able, of course, and in some, like ungulates, who are mobile virtually at birth, they resemble imprinting. Using comparative data, David Gubernick (1981) showed that attachment of broadly the human sort (i.e., gradual development over months and use of mother as secure base and safe haven) is concentrated in mammals who had to solve two adaptive problems simultaneously: (a) providing adequate investment to slow-developing infants with limited mobility; and (b) doing so in the complex, intense, shifting, and sometimes dangerous social contexts like those of many primate societies.[13] (Baboons and our closest great ape relatives, the chimpanzee and bonobo, are good examples.) The problem with slow development and prolonged limited mobility[14] is that infants cannot readily follow their mothers or provide for themselves, so must have alternative means of acquiring resources. The problem with complex, intense, shifting, and sometimes dangerous social environments is that they make it more important to identify individual adults and juveniles and the social-emotional (intentional) relations among them (e.g., Byrne & Whiten, 1988; Whiten & Byrne, 1997). Gubernick thus took issue with Bowlby's fairly narrow view that attachment's original adaptive function was predator protection, proposing instead that it made more adaptive sense as a mechanism for insuring that parental investment is directed to mothers' *own* young, not those of some other female.[15] On this view, the original adaptive function of humanlike attachment was cooperative: (a) as a "resource elicitation mechanism" for infants; and (b) a "maternity certainty mechanism" for mothers, predisposing them to invest their limited resources in their own offspring.

However, the relentless logic of parent-offspring conflict means that cooperation has its limits and that mothers should always invest wisely, for mere genetic relatedness is not a sufficiently wise investment strategy. Instead, mothers should invest in each infant according to his or her capacity to provide grandchildren (or great-grandchildren, great-great-grandchildren, *ad infinitum;*e.g., Chisholm, 1999; Kaplan, 1994). The infants capacity to provide descendants (technically, reproductive value) depends on a huge range of factors, of course, both intrinsic and extrinsic, material and mental, but the final common pathway of many or most is the infant's capacity to elicit parental investment. This capacity is affected by infant health, nutrition, temperament, social-emotional environment, and much more, but at the end of the path what matters is *politics:* the infants capacity to *have an effect* on maternal behavior. Keep in mind Marris (1991): "managing the attachment relationship" requires the infant to "understand order and control," and Trivers (1974): "offspring [are expected] to attempt to mold the behavior of parents." The more that infants "understand order and control" the better they can "mold" mother's behavior and more skilfully "manage the attachment relationship" in their favor, eliciting more from mother than she might feel is "wise." This is the nub of the evolutionary arms race between mothers and offspring. Reducing uncertainty in the flow of material and social-emotional resources from mother by predicting how she will behave given certain contingencies would be a logical way for infants to manage this arms race (their attachment relations). Given the particular contingencies detected through his cue variable (subjective feelings of security or insecurity) the infant would then be better able: (a) to avoid fitness cliffs, whether intended (mother's *unwillingness* to invest) or unintended (her *inability* to invest); and (b) to set the stage for future good fitness moves (grow, play, explore, learn, etc. as a function of maternal willingness *and* ability to invest) (Chisholm, 1996, 1999).

To recap, if hominid infants evolved to require more parental investment than was "wise" for mothers, parent-offspring conflict would be expected to escalate. Given our mammalian phylogeny the main arena for this conflict would be the attachment relationship, with infants using it in an attempt to elicit more of mothers' resources and mothers using it to resist, attempting to allocate their limited resources ever more "wisely" with regard to both genetic relatedness and offspring reproductive value. But this begs the question, *did* hominid infants evolve to require more parental investment? I think they did, for two reasons: our evolutionary history of: (a) tension between sexual selection and K-selection; and (b) the so-called "obstetric dilemma." As a consequence, I suggest, an important force in hominid evolution was increasing parent-offspring conflict, and that improved social contingency detection—and eventually theory of mind —were results.

Humans are descended from hominids (5 million years ago), primates (65 million years), and mammals generally (180 y 230 million years). There is variation, to be sure, but as a class mammals are characterized by considerable sex differences in parental investment, with females having been selected primarily to bear and rear offspring and males to gain sexual access to females. All mammalian females possess anatomical and physiological adaptations for ovulation, gestation, parturition, and lactation, whereas mammalian males tend to possess adaptations for coercing other males (and females), locating females, attracting females, etc. (Clutton-Brock, 1991; Trivers, 1972; Williams, 1975). This characteristic pattern of sex differences is also widespread in primates (Hrdy & Whitten, 1987; Wrangham, 1980), including our closest great ape relatives, the chimpanzee, and, to a lesser extent, the bonobo (Wrangham, 1993). I also argue that it is more-or-less apparent in humans as well, albeit more variably (e.g., Hrdy, 2000; Lancaster, 1997; Smuts, 1992, 1995). But it is the evolutionary *effect* of these 100+ million years of sex differences in mammalian parental investment that I want to emphasize here, for it is more germane to my argument about the role of parent-offspring conflict in hominid evolution than the history *per se.* And this effect is an increase in the strength of sexual selection during hominid evolution. The reason for this increase follows from the logic of parental investment and sexual selection theories, which Trivers realized were two sides of the same coin.

> *What governs the operation of sexual selection is the relative parental investment of the sexes in their offspring.* Competition for mates usually characterizes males because males usually invest almost nothing in their offspring. Where male parental investment per offspring is comparable to female investment one would expect male and female reproductive success to vary in similar ways and for female choice to be no more discriminating

> than male choice…. Where male parental investment strongly exceeds that of the female…one would expect females to compete among themselves for males and for males to be selective about whom they accept as a mate. (1972, p. 141; original emphasis)

In effect, then, sex differences *are* sex differences in parental investment. The strength of sexual selection will therefore increase as sex differences in parental investment increase. Because of our particular mammalian and primate evolutionary histories of K-selection (see following) and greater parental investment by females, any increase in the capacity of hominid infants and juveniles to elicit and/or benefit from parental investment would tend to exert greater selection pressure on females than males to provide it. In turn, this increased sex difference in parental investment would tend to drive the sexes further apart, causing increased sexual selection for sex differences in reproductive strategies, with females selected to allocate even more of their limited resources to bearing and rearing offspring and males even more to acquiring sexual access to females. As females invested more time and energy in bearing and nurturing increasingly demanding and needy offspring, they would become proportionately more valuable as "reproductive resources" to males, thereby increasing the marginal rate of return to males of coercing other males and females to their will in order to acquire these "resources"—for example, through an increased appetite for risk-taking, impulsiveness, dominance, showing off, violence, male-male competition, and so on (e.g., Chisholm, 1999; Hawkes & Bliege Bird, 2002; Hrdy, 2000; Wilson & Daly, 1985; Wrangham & Peterson, 1996). Everything else being equal, the more that males benefited from coercing (or persuading) others into allowing them sexual access to females, the more that females would have to invest in reproduction—quantity and/or quality—just to maintain reproductive output at replacement level.

To this anthropoid-hominid tension between male and female reproductive strategies (sexual selection) now add the tension of our evolutionary history of *K*-selection. *K*-selection is a form or mode of natural selection. Natural selec-tion works in its *K* mode when environments are predictable, mortality rates are low or steady, and organisms tend to die or fail to reproduce not so much because they are unlucky, in the "wrong place at the wrong time," but because they are unfit. *K*-selection thus tends to favor individuals who produce "fewer and better," more competitive offspring; *K*-selected species produce small numbers of large, slow-developing, long-lived, big-brained, highly social offspring who require a great deal of parental care. Mammals tend to be more K-selected than other classes, primates more than other orders, and humans are among the most K-selected of all species (e.g., Charnov & Berrigan, 1993; Pianka, 1988; Stearns, 1992, 2000). This means that just as sexual selection was intensifying sex differences in hominid evolution, committing females to even more parental investment, K-selection was producing infants who developed even slower and required even longer and more intensive parental care. The result would be positive feedback: The more that hominid infants required parental investment, the more females would be selected to provide it, intensifying sexual selection and sex differences even more, increasing the pressure on females to sacrifice offspring quantity for quality, requiring them to invest even more in order to maintain replacement-rate reproductive rates (see also Lovejoy, 1981).

But there's still more. Now add the "obstetric dilemma" (Washburn, 1960), which resulted in even more helpless infants who took even longer to mature, thus requiring even more and longer parental (maternal) investment. The obstetric dilemma was the adaptive problem of reconciling two opposing selection pressures, one that favored narrowing the pelvis as an adaptation for bipedal locomotion, and the other (including K-selection) that favored intelligence and thus big brains and thus newborns with big heads.[16] The dilemma was how to accommodate ever-larger newborn crania through narrower birth canals. The solution was selection for "shorter" gestation to enable delivery before the child's head grew too large.[17] The effect of "premature" birth, however, would be to intensify the already vicious cycle of helpless newborns requiring even more intensive care from their already overworked mothers, causing even greater sexual selection and increasing the pressure on mothers to sacrifice offspring quantity for quality, requiring them to invest even more in order to maintain replacement-rate reproductive rates. What's a mother to do when she can't do any more?

One possibility is cooperative breeding: the capability of others ("helpers at the nest") to assist a mother in rearing her offspring. Cooperative breeding occurs in birds and mammals when biological and socioecological conditions conspire to raise the cost of successfully rearing offspring beyond what mothers can provide by themselves (birds: Cockburn, 1998; primates: Hrdy, 2000, 2001; mammals: Solomon & French, 1997). More technically, cooperative breeding is for solving the adaptive problem of reducing risk and uncertainty in the flow of energy, nutrients, security, information, etc. to offspring below the level that mother alone can reach. Cooperative breeding works by spreading the cost of parenting more widely among those with a genetic interest in the particular offspring. Those with the most obvious genetic interest in an infant are: (a) its mother and her kin; and (b) its father and his kin. (His and their interest, however, must be discounted by the degree to which his paternity is uncertain.) But what about baby herself? The theory of parent-offspring conflict maintains that because biological parents have a 50% genetic interest in offspring, whereas offspring have a 100% genetic interest in themselves (i.e., they carry copies of *both* parents' genes), offspring are expected to "want" more investment from parents than parents are "willing" to provide. The one with the greatest genetic interest in an infant is thus the infant herself. It follows that we would expect natural selection to favor mechanisms for spreading the cost of parenting not only among parents and their kin, but *offspring as well.*

Mechanisms for spreading the cost of parenting from mother to father almost certainly include a range of adaptations for promoting male-female sexual-emotional bonds. Consider body shape. Compared to the other great apes, humans (and bonobos) show little sexual dimorphism in body size. This does not mean, however, that the force of sexual selection decreased during human evolution, because humans (unlike bonobos) are quite dramatically sexually dimorphic in body shape. For starters, human females are the only primate female displaying permanent breasts. Nonhuman primate females lactate just fine without permanent breasts, nor is breast size related to lactational capacity in humans. Permanent breasts may thus have only a sexual signaling function. Consider also loss of estrus, disproportionately large penis, sex during pregnancy, and very high rate of nonconceptive or "communication sex" in humans compared to nonhuman primates (e.g., Hrdy, 2000; Short, 1979; Smuts, 1985; Wrangham, 1993). Given that sucking, mouthing, stroking, hugging, cuddling, and orgasm release oxytocin, which promotes relaxation and feelings of interpersonal warmth and affiliation in both sexes (Carter, 1998; Insel, 2000; Uvnäs-Moberg, 1998), the evolution of human sexuality may be seen as fostering the capability for adult male-female romantic attachment.[18] To the extent that such attachments predisposed males to invest in their own offspring (even if only indirectly, by providing resources to their mothers, thus freeing them for more or longer direct investment) increased female choice of males with a greater capability for such attachments would constitutes increased sexual selection to spread the cost of parenting.

Prolonged postmenopausal lifespan may be another mechanism for spreading the cost of parenting. Menopause occurs in other primates, but only in humans do females live for many years afterward. This unique life history trait may have evolved as a mechanism for predisposing older women to invest in grandchildren rather than having additional children of their own (e.g., Hawkes, O'Connell, Blurton, Jones, Alverez, & Charnov, 1998). Mechanisms for spreading the cost of parenting from mother and father to each other and their kin and allies surely also includes language, empathy, and other foundations for social exchange, politics, and economics (and cheating, of course).

But how could the cost of parenting be spread to the *recipients* of that parenting? After all, offspring can't provide their own parenting; that's precisely the problem. But they can "invest" in themselves, so to speak, by raising the cost of parenting for mothers and others by getting *better at eliciting it*—by more cleverly molding parents through their deeper understanding of order and control, thereby more often managing the attachment relationship (the parent-offspring arms race) in their favor. Those best at molding their parents— those with the deepest understanding of parents' intentions and the political economy of parent-offspring conflict—tend to elicit more or longer parental investment, thereby acquiring the security (material and social-emotional) that by all accounts fosters survival, growth, development, and long-term reproductive success (trading reduced offspring quantity for increased quality through more intensive parenting) (e.g., Chisholm, 1999; Chisholm & Burbank, 2001; Hrdy, 2000, 2001). As adults themselves, those best at molding their own parents and in turn also good at molding not only their own offspring (thereby maintaining the parent-offspring arms race), but others as well, because as adults, they could scarcely avoid using their pre-existing CDM or theory-of-mind skills for understanding order and control in their machiavellian attempts to mold social relations generally.

This phylogeny of theory of mind (CDM, really) may be speculative, but it is principled speculation. It is consistent with basic principles of sexual selection theory, parental investment theory, parent-offspring conflict theory, and mammalian and primate evolutionary ecology, in particular the evolutionary ecology of attachment. It is also consistent with an intriguing fact of human anatomical development: Compared to nonhuman primates, human infants are motorically and sexually highly altricial but cognitively quite precocial, with high rates of fetal and neonatal brain growth being maintained much longer during development than in other primates (e.g., Bogin, 2001). Development in logicomathematical, physical, and social cognition also co-occur in humans more than in nonhuman primates; therefore, there is increased mutual reinforcement through crossover and integration across cognitive modes (Langer, 2000; Parker, 1996; Parker & McKinney, 1999). Slow motor development and delayed sexual maturation are characteristic features of K-selection, but precocial cognitive development is not. On the other hand, mosaic development is phylogenetically widespread, and relatively precocial cognitive development, especially with greater cross-modal integration with social cognition, might well serve the special adaptive function of increasing the infant's capacity to manage parentoffspring conflict in her favor.

THE ONTOGENY OF THEORY OF MIND

Of course, even principled speculation has its limits. Fortunately, as foreshadowed, there is empirical evidence about theory of mind's development that is also consistent with the working hypothesis that theory of mind emerged phylogenetically from CDM via the attachment process, and emerges ontogenetically the same way, and so is not an innate, domain-specific module in the usual sense, but becomes modularized ontogenetically (through overlearning) and thus constitutes a developmental exaptation. In other words, theory of mind is a developmentally contingent, adult exaptation for detecting intentions generally, but was originally (phylogenetically and ontogenetically) an infant adaptation for eliciting parental investment.

First, there is robust evidence that contingency detection simply *feels good.* Some time ago, John S.Watson (1972, 1979, 1985) discovered that when young infants perceived that they could control the movement of a mobile suspended above their cribs by wriggling their bodies (thereby closing pressure-sensitive switches activating a motor driving the mobile), not only did they wriggle more than infants who lacked such control, they also *smiled* more than the other infants. This finding has been widely replicated and there is now a consensus that the perception of "response-contingent stimulation" is innately pleasurable, that it "releases" feelings of positive affect. Recently, Gergely and Watson (1996, 1999) have placed the idea that perception of causal efficacy is innately rewarding at the foundation of their social-biofeedback model of early socialemotional development. Although logically an agent can only notice a contingency between X and Y if she can detect both X and Y in the first place, noting the contingency can also refine the agent's awareness of X and Y. For example, adults are not very aware of their own blood pressures, but can increase their awareness (sufficient even to control it) through biofeedback from a blood pressure cuff that mirrors their blood pressure back to them. By analogy, Gergely and Watson doubt that infants are very aware of their own emotional states, but increase their awareness via parental mirroring of their affective states and automatic behavioral expressions. Consistently sensitive and responsive parental mirroring of infant affect provides the infant with an external (environmental) stimulus that is highly contingent on her internal emotional/ physiological states (her cue variable). Given sufficiently consistent mirroring, the infant's CDM does two jobs: First, it helps her refine her awareness of her internal states because they covary with the "mirroring" provided by her environment (her mother), thus providing

> a kind of "teaching" or "scaffolding" environment that results in the internalisation of the maternal affect-regulative function through the establishment of secondary representations of the infant's primary emotionstates. (Gergely & Watson, 1999, p. 1200)

Second, the mirroring *feels good,* because the perception of causal efficacy is innately satisfying. Because her primary emotion-states covary with her mother's mirroring, her awareness of these states increases because the mirroring feels good. And because this mirroring feels good, the internal states that it reflects become more salient; as Watson put it, "contingent effects *become interesting* by virtue of being detected as contingent" (2001, p. 305; emphasis added). With more interesting internal states, the infant becomes more interested in playing with them or manipulating them, leading to more complex and sophisticated secondary representations—and thus awareness—of them. Whenever the CDM does its job—whenever it detects parental mirroring, which by definition is contingent on infant behavior—it releases subjectively positive feelings.

> Therefore, the detection of contingent control over the parent's affect-mirroring display will induce positive affect in the infant which, through reciprocal inhibition, can be expected to further decrease his/her negative affect-state. (Watson, 2001, p. 1196)[19]

Parental mirroring kills two birds with one stone: (a) it modulates infant arousal, thereby reducing negative affect; and (b) provides the infant with the environmental scaffolding sufficient for learning, in effect, that she is a “movement control system”—an intentional agent. Just as we can logically, if figuratively, attribute intentionality to a thermostat, the infant can logically and literally attribute intentionality to herself. And *this feels good.* When she realizes that she has had an effect on mother, she feels like *smiling.* To have an effect on mother is to mold her, which is both the essence of politics and just what we would expect from the theory of parent-offspring conflict.

Second, there is reason to believe that the infant’s perception of parental mirroring is intimately involved in the attachment process. Peter Fonagy and his colleagues note that most of the research on theory of mind has come from a social-cognitive developmental perspective that treats the child as an “isolated processor of information engaged in the construction of a theory of mind from first principles” (Fonagy, Redfern, & Charman, 1997, p. 52). They maintain instead that because theory of mind is inherently an intersubjective process measures of social-emotional development can’t help but be involved in theory of mind’s development and the understanding of others’ intentions. Following Mary Main (1991), among others, Fonagy et al. propose an attachment model of the development of theory of mind in which mothers scaffold theory of mind by

> behaving towards the child in such a way that leads him or her to postulate that their own behavior may best be understood through the assumption that they have ideas and beliefs, feelings and wishes which determine their actions and the reactions of others to them. (Main, 1991, p. 52)

The infant’s *ability to make* this assumption, they argue, comes from CDM, for in mirroring her child’s affect states and automatic behaviors mothers act as if their children had intentions, which enables the child’s CDM *to learn* that in fact *they do* (Fonagy & Target, 1997). And this learning *feels good.* This may help explain why infants judged securely attached are regularly found to have had the most *consistently* sensitive, accepting and responsive caretakers (e.g., Ainsworth, Blehar, Waters, & Wall, 1978; Marvin & Britner, 1999): because they most consistently experience the positive affect automatically released by their mothers’ mirroring. Such consistently positive feelings help the child learn “to move from a teleological to a mentalising model of behavior” (Fonagy & Target, 1997, p. 689)—i.e., from understanding behavior in terms simply of contingency detection (which nonetheless feels good) to understanding behavior in terms of what Bowlby (1969) called “internal working models” of attachment (IWMs, for short). Internal working models are a lot like theory of mind: they

> …ve to regulate, interpret, and predict both the attachment figure’s and the self’s attachment-related behavior, thoughts, and feelings. If appropriately revised in line with developmental and environmental changes, internal

> working models enable reflection and communication about past and future situations and relationships, thus facilitating the creation of joint plans for proximity regulation. (Bretherton & Munholland, 1999, p.89)

As a function of both our CDMs (old information about the objective conditional probabilities connecting events or actions in our evolutionary past) and our individual attachment histories (new information about our current environment) we construct IWMs of ourselves and others in terms of “behavior, thoughts, and feelings” (actions/intentions, beliefs, and desires). If our attachment relations have been consistently positive we are apt to construct IWMs of ourselves as the kind of person about whom others have good beliefs, desires, and intentions. As a result, we may have a greater capacity to use our attachment figures as both secure base to explore our worlds and safe haven when we find them frightening. Our worlds, of course, consist very importantly of other minds, because getting information into and out of other minds is what culture is all about (Dennett, 1995; Tomasello, 1999). According to the synthesis of attachment theory and Gergely and Watson’s social-biofeedback model proposed by Peter Fonagy and his colleagues, children with secure attachment histories are better able to construct and perform mental operations on images of their own and other people’s minds because they are less frightened of the images they come up with. Finding such images easier to hold in mind, secure children come to have more experience in manipulating them, thereby eventually “overlearning” them and coming up with more complex, sophisticated, and realistic images of minds than children whose IWMs contain more fearful images.

It is no wonder then that secure attachment is associated with earlier and/ or better performance on various measures of theory of mind. For example, Fonagy, Redfern, and Charman (1997) found that a projective test of attachment security was a good predictor of belief-desire reasoning in 77 3- to 6-yearold children even after controlling for age, verbal mental age, and social maturity. In another study, 82% of children classified as securely attached at 12 months passed a belief-desire test at 18 months compared to just 46% of those classified as insecurely attached (cited in Fonagy & Target, 1997). Meins, Fornyhaugh, Russel, and Clark-Carter (1998) found that securely attached 2½ year olds performed significantly better on a false-belief task than insecures, and had mothers who were more likely to describe them in terms of their mental characteristics. Steele, Steele, Croft, and Fonagy (1999) found that children’s understanding of others’ emotions at 6 years was predicted by their security of attachment 5 years earlier. Finally, because theory of mind is the ability to enter into other minds, to see things from another’s perspective, we shouldn’t be surprised to find that securely attached 19-month-old children were significantly better at recognizing themselves in mirrors when their noses had been daubed with rouge than were insecure children (Schneider-Rosen & Cicchetti, 1984).

CONCLUSION: THEORY OF MIND IS A DEVELOPMENTAL EXAPTATION OF THE CONTINGENCY DETECTION MODULE AND ATTACHMENT

This has been an exercise in bridge building between evolutionary theory and cognitive science. Its premise was that if modules are adaptations and theory of mind is a module then theory of mind is an adaptation and should have the characteristics of an adaptation. From the combined developmental/evolutionary perspective of life history theory theory of mind seemed *not* to be a module in the usual sense of the word, or an adaptation, but a developmental *exaptation* of two phylogenetically earlier adaptations, a contingency detection module and the mammalian or primate infant-mother attachment module. Evolution can only work with what it already has—with traits that have been expressed phenotypically. Before any animal had theory of mind, many animals seem to have been able to detect the intentionality of movement (behavior) control systems. When this capacity found itself in the particular phylogenetic context of increasing sexual selection and *K*-selection and the infant-mother attachment module in hominids, the result was contingency-detection in the service of parental investment and thus parent-offspring conflict as well. In the ensuing parent-offspring arms race, mothers began to function more and more as mirrors for their infants, thus providing them with the scaffolding whereby they could overlearn that mothers, themselves, and by extension, other people, all have beliefs, desires, and intentions.

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NOTES

1. Quoted in Remnick, D. (2002). The dreamer. *The New Yorker,* January 7, p. 63.
2. A gene has pleiotropic effects when it makes a differences in the expression of two or more phenotypic traits.
3. Cosmides and Tooby (Cosmides, 1989; Cosmides & Tooby, 1989,1992,1994) have proposed the existence of a cheater detection module as part of a larger social exchange monitor. I think it is arguable that theory of mind subsumes the detection

of cheaters. If theory of mind is for predicting and explaining other people's intentions then it fulfills the cheater detection module's supposed function whenever it detects the intention to cheat. It seems wrong to suppose two modules when one will do.

4. Whether or not theory of mind is one, modules *per se* may evolve through the process of genetic assimilation or the Baldwin effect. See Deacon, 1997 (pp. 322–334) for an excellent recent account of this little-understood but probably very important form of natural selection.
5. Evolutionarily old information for the subjective experience of both confidence and hope (good) and fear and anxiety (bad) seems to be represented significantly in the amygdala (e.g., Hamann, Ely, Grafton, & Kilts, 1999; LeDoux ,1996).
6. See Belsky (1999), Chisholm (1999), and Simpson (1999) for recent evolutionary ecological/psychological analyses of the attachment process.
7. This is well before theory of mind emerges at around 3 to 4 years, at least as evidenced by ability to pass false-belief tasks.
8. Gergely and Watson rely heavily on Watson's extensive early work on the perception of response-contingent stimulation in infancy (e.g., Watson, 1972, 1979, 1985).
9. The latter figure is much more likely than the former. Dean et al. (2001) compared daily increments in deposition of tooth enamel as an index of the evolution of growth processes in modern humans, *Homo erectus,* and earlier hominids. They found that modern growth patterns first appeared in the larger-brained Neanderthals roughly 200,000 years ago. They note that

> The size of key brain components associated with learning and cognition correlates with the timing of dental development in primates as the cost in time needed to grow and learn to use a larger brain increases. In this context a slower trajectory of enamel growth in permanent teeth, one of the basic determinants of tooth formation time, can be regarded as a life-history attribute associated with the extended, or prolonged, growth period of modern humans. (p. 630)

10. "Mother" is more accurate than "parent" because, as a class, mammalian mothers provide much more parental investment than mammalian fathers. The same logic holds for fathers, of course, but only if they really *are* fathers (i.e., 50% genetically related to offspring). This is why the concept of paternity confidence is so important for thinking about human evolution: because male parental investment is—or at least can be—much greater in humans than any other mammal.
11. Want" and "willing" are in quotation marks to indicate that there is no requirement that offspring or mother be consciously aware of their "desires."
12. The most authoritative and thorough discussion of attachment is Cassidy and Shaver (1999).
13. One enduring source of danger is infanticide by conspecifics other than parents (e.g., Hausfater & Hrdy, 1984; Hrdy, 1977, 1979, 1992, 2000), identified so far in 34 nonhuman primate species (Hrdy, 2001).

14. Gubernick notes that "limited mobility" is not quite the same as altricial ("slowdeveloping"), for some otherwise precocial species have relatively immobile newborns but also fairly intense attachment (e.g., pinnipeds, chiroptera). Development, in other words, is mosaic, with different organ systems developing at different rates across species (and often within).
15. Along with energy, nutrients, time, information, etc., parental investment *includes* protection, but from all sorts of risk and uncertainty, not just predators.
16. Relative to body size, the brain size of newborn humans is 1.33 times larger than that of great apes (Bogin & Smith, 1996).
17. In fact, selection did not shorten gestation but prevent it from increasing as much as it otherwise would have because of *K*-selection for longer and slower growth throughout the life cycle. Some estimates are that if human gestation were proportionately as long as it is in other primates it would last 21 months (Gould, 1977).
18. Given that the same (except orgasm) happens in infancy (Gunnar, 1998), it is interesting to note that the psychobiology of infant-mother attachment is also implicated in the ontogeny of adult male-female romantic attachment (Feeney, 1999; Hazan & Zeifman, 1999).
19. Many infancy researchers have concluded that facial-vocal mirroring of infant expressions of affective or physiological state is critical for the infant's ability to become aware of and to regulate his own emotions and states (e.g., Beebe, Lachman, & Jaffe 1997; Brazelton, Koslowski, & Main, 1974; Field, 1994; Mahler & McDevitt, 1982; Stern, 1985; Trevarthen, 1979; Winnicott, 1965). Gergely and Watson, however, note that

> the term 'mirroring' is a seriously midleading one [for] no matter how well the parent is attuned to the baby's state, his/her mirroring [of] facial and vocal behaviour will never match perfectly the temporal, spatial and sensory intensity parameters of the infants behavioural expressions (1996, p. 1197)
>
> On the other hand they also provide theory and data to show that after a certain developmental stage, infants are predisposed to be especially sensitive to *deviations* from perfect imitations.

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7

Recursion as the Key to the Human Mind

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*I*n this chapter, I argue that the ability to think recursively is what distinguishes the human mind from the minds of other animals. The two mental activities that most obviously exhibit recursion are language and theory of mind. Both have featured prominently in psychological research on the great apes over the past half century, with the consensus being that even our closest relatives, chimpanzees and bonobos, have proven incapable of either true language or fully developed theory of mind, although both claims have remained controversial. My contention is that although great apes are capable of some languagelike behavior and have some capacity to take the perspective of others, the missing ingredient is recursion.

I begin, therefore, by defining recursion, and showing how it is involved in language, theory of mind, and some other human capacities. I shall then go on to argue that recursion may be a product of an interaction between critical periods of postnatal growth of the brain and environmental inputs, creating a phase of development that may be unique to the genus *Homo.*

RECURSION

Recursion is a mathematical device for generating terms in a sequence, in which the rule for generating the next term in a sequence involves one or more of the preceding terms. A simple example is the use of integers for counting. So long as we have rules that allow us to generate a number from the preceding number in a sequence, then there is no limit to the number of objects we can count. The basic rule, of course, is simply to add 1 to the preceding number. This is not an entirely trivial exercise, because there are subrules that tell you how to do this by starting with the rightmost digit, and dealing with the special case where the rightmost digit or digits equal 9. The point is that the procedure is recursive, and the sequence can be continued indefinitely. Counting is an example of what Chomsky (1988) has called “discrete infinity.”

Recursion and Syntax

Syntax is also recursive, and allows us to generate sentences of any length, and of infinite variety. Consider the following well-known children’s story:

> *This is the house that Jack built.*
>
> *This is the malt that lay in the house that jack built.*
>
> *This is the rat that ate the malt that lay in the house that jack built.*
>
> *This is the cat that killed the rat that ate the malt that lay in the house that Jack built.*
>
> *This is the dog that worried that cat that killed the rat that ate the malt that lay in the house that Jack built.*

This is an exercise in recursion that can be continued indefinitely, just as counting can. Clauses like *that ate the rat* and *that killed the cat* are relative clauses, and our ability to string them together is based on a simple recursive formula, which says that a relative clause can defined (or “rewritten”) as a relative clause plus an (optional) relative clause:

Relative clause   Relative clause+(relative clause)

This is a very simple example of a rule in which the definition of an element involves itself, leading to a potentially infinite loop.

Here are some slightly more complex rules:

(Rule 1)	S	NP+VP
(Rule 2)	NP	article+noun+[RC]
(Rule 3)	RC	relativepronoun+VP
(Rule 4)	VP	verb+[NP]

where S=sentence, NP=noun phrase, RC=relative clause, and VP=verb phrase, and the bracketed items are optional. These rules are sufficient to generate sentences like *The dog that chased the cat killed the rat,* as shown in Figure 7.1. But these rules illustrate a slightly more complex recursion, because the NP involves an optional RC, the RC involves VP, but the VP involves an optional NP, so one can cycle endlessly from Rule 2 to Rule 3 to Rule 4 and back to Rule 2.

In practice, of course, we are limited by constraints on short-term memory, especially if phrases are embedded within phrases. For example, we can rewrite *The House that Jack Built* like this:

> *The malt that the rat ate lay in the house that Jack built.*

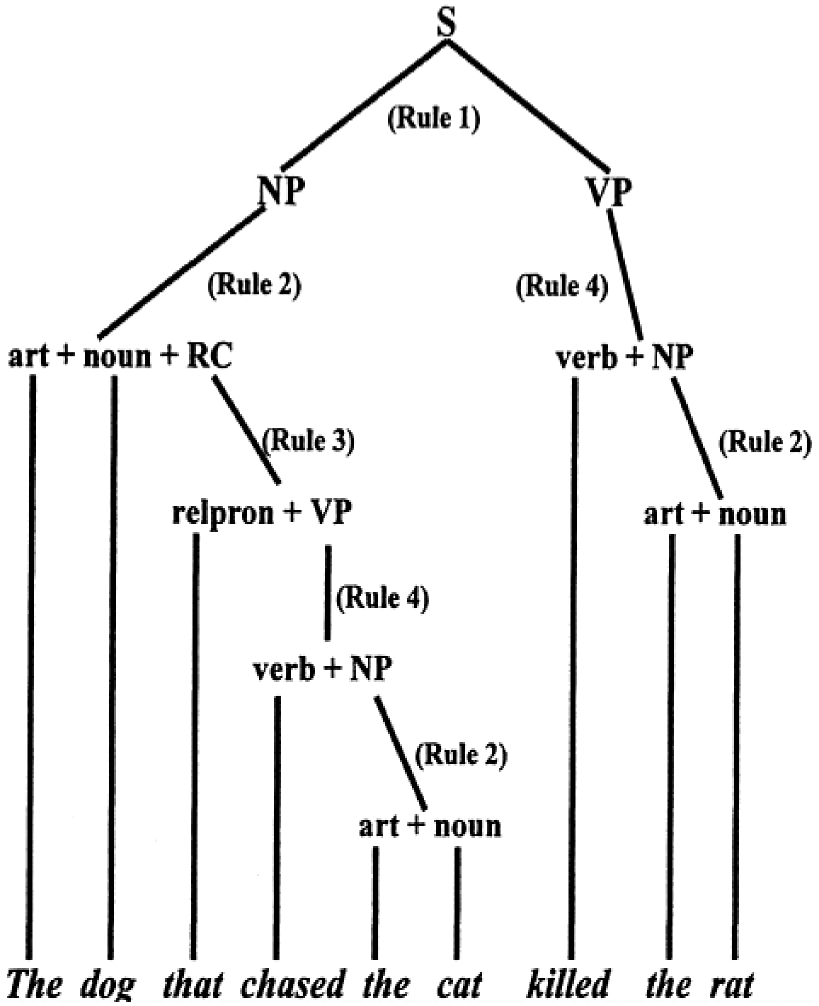

FIGURE 7.1. This tree diagram shows how the recursive use of rules can produce a sentence with an embedded clause.

> *The malt that the rat that the cat killed ate lay in the house that jack built.*
>
> *The malt that the rat that the cat that the dog worried killed ate lay in the house that jack built.*

By now you have probably lost it, even though the sentence is still grammatical and obeys the rules, because you have essentially run out of processing capacity. For a start, there is now a considerable gap between nouns and their corresponding verbs. For example, in the last of the preceding sentences the word “rat” goes with the word “ate”, but there are eight intervening words. An added complication is that you have to perform several pairings in the meantime, so that “cat” goes with “killed” and “dog” with “worried.” In order to keep it all straight, you need considerable working memory capacity in which to both store and organize the relevant information.

Recursion and Theory of Mind

In humans, at least, theory of mind has essentially the same recursive structure. For example, *I may know what you are thinking,* and *I may know that you know what I am thinking.* Concepts such as knowing, thinking, remembering, believing, seeing, and so forth describe mental states and can be employed recursively, because the content of your mental state can be another mental state. In fact, you can be in a mental state about your own mental state, as in the following examples.

I remember seeing him yesterday.
I know that I know that face, but I can’t remember her name.

Of course the sentences that describe these recursive states of mind are themselves recursive sentences. In principle, we can employ recursion as often as we like to establish highly complex social understanding, but in practice we are again limited by constraints on short-term memory. The problem is a bit like that of a chess player, who might figure out what she will do if he makes a particular move, and then how she will respond to that, and then how he will respond—and so on. It is this aspect of chess that computers excel at, but humans soon run out of processing capacity and are better off employing more holistic strategies.

The ability to think recursively may be at least part of what Suddendorf (1999) has characterized as *metamind.* Central to the idea of metamind is the idea of *metarepresentation,* that is, a representation of a representation. Of course, this is inherently recursive. In principle, an understanding of recursion allows the process to continue indefinitely. The very notion of metarepresentation might be considered a meta-metarepresentation—a representation of a representation of a representation. Dennett (1987) has referred to different levels of *intentionality* to characterize the degrees of hierarchy so formed. The principle of recursion may lead to any number of levels of representation, although in practice we are again limited by the constraints on working memory.

Nevertheless, by adolescence most of us have little difficulty in handling fourth- or fifth-order intentionality, or even beyond. In Jane Austen’s *Pride and Prejudice,* Elizabeth Bennet *thinks* that Darcy *thinks* that she *thinks* he *thinks* too harshly of her family. Or in Shakespeare’s *Twelfth Night,* Maria *foresees* that Sir Toby will eagerly *anticipate* that Olivia will *judge* Malvolio absurdly impertinent to *suppose* that she *wishes* him to *regard* himself as her preferred suitor.* Here we have a sixth-order intentionality that may escape the bounds of working memory, but is easily assimilable under the relaxed temporal constraints of the theatre.

DEVELOPMENTAL CONSIDERATIONS

Given the recursive nature of both syntax and theory of mind, it is not surprising that they emerge at around the same time in young children. The "classic" tests of theory of mind in children are tests of the understanding of false belief. Typically, a child is told a story or shown a scene in which some character comes to believe something that turns out not to be true. For example, in one common scenario, a puppet is shown to be asleep, and eggs are removed from an egg container and placed in another unmarked box. When the puppet wakes up, the child is asked where the puppet will look for the eggs. It is usually not until some time in the fourth year of life that children understand that the puppet holds a false belief as to the whereabouts of the eggs. Until they reach this understanding, they typically answer that the puppet will look in the box where the eggs are actually located. The understanding of false belief is a simple test of recursive thought. The child who passes the test *knows that the puppet believes that the eggs are in the carton.*

Syntax also develops between the ages of two and four, and it is typically during the fourth year that understanding of embedded clauses begins to appear. For example, verbs of experiencing, such as *think, see, believe, remember, wonder,* as well as verbs of communication, such as *say, tell, inform,* can be used in recursive fashion with embedded complements that themselves contain verbs. Lewis Carroll provides an amusing example:

> *He thought he saw an elephant*
> *That practiced on a fife.*
> *He looked again, and found it was*
> *A letter from his wife.*
> *"At length I realize," he said,*
> *"The bitterness of life."*

Given that theory of mind and the understanding of embedded complements both involve recursion, and both appear in the fourth year of life, it is reasonable to ask if one precedes the other in development, and is necessary for the development of the other. There is some evidence that recursive syntax actually

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* I am indebted to Brian Boyd for the examples of higher-order intentionality from *Pride and Prejudice and Twelfth Night*. They are taken from a talk entitled "Evolution, Cognition, Narration, Fiction," which he presented at the Interdisciplinary Symposium on the Nature of Cognition, Tamaki Campus, University of Auckland, on October 13, 2001.

develops first, and is perhaps necessary if the child is to go on and develop a theory of mind. de Villiers and de Villiers (1999) gave children aged from 2 to 4 a series of false-belief tasks, and also tested their ability to understand embedded clauses. The embedded clauses themselves referred to situations in which some character in a story had a false belief, told a lie, or made a mistake (as in the Lewis Carroll poem). For example, the child would be given a sentence describing a happening, such as:

He thought he found his ring, but it was really a bottle cap.

The child would then be asked questions like "What did he think?," or in a simpler version "What did he think he found?" Young children typically answer that he thought he found the bottle cap rather than the ring, thereby showing that they failed to correctly represent the embedded complement.

de Villiers and de Villiers found that the results on the tests of false belief correlated significantly with performance on the embedded complements, and also with a measure of the children's ability to use complements in spontaneous speech. Moreover, those who passed the false-belief tests showed continued improvement over testing rounds on the representation of complements, implying that the linguistic understanding precedes the attribution of false beliefs.

One difficulty here is that the linguistic tasks may simply tap memory for sentences, and not the ability to parse them. de Villiers and de Villiers argued against this on the grounds that the children did not simply parrot the sentence. Instead, if they had not reached the stage of understanding false complements, they tended to "fix" the sentence so that the complement was true. That is, the child may reinterpret the sentence to read something like, "He found his ring and a bottle cap." To further boost their argument, de Villiers and de Villiers went on to show that language-delayed deaf children were also delayed in the understanding of false belief (see also Peterson & Siegal, 1999).

These findings need not imply, however, that language is necessary for the development of theory of mind. There is also evidence that a severely agrammatic adult could understand false belief (Varley & Siegal, 2000), suggesting that the critical component of language may not be grammar itself, even though it is grammar that sustains the recursive nature of language. It has been suggested rather that theory of mind may depend on conversational awareness rather than on grammar (e.g., Siegal, Varley, & Want, 2001).

There are also some respects in which language depends on theory of mind, rather than vice versa. Language is a way of influencing what other people think or feel, and to do that requires that we have some appreciation of what is going on in their minds—and even how their minds work. Taking the mental perspective of others is especially evident in that aspect of language known as *pragmatics,* which has to do with the practical use of language appropriate to social settings. It includes understanding the difference between polite and impolite speech, modifying one 's language depending on whether one is speaking to children or adults, understanding irony, and so forth. The pragmatic aspects of language are those that relate most clearly to theory of mind.

There is some neuropathological support for the link between language and theory of mind. Autism has been characterized as a deficit in theory of mind, but children diagnosed with this disorder also typically have difficulty acquiring spoken language. Indeed, despite the fact that theory of mind is often characterized as a "module" in Fodor's sense (Baron-Cohen, 1999), language disorder is such a common feature of autism that it is among the primary identifying features of autism proposed by both the American Psychiatric Association (1994) and the World Health Organization (1993). This is not to say, of course, that autism is simply the result of a language deficit, and the two are at least partially dissociable. It is of interest, for example, that many children with autism who have failed to develop speech have nevertheless learned to communicate using sign language (Bonvillian, Nelson, & Rhyne, 1981). Moreover, it is the pragmatic aspect of language that seems most obviously lacking in autism (Baron-Cohen, 1999), and the syntactic and semantic aspects of language may be relatively unimpaired.

Another aspect of mental development that normally appears in about the fourth year of life is episodic memory. On average, the earliest memories that people have date from around 3½ years (Usher & Neisser, 1993). According to Tulving (1985), episodic memory depends on "autonoetic" consciousness of having experienced remembered events. One can remember that one saw something, remember that one felt ill, or even remember that one once remembered something now forgotten. These acts of reliving the past have a recursive structure, of the form *I know that I experienced X*, and according to Perner and Ruffman (1995) this develops some time between the ages of 3 and 6. Episodic memory may be regarded as part of a more general ability to engage in *mental time travel,* which involves imagining the future as well as the past (Suddendorf & Corballis, 1997). Although we tend to think of memory as factual and the future as hypothetical, both are probably constructed from a mixture of fact and fantasy.

Syntax, theory of mind, and mental time travel all develop at around the same time. I suspect, however, that there are no prior dependencies among them; that is, no one of these capacities is dependent on the prior emergence of another. This is not to say, however, that they do not interact. Language may be influenced by the emergence of theory of mind, especially with respect to pragmatics; theory of mind may be helped by the ability to verbalize episodic memo ries or future plans; mental time travel involves the understanding of one's own mental state at another point in time, and this in turn may be facilitated by the ability to understand the mental states of others. But my guess is that all of these capacities depend separately on the emergence of recursive structures that arise from an interaction between brain growth and environmental input, especially during early postinfancy years from about 2 to 5. This is explained in more detail in the following.

AN EVOLUTIONARY PERSPECTIVE

Language and Syntax

After several decades of attempting to teach language to great apes, there is still controversy over whether any great ape has demonstrated anything resembling syntax. Probably the most impressive of the so-called linguistic apes is Kanzi, the bonobo studied by Savage-Rumbaugh (see Savage-Rumbaugh, Shanker, & Taylor, 1998 for a detailed account). Kanzi has shown a remarkable ability to use symbols on a specially designed board to generate messages, and to interpret messages generated by others. The symbols, known as *lexigrams,* are specially chosen *not* to be pictorial representations of what they stand for. Kanzi has mastered the 256 lexigrams on the keyboard, and has spontaneously supplemented them with manual gestures to expand his vocabulary. Kanzi is able to generate novel requests by pointing to appropriate combinations of symbols on the board, and also to understand novel sequences. These are simple, comprising two- or three-word combinations such as "hide peanut," "chase you," "hot water there," or "childside food surprise." It is reasonably clear from accounts of these exploits that Kanzi is not prompted in any way, and that many of his "utterances" are novel combinations that neither he nor his trainers have used before.

As is the case in human children, Kanzi's comprehension goes beyond his ability to produce utterances, and he has even developed a striking ability to understand spoken English. In one study, he was given a list of 660 unusual spoken commands, some of them eight words long, and was able to carry out 72% of them correctly. Kanzi was nine at the time, and did a little better than a 2½-year-old girl called Alia, who managed 66% correct. It would be easy to overestimate Kanzi's linguistic skill from these examples, becuase Kanzi need not process every word, and almost certainly did not do so—and nor did Alia. The sentences can generally be understood if one extracts only the content words, ignoring the function words. For example, a sentence like "Go get the balloon that's in the microwave" can effectively be reduced to "Get balloon microwave," and the meaning inferred with little ambiguity. Even so, Kanzi's ability to pick out the content words from the more-or-less continuous sequence of sounds is impressive and unexpected.

It is unlikely even from these examples that Kanzi has mastered any but the most elementary syntax; as Pinker (1994) remarked, great apes like Kanzi "just don't 'get it'" (p. 340). There is certainly no evidence that they understand anything as complex as an embedded clause—or indeed any aspect of language that involves recursion. Language in the chimpanzee does not appear to have developed beyond that of the 2-year-old child, a presyntactic level of language that Bickerton (1995) called “protolanguage.”

Theory of Mind

There is some evidence that the great apes are capable of limited theory of mind. This is illustrated by instances of tactical deception, as when a subordinate animal waits until a more dominant animal isn’t looking before committing some forbidden act. Such instances seem to imply an understanding of what it means for another animal to see. Whiten and Byrne (1988) undertook a broad survey of field workers for instances of tactical deception in primates, rejecting examples where the behavior may be simply a result of trial and error, and concluded that only the great apes are capable of intentional tactical deception. Even so, there were relatively few recorded instances—only 12 from common chimpanzees, and three each from bonobos, gorillas, and orangutans.

It is of interest that 17 of the 21 instances seemed to involve mental-state attribution, and only four involved visual perspective-taking. It has been argued that chimpanzees, at least, actually do not understand that others can “see,” even though their behavior may sometimes be governed by where others are looking. Chimpanzees naturally and easily follow the gaze of others, and human children also show this ability as early as the second year of life (Moore & Corkum, 1998; Tomasello, Hare, & Agnetta, 1999), even though they are not yet capable of passing false-belief tests. Moreover, there is evidence that chimpanzees do not interpret or understand pointing or eye gaze in quite the same way that humans do even at the tender age of 3. This was illustrated by experiments carried out by Povinelli and his colleagues (see Povinelli, Bering, & Giambrone, 2000, for a summary). Chimpanzees can easily be taught to approach people they know and beg for food. If a person sits in front of a chimpanzee and points to one of two boxes to left or right, the chimpanzee understands readily enough that if it wants food, it should go to the box that the person is pointing to. But the choice breaks down if the person points from some distance away, and is systematically reversed if the person sits closer to the box that does not contain the food and points to the other one. It seems that chimpanzees respond on the basis of how close the pointing hand is to the box containing the food, and not on the basis of where the hand is actually pointing. Again, young children have little difficulty with these choices.

Similarly, when confronted with a choice between two people, one with and the other without a blindfold over her eyes, chimpanzees did not seem to appreciate that they should beg from the one who could see. The same was true when one of the people had a bucket over her head, or covered her eyes with her hands. Only when one of the people was actually facing the other way did the chimpanzees easily choose the one facing toward the animal. Young children, on the other hand, quickly recognize that they should approach the person who can see them, and understand that this depends on the eyes. The failure of the chimpanzee to appreciate this does not arise from failure to observe the eyes, because they readily follow the gaze of a person confronting them. Chimpanzees may eventually choose the person who can see them, but this is probably based simply on painstaking associative learning, and not on the understanding that eyes are for seeing.

One may be tempted to conclude from this that chimpanzees are simply rather stupid, and have to rely on dogged learning rather than on theory of mind. Povinelli argues, though, that many behaviors, such as following eye gaze, have the same basis in humans as in other primates, but that we "reinterpret" these behaviors as being more sophisticated than they really are. For example, we may spontaneously follow the gaze of someone who seems to be gazing at something in the sky without going through an intellectual exercise along the lines of: "That fellow must be able to see something up there that's interesting." Gaze following may simply be an adaptive response that alerts other animals to danger or reward, but we humans have intellectualized it, often after the fact.

There is some evidence, though, that the work of Povinelli and his colleagues underestimates the social intelligence of the chimpanzee. Chimpanzees are by nature competitive creatures, and one may wonder why they should trust what humans are trying to indicate. Dogs, in contrast, have been bred to cooperate with humans, and Hare and Tomasello (1999) have shown that dogs do seem to be able to choose food sources according to where either a person or another dog is looking or pointing. Hare, Call, Agnetta, and Tomasello (2000) have also shown that chimpanzees are aware of what other *chimpanzees* can see, and modify their behavior accordingly. For example, a chimpanzee will approach food when a more dominant chimpanzee cannot see the food, but will be reluctant to do so when it can see that the dominant chimpanzee is watching.

Leslie (1994) has distinguished between two levels of what he calls the Theory of Mind Mechanism (ToMM). A child or a chimp with ToMM-1 can attribute goal-directedness and self-generated motion to others, whereas a child with ToMM-2 attributes full-blown intentional states, such as believing, desiring, seeing, or remembering, to others. ToMM-1 does not seem to enable recursion; attributing self-generated motion to another creature carries no implication that the other creature can also attribute self-generated motion. But once desires and beliefs are attributed, as in ToMM-2, then recursion can follow, because one can attribute the attribution of such desires and beliefs. There seems no reason at present to suppose that the great apes can advance beyond ToMM-1, at most, and Hauser and Carey (1998) suggest that "the intellectual tie breaker between humans and nonhumans is likely to lie within the power of ToMM-2." A person with full recursive capabilities endows unto others all of the mental states that they themselves experience, and do so recur sively.

Episodic Memory and Mental Time Travel

There has been some debate as to whether other species are capable of episodic memory, or more generally of mental time travel. Tulving (1985), like Suddendorf and Corballis (1997), has argued that true episodic memory is uniquely human. Animals might remember where an object is located, but this need not mean that they remember the actual act of locating it. The memory might simply consist of an association of the object and its location. It has been claimed, however, that some birds have genuine episodic memory for the caching of food, on the grounds that they retrieve food on the basis of how long it has been stored. For example, European jays cache perishable food less often than nonperishable food and recover it more quickly (Clayton, Mellor, & Jackson, 1996). This might seem to indicate that the birds remember where, when, and what they have cached—three of the ingredients of episodic memory. Yet there must be some doubt as to whether this memory is autonoetic, or selfreferential, as defined by Tulving (1985, 2001). A simple code that links the type of food with the place where it was cached, together with a time-varying tag that indicates how long it has been in storage, could explain the results more parsimoniously than the supposition that the birds can recover the actual experience of caching the food.

There has also been some discussion as to whether nonhuman species are capable of mental time travel into the future. For example, chimpanzees have been shown to make pointed tools from sticks at one location for use later in termite fishing at another (Goodall, 1986). Again, though, it has been argued that such behavior is governed by present drive state rather than anticipation of a future one (Suddendorf & Corballis, 1997). Tactical deception in great apes, discussed earlier, has also been taken as evidence of action with some future benefit in mind, but others have argued that such behaviors can be explained as a response to immediate behavioral and contextual cues whose significance is learned from past experience (Tomasello & Call, 1997). Although more complex, such anticipatory behaviours may be no more indicative of an awareness of the future than the conditioned salivation of a dog to a bell signaling the arrival of food. As Atance and O'Neill (2002, p. 537) state, "At present, the consensus appears to be that primates are solely present-oriented."

For the most part, then, the evidence suggests that chimpanzees are roughly on a par with 2-year-old human infants with respect to language, theory of mind, and perhaps mental time travel. Language remains at the level of protolanguage, capable of producing and understanding simple propositions, but without the recursive structure to produce embedded complements, or to generate sen tences of any desired length or complexity. Similarly, theory of mind in the chimpanzee may simply involve an awareness of social relations, but still without the recursive understanding that gives rise to what has been called the machiavellian mind—the ability to know what others are thinking and plan one's devious behavior accordingly

A NEUROPSYCHOLOGICAL PERSPECTIVE

The progression from protolanguage to language, and from social awareness to true theory of mind, might depend partly on brain growth. Humans differ from the chimpanzees in that they have much larger brains relative to body size. It is estimated that humans have brains that are three times as large as that predicted in a primate of the same body size (Passingham, 1982).

The increase in brain size began about 2½ million years ago, with the emergence of the genus *Homo.* This genus, of which our own species *Homo sapiens* is the only survivor, is also credited with the earliest stone tools, also dating from around 2½ million years ago and possibly associated with *Homo rudolfensis.* Not long afterwards, from perhaps 2 million years ago, *Homo erectus* began the series of migrations out of Africa that were eventually to populate the entire globe. These developments may well reflect the emergence of syntactic language and perhaps theory of mind. Mental time travel may also have been involved, because migration to new territory presumably involved some anticipatory planning.

The recursive construction of sequences may impose a considerable burden on working memory, as illustrated earlier, which may explain the selective pressure toward a larger brain. But it may not be simply a matter of brain size. Humans also differ from other primates in that most of the growth of the brain takes place outside of the womb. Indeed, to conform to the general primate pattern, human babies should be born at around 18 months, not 9 months (Krogman, 1972), but as any mother knows this would be impossible, given the size of the birth canal. The brain of a newborn chimpanzee is about 60% of its adult weight, that of a newborn human only about 24%. Our prolonged childhood means that the human brain undergoes most of its growth while exposed to external influences, and is therefore more finely tuned to its environment.

This may explain the so-called “critical period” for the learning of language. There is abundant evidence that children do not learn language properly unless they are exposed to it early in childhood (e.g., Curtiss, 1977; Lenneberg, 1967; Pinker, 1994), and there is now good evidence that it applies to the learning of signed languages as well as spoken languages (Emmorey, 2002). The critical period need not imply some absolute time window for learning language. Rather, the ability to acquire language begins around age 2, and falls off gradually until around puberty, after which it becomes virtually impossible to acquire a first language, and the ability to acquire a second language also falls off over the same range (Johnson & Newport, 1989).

One aspect of language that is especially dependent on the critical period is syntax, which suggests the more general conclusion that recursion depends on that critical period of growth between the age of about 2 and puberty. Elman (1993) has devised a connectionist network with recurrent loops that can apparently learn something resembling grammar. Given a partial sequence of symbols, analogous to a partial sentence, the network can learn to predict events that would follow according to rules of grammar. In a very limited way, then, the network "learns" the rules of grammar. At first, the network was not able to handle the recursive aspects of grammar, in which phrases are embedded in other phrases, so that words that go together may be separated by several other words. However, this problem was at least partially surmounted when Elman introduced a "growth" factor. Early on, the system was degraded so that only global aspects of the input were processed, but the "noise" in the system was gradually decreased so that it was able to process more and more detail. This decrease in noise was considered equivalent to growth. When this was done, the system was able to pick up some of the recursive quality of grammar, and so begin to approximate the processing of true language (Elman, 1993; Elman, Bates, & Newport, 1996).

My hypothesis, then, is that experience during growth is the key to recursion. It is also likely that the frontal lobes are especially critical to this. According to Deacon (1997), the human prefrontal cortex is about twice the size one would expect of a primate with the same brain size. Syntax is usually associated with Broca's area in the left prefrontal cortex in humans, and there is also good evidence that theory of mind depends on the prefrontal cortex. Stuss, Gallup, and Alexander (2001) tested people with brain damage on frontal-lobe damage on theory of mind tasks, including visual perspective taking (similar to the tasks used by Povinelli and his colleagues in testing chimpanzees) and the detection of deception, and found impairments in those with frontal lesions, but not in those with lesions elsewhere in the brain. There is also evidence that the frontal lobes are critical to episodic memory, and indeed to mental time travel into the future as well as into the past (Wheeler, Stuss, & Tulving, 1997).

This is not to say that there is a single "recursion module" underlying syntax, theory of mind, and other recursive structures. For a start, theory of mind and the pragmatic aspects of language appear to be more dependent on the right than on the left hemisphere (Happé, Brownell, & Winner, 1999; Siegal, Carrington, & Radel, 1996; Stuss, Gallup, & Alexander, 2001). There is also evidence that musical syntax involves the area homologous to Broca's area in the right frontal cortex, at least in those without extensive musical training (Maess, Koelsch, Gunter, & Friederici, 2001). Among professional musicians, by contrast, the representation of musical skill appears to be predominantly left-hemispheric, involving areas that lies close to the language areas, including Broca's area (Sergent, Zuck, Terriah, & MacDonald, 1992). This suggests that the allocation of cortical space to different skills may depend in part on differential growth in the two sides of the brain. There is evidence of a left-hemispheric growth spurt between the ages of 2 and 4 years (Thatcher, Walker, & Guidice, 1987), and given Elman's demonstration of the role of growth, this may explain why syntax is left-hemispheric, and may perhaps also explain why those exposed to musical training from an early age may also develop left-hemispheric dominance for music. (It is also possible that the left-hemispheric growth spurt is a consequence of the fact that syntax develops in that hemisphere. This is not my preferred interpretation.) This may be followed in later childhood by a right-hemispheric growth spurt that allocates musical syntax to the right hemisphere in those who do not receive early training.

Another activity that has a recursive structure is the use of tools and manipulation of objects. Greenfield (1991) has given an account of how children develop hierarchical representations for both language and the manipulation of objects at around the same time. Just as they begin to combine words into phrases and then use those phrases as units to combine into sentences, so they begin to combine objects, such as nuts and bolts, or nesting cups, and then use the combinations as objects for further manipulation. Greenfield argues that both activities depend on a region corresponding to Broca's area on the left side of the brain. She goes on to suggest that this relation between language and hierarchical manipulation persists into adulthood, citing evidence that people with Broca's aphasia are also poor at reproducing drawings of hierarchical tree structures composed of lines (Grossman, 1980).

Greenfield's analysis stops short of the development of true recursion, and the sophisticated understanding and use of tools that we humans possess relative to other animals. Of course, many animals and birds use tools, and some even manufacture tools. New Caledonian crows, for example, fashion digging tools from pandanus leaves according to specific design templates (Hunt, 2000). In a review of evidence, however, Beck (1980, p. 218) noted that "Unquestionably man [*sic*] is the only animal that to date has been observed to use a tool to make a tool"—and 22 years later I know of no evidence to contradict this. In short, we humans—women as well as men—manufacture tools recursively, and it is largely because of our recursive understanding of manufacture that we have polluted the earth with immense cities.

However, Greenfield also refers to evidence that, in a sample of mentally retarded children, some were skilled in hierarchical construction but deficient in grammar, whereas others showed the reverse pattern. She relates these findings to neurophysiological evidence that up to the age of 2, the same brain area may be involved equally in both functions, but beyond that age there is increasing differentiation in the region of Broca's area, such that an upper region has to do with the manipulation of manual objects and an adjacent lower region organizes grammar. In many cases of brain injury, both regions are damaged, result ing in combined deficits, but in some cases one or other region might be damaged, resulting in dissociated deficits.

TOWARD A THEORY

Greenfield's analysis may have wide generality, and apply to the growth and differentiation of a number of recursive skills, including language, theory of mind, episodic memory, music, and object manipulation. All of these skills seem to emerge early in childhood, at a time when the brain is growing. The critical period of postnatal growth is both an evolutionary and a developmental phenomenon; it emerged as a characteristic of the genus *Homo* some 2½ million years ago, and governs the way children acquire skills. My surmise is that it is the key to recursion, taking us beyond simple associative networks toward more dynamic processors that can parse hierarchical structures and use rules recursively It is also possible that recursive thinking begins with a common network, probably involving the frontal lobes, but that the networks gradually become differentiated, perhaps guided by genetically programmed growth gradients that apportion different skills to one or other side of the brain, and to different regions within each side. This may be nature's clever way of creating cerebral asymmetries, and maximizing storage capacity.

Syntax, theory of mind, and mental time travel may all be part of this complex. All may depend on the recursive structuring of neural networks as a function of the interaction between growth and environmental input. All are somewhat dissociable, yet their codevelopment, and perhaps coevolution, may lead to linkages. For example, the emergence of recursive syntax may have been selected for precisely because it maps on to the recursive structure of theory of mind, and allowed our ancestors to communicate their machiavellian thoughts—no doubt to their accomplices, not their rivals. It may also allow people to express past events or planned future ones. Theory of mind may be involved in a different way in language, allowing us to modulate our speech in conformity with the mental state of the listener. Recursion, then, is a property that infuses the early development of basic skills, providing us with the flexibility and creativity that characterizes the human mind.

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8

Reinterpreting the Mentality of Apes

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*A*s a soccer player, one of us was often confronted with the challenging dilemma of taking penalty kicks. Up to about age 13, 1 (T. S.) could quite reliably convert the shot by simply peeking briefly to one corner of the goal, running up and then casually placing the ball in the other corner. I relied not on the accuracy or velocity of my shot, but almost entirely on fooling the keeper that I intended to shoot in the opposite direction. But then some clever keepers picked up on this simplest of tricks and tried to thwart my attempt by jumping in the opposite corner to the one I looked to. Some even tried to turn the tables by offering one side (moving closer to the other post). The battle became increasingly more challenging as I was sizing up the keeper's ability to read my intentions and do the opposite of what I thought he thought. For example, I pretend to place it right, but I think that he thinks I am only pretending to place it right—so I may chose to place it right after all. This is theory of mind in action.

Most theory-of-mind research uses verbal paradigms to assess children's reasoning about the mind. As the preceding example illustrates, however, theory of mind manifests in our nonverbal actions. Naturally, investigations into potential theory of mind in nonhuman animals must rely on such nonverbal performances. Whether our closest animal relatives share such manifestations of theory of mind has been a topic of much research and discussion in recent years. But identification of an unequivocal behavioral indicator for mind-reading has proved frustratingly difficult. Although observations of great ape behavior in natural (Whiten & Byrne, 1988) and experimental (Povinelli, Nelson, & Boysen, 1990; Premack & Woodruff, 1978) settings resulted in initial enthusiasm, in recent years, research has increasingly failed to find support for the idea that great apes appreciate mental states (e.g., Heyes, 1998, for a review).

Thus, in spite of their sophisticated social skills (Goodall, 1986), social traditions (Whiten et al., 1999) and potentially mindreading-related behaviors such as deception (Whiten & Byrne, 1988) and gaze-following (Tomasello, Hare, & Agnetta, 1999), we have to contemplate the possibility that chimpanzees may not interpret the social world in the mental terms that are so natural to mature humans in these contexts. Chimpanzees have so far failed what many believe to be the acid test for theory of mind, a (nonverbal) false-belief task (Call & Tomasello, 1999), and close experimental scrutiny has suggested a purely behavioral basis to their apparent understanding of seeing, attending, and pointing (Povinelli, 1996; Povinelli & Eddy, 1996; Povinelli & Prince, 1998).

THE REINTERPRETATION HYPOTHESIS

To explain this apparent disparity between apes' social expertise and the failure to find empirical evidence for theory of mind, Povinelli and colleagues have proposed the reinterpretation hypothesis (Povinelli, Bering, & Giambrone, 2000; Povinelli & Giambrone, in press; Povinelli & Prince, 1998). According to this account, great apes are thought to display remarkably similar social behaviours to humans not because they share with humans the capacity for theory of mind, but because humans share with great apes sophisticated mechanisms for social behavior that function without second-order intentionality. Theory of mind may have evolved uniquely in the human species. Moreover, it may not have resulted in radically new behavioral patterns (hence the difficulty in identifying clear behavioral indicators of theory of mind). Povinelli and colleagues question the logic of what they call the argument by analogy, which runs as follows: we display behavior X and great apes display behavior X; we believe that our behavior X is usually caused by second-order mental states, thus great apes' behavior X is caused by the same second-order mental states (i.e., a theory of mind). The authors question whether we are correct in asserting that our behavior X is in fact usually caused by second-order mental states (even if sometimes it is). Perhaps the behavior can be generated by other mechanisms and we may only reinterpret that behavior in mentalistic terms after the fact. Consequently, the inference by analogy loses a lot of force. Great apes may have mechanisms that can generate behaviour often associated with theory of mind in humans, but their behavior may not be produced by theory of mind and they may indeed be unable to reinterpret it in such terms.

Consider our football example. In most ball games where an offensive player is trying to pass a defensive player, the trick is to make the opponent commit to one side or the other and delay one's own final move to such time in order to capitalize on that commitment. Now when we talk about these moves afterward, we explain that we wanted him to think X while we really were going to do Y. But do we actually go through all these intellectual-sounding notions? Standing at the penalty shot, perhaps at times we do. And we may find ourselves going in mental circles as we plot deception and counterdeception. In the heat of the action, however, there may simply be no time to formulate such explicit notions of each other's intent. Instead, we may quite automatically go through all the motions of deception and counterdeception and only afterward interpret them in mentalistic terms. We may simply *reinterpret* the causal basis of that behavior retrospectively (or perhaps, at times, even concurrently). Hence, the reinterpretation hypothesis.

Povinelli, Bering, and Giambrone (2000) suggest that "behavioural forms that primatologists are fond of calling deception, empathy, grudging, reconciliation, and even pointing, all evolved and were in full operation long before there were any organisms that could understand them in these terms." Thus, we may have learned to reinterpret our own ape behaviors in the last 5 million years. And we have (perhaps wrongly) applied this mental reinterpretation to other animals. Although chimpanzees may engage in a chase of rapid and effective side-to-side movements (e.g., de Waal, 1986; record no. 231 in Byrne & Whiten, 1990), 1 it may only be human observers who interpret these in terms of faking a left turn to create a false impression in the pursuant. In sum, the reinterpretation hypothesis comprises the following key claims:

1. Theory-of-mind-like behavior may be caused by mechanisms other than theory of mind.
2. Great ape behavior relies on such mechanisms, that is, they do not have a theory of mind.
3. When theory of mind did evolve, it did not result in a host of novel behaviors, but merely fine-tuned the existing behavioural mechanisms.

WHEN IS BEHAVIOR CAUSED BY THEORY OF MIND?

The reinterpretation hypothesis appeals to the introspectively plausible proposition that theory-of-mind-like behaviors may often not be caused by theory of mind, but only be reinterpreted, post hoc, in mental terms. But there is an alternative explanation (as Povinelli and colleagues acknowledged in a footnote to one of their expositions; Povinelli & Giambrone, in press). In humans, rapid social behavioral patterns (as in our football example) may only operate *after* they have earlier been consciously processed. In other words, the deception and counterdeceptions may have become automatic ("second nature") but only after a lot of practice involving explicit cognitive operations of a kind that Povinelli et al. are happy to consider true theory of mind. In general, the literature on human skill development is replete with examples where behaviors that were once governed by slow conscious processes become automatic with practice (e.g., Speelman & Maybery, 1998). Driving a car is at first a difficult, effortful, and conscious process but develops into automatic control with practice —freeing the mind to engage in other activities, such as having a conversation and reading advertisements that tell you what's on at the movies. Similarly, this may be true of many aspects of football where players' behavior becomes far more sophisticated, quicker and, yes, automatic as they get more experienced. It requires a lot of practice and explicit coaching to become a sophisticated player. Another classic example that does not involve physical skills of the type we kicked off with is chess. To begin, the novice has to compute each step and the likely specific responses—but the grand master can intuitively “see” whole strategies that to this extent have become automatic. Should theory-of-mind development also be regarded as skill acquisition, from effortful, explicit, controlled, and slow processing to, fast, automatic, noneffortful processing that comes with practice?

On the face of it, this seems to be a very plausible possibility. Initial understanding may develop, for example, on the basis of mental simulation: putting oneself in others’ (or one’s past or future self’s) shoes (e.g., Harris, 1991). Sufficient experience with this may then result in short cuts that allow for inference without explicitly mentally simulating experiences. In other words, people become *experts* and this is evident in more automatic processing and (possibly in addition) the ability to explicitly account for (reinterpret) the actions after the fact. Here we will not be able to discuss the pros and cons for the different theories of theory of mind acquisition. But it is worth noting that adult humans can engage in both instant (implicit) recognition and effortful, controlled simulation. We can quite automatically recognize that someone who was cheated is likely to seek retribution but we can also imagine what it must have been like for that person and more thoroughly appreciate that person’s perspective through the simulation called empathy (cf. Suddendorf, 1994).

Thus, appeal to the introspective observation that some behavior, although associated with theory of mind, may not be *caused* directly by second-order representations may not be immediately convincing as a fundamental critique. For example, the observation that we may follow gaze of others quite automatically may not result from some low-level non-theory-of-mind mechanism, but from sufficient experience and practice with these situations, resulting in automated response mechanisms. Thus, more generally, many theory-of-mind-like behaviors may result from: (a) mechanisms that are phylogenetically older and unaffected by theory of mind; or (b) mechanisms that have developed out of theory of mind but have become automated. One potential way in which one could differentiate between these possibilities is to look at whether the mechanism in question is informationally encapsulated. If social skills were to rest on type A mechanisms, then one would not expect that explicit reflection would interfere with these ancient low-level processes. Automatized mechanisms of type B, on the other hand, should be subject to optional top-down processes. That is, one can select the behavior (e.g., football or chess move) automatically, but certain situations may cause one to pause and explicitly think about what to do. Adult theory-of-mind-type behavior appears to be accessible in this latter way, which would support our automatization hypothesis rather than a reinterpretation account. In sum, we are not aware of any empirical reason why one should favor possibility A for these behaviors in human adults.

The case for the reinterpretation hypothesis perhaps could be rescued if one could argue that relevant behaviors that great apes show emerge first in human development *before* theory of mind is evinced. Could it be that children of age 2 or 3 have no real theory of mind, just sophisticated social mechanisms (as proposed for apes) that are only later reinterpreted in mental terms?

REINTERPRETATION ONLY WHEN FALSE-BELIEFS ARE UNDERSTOOD?

Some researchers have equated having a theory of mind with passing falsebelief tasks. False-belief tasks were originally conceived to establish that a subject "reasons" about the mind (Bennett, 1978; Dennett, 1978; Harman, 1978). In the case of true beliefs, reality and beliefs match, and hence, behaviors that result from cognitions that take true beliefs into account cannot easily be distinguished from behaviors that are simply responses to reality. In the false-belief task, however, subjects have to show that they understand that behavior is produced on the basis of beliefs about the world, even when these beliefs contradict what the subjects themselves believe to be true. Thus, success on the task is often said to evince an understanding of the representational nature of the mind. This ability is thought to involve *metarepresentation,* that is, representing representations *as* representations (Perner, 1991; Pylyshyn, 1978). Thus, perhaps it could be argued that younger children who systematically fail this task do not have the (metarepresentational) capacity to reinterpret behaviour in mental terms. So theory-of-mind-like behavior in these younger children may be based on the sophisticated social mechanisms the reinterpretation hypothesis claims to function without theory of mind in apes.

The false-belief task has become a standard tool for assessing theory of mind development and a recent meta-analysis (Wellman, Cross, & Watson, 2001) of 178 studies confirmed that there is a developmental trend from chance or below-chance performance to clear above-chance performance on such tasks from age 3 to 5. Of the many task variations, only motive (deception increases performance), salience (if protagonist's belief is clearly stated or pictured, then performance increases) and real presence (real world presence of the contradicting situation decreases performance) had a systematic effect on performance. Although several researchers have proposed early competence accounts, sug gesting that unnecessary aspects of the task mask young children's capacity, the meta-analysis did not find supportive age-condition interactions, showing instead that these conditions did not change the general trajectory (that is, they have equal effects for all age groups). The authors thus argue against early competence theories and instead suggest that a conceptual change occurs between ages 3 and 5.

There has been a strong research effort looking at the reasons why children younger than 3½ fail false-belief tasks. Arguably, however, this effort has somewhat missed the point of the task. The objective of the task was *not* to show that 3 year olds lack a theory of mind, or to discover whether acquisition of a theory is gradual or abrupt (Suddendorf, 1998a). Passing the task is informative, failing it may be less so. The reason that commentators to Premack and

Woodruff's paper, "Does the chimpanzee have a theory of mind?" suggested the false-belief task, was that passing it unequivocally establishes that the subject took the mental representation of the protagonist into account. It shows theory of mind. In spite of findings from Wellman et al.'s (2001) meta-analysis, failure on the task may result from any number of reasons other than lack of theory of mind. Bloom and German (2000) recently pointed out that theory of mind need not entail the ability to reason about false beliefs (e.g., one may hold a more primitive theory) and that succeeding on the task requires abilities other than theory of mind (e.g., understanding the test question). To repeat, younger children and animals that fail the task may do so for reasons other than lack of theory of mind. In fact, even if they were unable to appreciate false beliefs they may still have some form of theory of mind. Beliefs, after all, are not the only mental states and one may have ideas about, for example, desires or perceptions without understanding beliefs. This does not mean that the task is useless. Far from it. As originally conceived, when the task is reliably passed we can conclude that the individual has a theory of mind (Bennett, 1978; Dennett, 1978; Harman, 1978). If the task is failed, however, we can only conclude that we failed to reject the null hypothesis.

YOUNG CHILDREN'S THEORY OF MIND

Children may have some understanding of mind well before they pass falsebelief tasks, even if we do not have an unequivocal test for this. Studies have documented support for some, although limited, competence in reading emotion (Bartsch & Wellman, 1995), desire (Repacholi & Gopnik, 1997), ignorance (O'Neill, 1996), seeing (Flavell, Everett, Croft, & Flavell, 1981), intention (Meltzoff, 1995) and attention (Baldwin & Moses, 1994), to name but a few. Do these performances result from theory of mind or to other mechanisms?

Developmental psychologists differ in their accounts, but most do attribute some theory of mind to their young subjects. Let us briefly consider three of the most influential theories. Wellman and colleagues (Wellman et al., 2001) argue that even 2 year olds have a theory of mind, but a theory that is under constant revision based on feedback from the environment. Specifically, this account proposes that young children have a desire psychology (they reason about people's desires) that only later (indicated by false-belief competence) develops into a belief-desire theory of mind. Leslie and colleagues (e.g., Leslie & Thaiss, 1992; Scholl & Leslie, 2001) argue instead that even 2 year olds have an innate, operating *theory-of-mind module,* but insufficient *selection processing* (read: executive control) to pass false-belief tasks (because the default setting of the theory-of-mind module is thought to be for true beliefs). Finally, Perner (1991) proposed that 2 year olds have an intermediate representational capacity that allows them to construct a *mentalistic theory of behavior* (based on secondary representation) rather than a *representational theory of mind* (based on metarepresentation). What these approaches have in common is that they all acknowledge some form of theory of mind in children too young to pass false-belief tasks. Is there comparable evidence in great apes to extend such theories to our cousin species?

EVIDENCE FROM GREAT APES

Povinelli's own research has systematically investigated the capacities of chimpanzees and has been most prolific and influential in the area. In scores of studies, he and his colleagues subjected their group of young chimpanzees to tests of gaze following, pointing, understanding of "seeing" and "knowing," and the ability to distinguish intentional from accidental actions (e.g., Povinelli, Perilloux, Reaux, & Bierschwale, 1998; Povinelli & Eddy, 1996, 1997; Reaux, Theall, & Povinelli, 1999). Contrary to his earlier findings (Povinelli et al., 1990; Povinelli, Nelson, & Boysen, 1992), these extensive research efforts produced consistently negative results. That is, chimpanzees' performances on all these tasks suggested that they employed mechanisms other than theory of mind, which led the researchers to propose the reinterpretation hypothesis.

However, should we accept generalizations from the failures of this particular set of chimpanzees? Povinelli used the same group as subjects in numerous experiments. Are there reasons to doubt that he has tested a representative chimpanzee sample? Povinelli's subjects are all young (below age 10), and one may wonder whether we can generalize to older chimpanzees. This worry is a particular concern for his early studies when the subjects were juveniles. One may further wonder whether the subjects are otherwise atypical, perhaps unusually dense, impaired, or indifferent. Their artificial upbringing includes being raised in a peer group rather than by parents. Imagine Martians studying a group of human children raised as a group in a Martian enclosure. What generalizations about human intelligence could be drawn from research on these children? There is evidence for social traditions in wild chimpanzees and such traditions may be crucial to normal development of a chimpanzee mind. Generalizations from performances of Povinelli's chimps thus need to presuppose that the capacities under scrutiny are not affected by drastic changes to the normal developmental environment. We do not know whether these issues account for the data Povinelli reports, but they certainly are a threat to the external validity of the findings (see Allen, 2002; Hauser, 2001; Whiten, 2001, for further critical assessments of the generalizability of Povinelli's results).

Another potential problem is that, as part of their daily routine, these chimpanzees are being tested constantly in weird and wonderful ways. At the end of the day, however, their performance on any of the tasks may not matter to them too much—they will be fed anyway. Many of the ingenious experimental paradigms presuppose that the chimpanzee has a certain understanding of a rather unnatural situation. In many cases, for example, the chimpanzee needs to communicate with a human and appreciate that he (or she) wants to help her find food. Helping to find, rather than competing for, food is itself an atypical behavior for a chimpanzee. Therefore, although these studies are internally coherent and consistently fail to reject the null hypothesis, they do not close on whether the null hypothesis should be accepted for the chimpanzee population in general.

Work by Tomasello, Call, and colleagues has recently provided several positive results that question Povinelli's conclusions. First, these authors found evidence for quite sophisticated gaze following abilities in chimpanzees. Tomasello, Hare, and Agnetta (1999), for example, found that chimpanzees could project somebody else's line of sight geometrically beyond their immediate visual field. They did this even around barriers, moving in ways that allowed them to see what was behind them. They also followed gaze after having been distracted by an interesting object, which contrasts with the low-level hypothesis (Povinelli & Eddy, 1996) that chimpanzees simply look in the general direction the experimenter is gazing to find an interesting object or sight. Furthermore, they have also been observed "checking back" to the experimenter in cases of apparent uncertainty (Call, Hare, & Tomasello, 1998). Other studies suggest that chimpanzees and orangutans, like 2-year-old toddlers, can discriminate between accidental and intentional actions (Call & Tomasello, 1998; Povinelli, 1991).

Perhaps most important, chimpanzees were found to preferentially head toward seizure of whichever of two food items was visually screened from a more dominant competitor (Hare, Call, Agnetta, & Tomasello, 2000). Several control tests appear to rule out the possibility that the chimpanzees were using some kind of behavioral or other contextual cues. When the opaque screens were replaced with transparent screens, this preference for the "concealed" food vanished; the chimpanzee presumably recognized that the screen no longer blocked the visual access of the dominant competitor. This study of understanding "seeing" was then extended to investigate appreciation of "knowing." The results showed that chimpanzees also took into account whether a dominant competitor had earlier observed the baiting or was ignorant of the location of the food (Hare, Call, & Thomasello, 2001). These findings suggest a very different picture of chimpanzees' competence.[2]

Why should these new studies have produced positive results where other attempts failed? One possibility is that they appear to be more ecologically valid than earlier laboratory experiments (e.g., Povinelli & Eddy, 1996) in that they utilize the naturally strong motivation of competition with a conspecific. In previous attempts, as mentioned, chimpanzees had to communicate with humans, be trained extensively in particular ways, and often had to appreciate an unusual situation where a human had a cooperative motive (indicating where food is hidden) that runs against natural chimpanzee food competition. Whatever the reasons may be, these positive findings have more epistemological weight than previous failures to reject the null hypothesis.

These findings are also more consistent with observations both in the wild and in rich captive settings that indicate that great apes compute basic mental states such as attention and intention in their social dealings (see also Byrne & Whiten, 1990; de Waal, 1986; Whiten & Byrne, 1988). Does this mean that we should abandon the reinterpretation hypothesis and accept great apes into the circle of mindreading species? Hare et al. remain cautious in their interpretation (see also Call, 2001). They suggest that we should abandon the simple dichotomy between either having or not having a theory of mind (c.f., Suddendorf, 1998b, 1999; Suddendorf & Whiten, 2001; Whiten, 1996). Chimpanzees may occupy some middle ground in that they do have, as Hare et al. (2001) suggest, some limited understanding of, for example, "seeing" (level 1 perspective taking—knowing what others can or cannot see) without level 2 understanding (knowing how others see objects—e.g., from a different perspective) (Flavell, 1985). We agree with such an intermediate position (Whiten & Suddendorf, 2001). So our claim is that great apes and young children can entertain information about other minds that influences their behavior without necessarily having an understanding of beliefs sufficient for passing false-belief tasks. We have also recently made a broader claim about the nature of such an intermediate position.

THE MENTALITY OF APES

Utilizing the scheme of Perner (1991) referred to earlier, we recently reviewed the evidence for cognitive capacities in great apes and young children (Suddendorf & Whiten, 2001). We concluded that great apes have not yet been shown to be capable of the metarepresentation that characterizes the normal 4year-old child in Perner's scheme, but that there is more reason to believe they are capable of secondary representation. Let us first clarify the difference between *secondary representation* and *second-order representation* (see Suddendorf & Whiten, 2001, for more details). The term second-order representation was introduced to describe recursion in mindreading. "I think" is firstorder, '"I think that you think" is second-order, "I think that you think that he thinks" is third-order, and so on (Dennett, 1983). Secondary representation, on the other hand, does not refer to a level of recursion, but to mental representations that, in addition to primary representations of the reality, can reflect nonpresent states of the world (e.g., past, future, or hypothetical). Once capable of entertaining secondary representations one can collate and compare the current state of affairs with what was, what could be, and what might be the case. One of us therefore coined the term *collating mind* (Suddendorf, 1999).

Perner (1991) proposed secondary representation as the intermediate stage in his three-stage model of the development of representational capacities underlying theory of mind. Infants are believed to entertain single updating models of the world until in the second year they begin to additionally entertain hypothetical models; models that can refer not just to the present but to different entities, properties, times, and places. For example, toddlers begin to attribute desires to another person that are different from their own. In their early pretend play, they hold a primary representation of the true current situation and simultaneously a secondary representation of the pretend world. Around this age they also begin to understand hidden displacement (Piagetian stage 6 object-permanence), demonstrate means-ends reasoning, show empathic behavior, engage in simultaneous imitation games, interpret external representations, and recognize themselves in mirrors (see Suddendorf & Whiten, 2001, for a review). All these new displays are argued to be based on the ability to go beyond a direct model of the world and recombine elements of previous primary representations into past or novel (projected future or merely hypothetical) constellations (Asendorpf, Warkentin, & Baudonniere, 1996; Perner, 1991; Suddendorf, 1998b) and correlational findings in support of this have been reported (Asendorpf et al., 1996; Baudonniere, Margules, Belkhenchir, Pepe, & Warkentin, in press; Bischof-Köhler, 1989; Chapman, 1987; Lewis & Ramsay, 1999; Nielsen & Dissanayake, 2001).

Is there evidence for secondary representations in the great apes beyond the apparent mind-reading discussed thus far? Do they perform similarly to human toddlers in their second year? At least some data can be put forward for each of the domains (reviewed fully by Suddendorf & Whiten, 2001). We discuss them in turn.

Stage 6 Object Permanence

Hidden displacement tasks, where an object is invisibly displaced and a subject's search behavior is recorded, are supposed to measure the ability to mentally reconstruct the past trajectory of the target object. Although early research on chimpanzees (e.g., Mathieu & Bergeron, 1981; Wood, Moriatry, Gardner, & Gardner, 1980) and gorillas (Redshaw, 1978) suggested stage 6 capability in great apes, these studies have been criticized for lacking necessary control conditions (e.g., Natale & Antinucci, 1989). Subjects may simply have adopted a rule-based search strategy. Thus, success alone may not be regarded as evidence for stage 6 object permanence. However, careful use of appropriate control conditions has shown that at least one gorilla, in contrast to a macaque, has reached this level (Natale, Antinucci, Spinozzi, & Poti, 1986). Similarly strong evidence now is also available for orangutans (de Blois, Novak, & Bond, 1998). Chimpanzees, however, still need to be subjected to an appropriately controlled research paradigm. Nevertheless, given the earlier reports it seems likely that all great ape species can reach stage 6 object permanence.

Pretense

Pretend play is difficult to investigate experimentally. Observational reports of pretence in great apes, however, have become more frequent. Home-reared members of all great ape species have been reported to treat dolls and toys as if they were animate characters (e.g., bathing them), sometimes even employing sign language to convey the pretend processes (e.g., Jensvold & Fouts, 1993;

Patterson & Linden, 1981; Savage-Rumbaugh, 1986; Savage-Rumbaugh & McDonald, 1988; Tanner, 1985). The chimpanzee Viki reportedly acted as if she was pulling a toy on a string and then followed through with the pretend implications when the pretend string appeared to be stuck on a real object (Hayes, 1951). It is, of course, inherently difficult to judge if a behavioral display in the wild involves any make believe. However, Wrangham (1996) reported that a subadult chimpanzee traveling with his pregnant mother, picked up a log, carrying it for hours and treating it suspiciously like a baby (e.g., he made a nest and placed the log in it). A more adaptively valuable expression of this skill may be in the realm of deception, where a perpetrator acts *as* if doing something that must be in contradiction with their primary perception of the situation. Whiten and Byrne (1988) collated such cases, in which, for example, an individual was observed to notice some half-hidden choice food, yet when a competitor appeared they inhibited their attention to the food, only looking at it again and retrieving it once the competitor departed. Such records were logged only for great apes (chimpanzee and gorilla). Although this suggests that pretence is within great apes' potential, it contrasts with 2-year-old children, in that it appears to be relatively rare, rather than an obvious everyday manifestation.

Means-Ends Reasoning

When problem solving involves keeping in mind a goal state while mentally working through subgoals, then this may be regarded as means-ends reasoning. Chimpanzees have been shown to solve tasks that appear to involve such reasoning as when they are required to make the right initial choice on the basis of foreseen consequences (Döhl, 1966, 1968, 1969; Rensch & Döhl, 1968). Köhler's *The mentality of apes* (1927) reported experiments that suggested that chimpanzees can solve problems by mental processes rather than mere physical trial and error. Visalberghi's (1996) findings on tool use support the idea that chimpanzees can understand means-ends relationships at least in some cases without recourse to trial and error. All these studies, however, also reveal inconsistencies and individual differences that have dampened some researchers' enthusiasm (Povinelli, 2000). Povinelli concludes from the failure of his chimpanzees that they cannot take unobservable forces into account.

Again, ecologically valid tasks may be most likely to elicit behavior that reflects great apes' potential. Group-specific tool-use traditions have been reported in wild chimpanzees (e.g. Boesch & Boesch, 1984; Goodall, 1986; Whiten et al., 2001) and orangutans (Fox, Sitompul, & van Schaik, 1999; van Schaik, Fox, & Sitompul, 1996). These apes use different tools for different tasks (Whiten et al, 1999) and modify them appropriately for the task at hand (McGrew, 1992), suggesting a high degree of flexible anticipation in relation to goals. Gorillas are not known to use tools in the wild, but tool use in captivity includes comparable tool modification and application (Boysen, Kuhlmeier, Halliday, & Halliday, 1999; see Parker, Kerr, Markowitz, & Gould, 1999, for a review).

Understanding External Representation

Members of all great ape species have been reported to enjoy watching pictures and television (e.g., Patterson, 1991; Premack & Woodruff, 1978; Savage-Rumbaugh, 1986). They also do not seem to have problems interpreting photographs appropriately (Jensvold & Fouts, 1993; Miles, Mitchell, & Harper, 1996; Patterson, 1978). The home-reared chimpanzee Viki, for example, was able to match photographs to real objects, suggesting that she understood what the picture represented (Hayes & Hayes, 1953). Sherman, a male chimpanzee who spent his early years with human female caretakers (e.g., Savage-Rumbaugh, 1986), displays interest in photos of human females, especially when wearing fur, and has become sexually aroused by them to the point of ejaculation (AW, personal observations). Now, such behavior may simply result from the image involving sufficient cues to elicit certain behavioral responses. To ascertain that an individual has understood that one object can inform about another (representational insight), developmental psychologists use a search paradigm (e.g., DeLoache, 1987). For example, a photo shows the location of an object in another room and the child is asked to search for that object after viewing the picture (or video, drawing, scale model, etc.). Kuhlmeier, Boysen, and Mukobi (1999) have recently shown that at least some chimpanzees can pass such a task. Great apes have not invented external representations in the wild, but their representational capacities appear to enable them to interpret and utilize human symbols.

Mirror Self-Recognition

It is established that some members of all genera of great apes can pass the classic mirror self-recognition task (e.g., Gallup, 1970; Parker, Mitchell, & Boccia, 1994; Povinelli et al., 1997; Povinelli, Rulf, Landau, & Bierschwale, 1993; Swartz, Sarauw, & Evans, 1999). Alternative interpretations such as the anesthetic recovery hypothesis (Heyes, 1994) have not held up to further scrutiny (van den Bos, 1999). Chimpanzees and orangutans have provided the most convincing performance (Boysen, Bryan, & Shreyer, 1994; Calhoun & Thompson, 1988; Gallup, 1970; Gallup, McClure, Hill, & Bundy 1971; Hill, Bundy Gallup, & McGlure, 1970; Lethmate & Dücker, 1973; Lin, Bard, & Anderson, 1992; Miles, 1994; Povinelli et al., 1993; Suarez & Gallup, 1981; Swartz & Evans, 1991). Early studies with gorillas were unsuccessful (e.g., Ledbetter & Basen, 1982; Suarez & Gallup, 1981), but more recent research has provided positive results (Parker, 1994; Patterson & Cohen, 1994; Swartz & Evans, 1994). Reviewing the research to date, Swartz, Sarauw, and Evans (1999) report that 43% of chimpanzees, 31% of gorillas, and 50% of orangutans tested with the classic mirror mark test have passed. Failure may result from any number of reasons ranging from differences in motivation to differences in age. The research clearly shows that MSR is within the capacity of great apes.

Empathic Responding

Comforting conspecifics other than one's own offspring, is rare in animals but has been reported for chimpanzees (Boesch, 1992; de Waal, 1996; Goodall, 1986). Special care is given to conspecifics that are distressed or injured (de Waal, 1996). De Waal and Aureli (1996) analyzed over a thousand spontaneous aggressive incidents in a group of chimpanzees and found that bystanders' kissing, hugging, grooming, and gentle touching were provided significantly more often to victims of aggression, immediately after fights and more after serious than after mild conflicts. The other great apes have not been reported to show such consoling behavior.

Understanding Imitation

Great apes have shown some ability at imitation as a way of learning and exploring (Russon & Galdikas, 1993; Stoinski, Ure, Wrate, & Whiten, 2000; Whiten, 1998). But do they understand imitation in the way a human toddler does? Some great apes appear to have some grasp of the concept of imitation. For example, they have been taught to play a do-as-I-do imitation game (Custance, Whiten, & Bard, 1994; 1995; Hayes & Hayes, 1952; Miles et al., 1996; Tanner & Byrne, unpublished). However, there appear to be no formal studies that have investigated whether apes can recognize when they are being imitated, as children come to do in the second year of life, although some observations suggest that this may be within their capability (Miles et al., 1996).

TABLE 8.1. Strength of Reviewed Evidence for Each Realm

Expression of Secondary Representation	Chimpanzee	Gorilla	Orangutan
Hidden displacement	*	**	**
Pretence	*	*	*
Means-ends reasoning	**	*	**
External representation	**	*	*
Mirror self-recognition	***	**	***
Empathic responding	*		
Recognizing mental states	**		**
Understanding imitation	*	*	*

*Some evidence (observational or weak experimental).
**Stronger evidence (experimental).
***Undisputed evidence.

Taken together, then, the current data suggest that these achievements characteristic of humans' cognitive development in the second year are within the capacities of our great ape cousins (Table 8.1). If it is correct that all of them are related to the emergence of the ability to entertain secondary representations, then it seems reasonable to propose that, as a working hypothesis at least, the great apes share this ability. Our working hypothesis thus predicts that chimpanzees *(troglodytes and pansicus),* orangutans, and gorillas will all be shown to have skills in all those realms. Furthermore, it predicts correlations between performances in these domains (e.g., between mirror self-recognition and empathic behavior) similar to those found in toddlers.

This working hypothesis is also the most parsimonious explanation when one considers the phylogenetic relationship of the species under investigation. The great apes shared a common ancestor approximately 16 million years ago. When all species of this group display the same behavioral phenotype, it is more parsimonious to assume that these features are homologous (that is, that they originated in a common ancestor), than to conclude that each of them evolved it independently. The latter explanation requires more assumptions than the former. Furthermore, if those capacities discussed in the preceding are the product of a common history, then it is also likely that they are based on the same underlying psychological processes (Byrne, 1995; de Waal, 1996; Parker, 1996; Suddendorf, 1998b, 1999). This reasoning may be best described as the argument by homology (in contrast to the criticized "argument by analogy," see Suddendorf & Whiten, 2001).

Although toddlers and great apes may share some fundamental cognitive capacities, we are, of course, not saying that they are mentally equivalent. Apes, for example, do not display protodeclarative pointing. Note especially that toddlers display evidence for all of the reviewed abilities quite frequently in their everyday activities (e.g., pretend play), whereas great apes, on the other hand, may display them only sporadically or weakly; in natural conditions they may be truly functional only in a subset of the domains. One may be concerned that such differences in the way and frequency by which the discussed characteristics are being expressed in each species undermines our parsimony argument. (Would one not need to make additional assumptions about the evolution of these differences?) Our argument is aimed at the basic capacity to entertain secondary representations—that is the homology. The expression of this basic capacity is influenced by other factors within and between species. Nor do we suppose that the ontogenetic and phylogenetic parallels, such as they are, represent any simple principle of "recapitulation." Perhaps the human propensity owes to the fact that it is a stepping stone toward more sophisticated cognitive developments, whereas this may be the outer limit of what great apes have evolved to do.

The other obvious difference is language. Language may be causally linked to the development of theory of mind (Astington & Jenkins, 1999). Indeed, language is ideally suited for explicating mental states, for teaching about mental states and to interact purely on a mental level (as in asking questions). As Dennett (2000) pointed out, chimpanzees never have the opportunity to compare notes on their mind-reading activities, if indeed they had them. Language appears to be necessary not only for public exchanges about the mind, but also to facilitate internal explicit representations of mental states. Of interest here is recent research we discuss next, which suggests that children may understand false beliefs first in action and only later in words.

IMPLICIT UNDERSTANDING OF THE MIND?

Even 3 year olds tend to look in the correct direction in false-belief tasks while continuing to explicitly select the wrong option (Clements & Perner, 1994; Garnham & Perner, 2001). This correct responding is not restricted to visual orienting, but also manifests in other action. When a character holding a false belief is about to use what amounts to the wrong one of two slides, even 36 month olds tend to place a mat for safe landing at the bottom of the appropriate slide (the one the protagonist falsely believes to be the correct slide) when asked to act quickly. Explicit verbal responses of these children, however, show the classic error: When asked which slide the protagonist will come down on, they fail to attribute a false belief (Garnham & Perner, 2001). The researchers went on to test various alternative explanations (e.g., lack of confidence; misinterpretation of question) as to why young children do not express their apparent knowledge in the explicit tasks. They found no support for these alternatives and instead argued that children's behavior is more akin to implicit processing (as in blindsight, where implicit visual perception exists without conscious awareness, inaccessible for deliberation and declaration) (Perner & Clements, 2000).

Perhaps this approach may offer an alternative avenue to adapt the reinterpretation hypothesis. Rather than explicit processing becoming automated, as discussed, Perner's research outlined in the preceding section suggests that initial implicit processing becomes progressively more explicit. Such an account is in line with Karmiloff-Smiths (1993) hypothesis of representational redescription, which proposes development of capacities to progressively recode implicit information in more explicit formats during development. Indeed, Povinelli and colleagues (Povinelli et al., 2000) view their reinterpretation model as an evolutionary analog to this developmental account. Given the data on early implicit false-belief understanding, one may advance a version of the model that postulates that great apes possess some implicit theory of mind (rather than no theory of mind) that our ancestors then progressively made more explicit (perhaps aided by the emergence of language).

The challenge is thus to design nonverbal implicit false-belief tasks for great apes that should generate positive findings if the preceding notions are correct. To increase one's chance, we might suggest making the subjects active participants in a deceptive ploy (to use those conditions shown to be easiest in children; see Wellman et al., 2001) and to create a motivating competitive situation (to use those conditions shown to be most successful in chimpanzees; see Hare et al, 2001).

The bottom line is that the debate about whether or not the chimpanzee has a theory of mind is still open, even if the likely possibilities have been narrowed down. At present there is no reason to favor the idea that great apes do not have a theory of mind over the possibility that they do have a limited (perhaps, implicit) one.

ADAPTIVE ADVANTAGE

Why would the capacity for (an explicit) theory of mind have evolved? At first sight, it seems obvious that evolution of theory of mind would have had great social ramifications and selective advantages. In children, passing the false-belief task is associated with enhanced social competence (Lalonde & Chandler, 1995) and modern-day hunter-gatherers use their deep social minds for crucial cooperative tasks; theory of mind was likely instrumental in the evolution of early human culture (Whiten, 1999).

The reinterpretation hypothesis, however, argues that theory of mind need not have resulted in radically new behaviors. Instead, it is suggested that its emergence may have had only subtle advantages in reorganizing existing behavioral propensities. This is the third key claim of this hypothesis. It is probably the result of the nagging difficulties Povinelli and other researchers encountered in their attempts to identify a simple discriminating behavioral correlate of theory of mind. And the point that whatever hominids evolved would have been built on, and interwoven with, the extant capacities is well taken (although some scholars seem to believe that many whole new modules were added from scratch). However, the question why humans evolved theory of mind (or why they began to explicitly reinterpret or redescribe their implicit knowledge) remains in the realm of speculation.

There are certainly viable alternatives to the reinterpretation account. One obvious candidate that needs consideration concerns language. Language, in itself a deeply social phenomenon, might have fostered the evolution of an explicit theory of mind. As pointed out, language is exceptionally well suited for mind-reading, declaration of mental states, teaching, and exchanging notes. Syntax, often hailed as the distinguishing aspect of human language, is also a prime example of the recursion that is so typical of human generativity, and recursion is also evident in human mind-reading (see Corballis, this volume).

Of course, one can argue the case both ways. Language may have fostered theory of mind evolution, but elaborate language evolution may have been possible only after the emergence of theory of mind. (How would one agree on a symbolic system to exchange ideas between minds, if no one believes the other has a mind?) The latter version of the argument, of course, implies that theory of mind has had a huge impact on the behavioral repertoire: It has made complex language possible. A more modest account simply acknowledges that there might have been coevolution of both language and theory of mind, facilitating each other's complexity. Unfortunately, the suspicion of a causal link to language evolution does not help us much in terms of the timing of the evolution of explicit theory of mind. In spite of the recent flurry of theorizing (since the apparent abandonment of earlier self-restraint, such as the 1866 ban of such speculation by the Paris Linguistic Society), the origin of language remains a mystery. There are influential accounts in both, "early-bloomer" and "latebloomer" camps (see Suddendorf, 2000, for a discussion).

Finally, it is also possible that theory of mind is something of a byproduct of a different kind of adaptive evolution. Elsewhere, one of us argued that passing false-belief tasks is just one expression of a general shift in cognition (i.e., the emergense of *metamind*) occurring in children between age 3 and 4 involving advances in executive control and metarepresentational capacity (Suddendorf, 1999). These advances appear to have consequences not only in the social realm, but also in regard to how one's own mind is utilized. For example, divergent thinking in search of alternative problem solutions has been found to be strongly associated with false-belief understanding (Suddendorf & Fletcher Flinn, 1999), and the adaptive advantage of solving problems needs no further elaboration. A related possibility is that the processes that allow for explicit theory of mind may also allow an individual to travel mentally in time (Suddendorf, 1994; Suddendorf & Corballis, 1997). This proposal turns out to be not all that new. In 1805 William Hazlitt wrote

> The imagination by means of which alone I can anticipate future objects, or be interested in them, must carry me out of myself into the feelings of others by one and the same process by which I am thrown forward as it were into my future being, and interested in it. (1805, p. 1)

Perhaps understanding other minds is merely a byproduct of the obviously adaptive evolution of a concern for ones own future being (Suddendorf & Corballis, 1997). To be sure, there are no doubt plenty of other interesting possibilities (the explosion of cooperation in hominids, for example, could be intimately linked to the evolution of theory of mind; Sterelny, 2003). These examples, however, should suffice to illustrate that this aspect of the reinterpretation hypothesis is, at best, an educated guess (as are the other proposals) and that there are many human behavioural facets that are unique and may be causally linked to theory of mind.

In conclusion, the three key claims of the reinterpretation hypothesis that we looked at more closely are possibilities worth considering, although at present we see no reason to favor them over viable alternatives:

1. *Human theory-of-mind-like behavior may not be caused by theory of mind.* However, this impression may be the result of humans becoming theory-ofmind experts, with automatized mechanisms.
2. *Great apes do not mind-read.* However, there are positive results that suggest some intermediate theory of mind capacity.
3. *Theory of mind did not result in a host of novel behaviors.* However, unlike apes, we talk and play football.

NOTES

1. Frans de Waal describes an elaborate example in which a male chimpanzee aggressively chased a female. As the female sought cover behind a tree trunk, the male moved to the left, which prompted the female to move to the right. In full motion, the male then threw a brick in the direction of her path (to the right), while continuing to move to the left himself. To avoid the projectiles the female changed directions only to find herself being caught by the male.
2. Further preliminary reports are emerging that appear to substantiate these results (Boysen, 1998; Whiten, 2000).

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9

Does the Selection Task Detect Cheater-Detection?

DAN SPERBER

VITTORIO GIROTTO

Evolutionary psychology—in its ambitious version well formulated by Cosmides and Tooby (e.g., Cosmides & Tooby, 1987; Tooby & Cosmides, 1992)—will succeed to the extent that it causes cognitive psychologists to rethink central aspects of human cognition in an evolutionary perspective; to the extent, that is, that psychology in general becomes evolutionary. The human species is exceptional by its massive investment in cognition, and in forms of cognitive activity—language, metarepresentation, abstract thinking—that are as unique to humans as echolocation is unique to bats. The promise of evolutionary psychology is thus to help explain not just traits of human psychology that are homologous to those of many other species, but also traits of human psychology that are genuinely exceptional and that in turn help explain the exceptional character of human culture and ecology.

However, most of the work done in evolutionary psychology so far is on aspects of human psychology that are not specifically human except in their details. Showing, for instance, how human preferences in mate choice are finetuned in the way the theory of evolution would predict is of great interest (see e.g., Buss, 1994) but it can be done on the basis of a relatively shallow psychology.

This makes work on distinctly human adaptations involving higher cognition of particular importance for defenders of a psychologically ambitious evolutionary psychology What is often presented (e.g., Pinker, 1997) as the signal achievement of cognitive evolutionary psychology in this respect is the experimental testing of Cosmides' (1989) hypothesis that there exists an evolved competence to deal with social contracts, and, in particular to detect cheaters. We want to argue that, because of faulty methodological choices—the quasi-exclusive reliance on the four-cards selection task—the hypothesis has in fact not yet been tested.

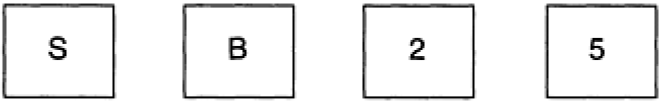
The plan of this chapter is as follows: We begin with a short presentation of Cosmides's social contract hypothesis, Wason selection task, and Cosmides' reasons to use the task in order to test the theory. In a second section, we present the relevance-theoretic analysis of the selection task proposed by Sperber, Cara, and Girotto (1995), which cast doubts on the appropriateness of the task to study reasoning. In a third section, we present Fiddick, Cosmides, and Tooby's (2000) defense of the use of the selection task as a tool to test evolutionary theories of reasoning, and argue that it is methodologically flawed. In a fourth section, we present three experiments designed to test contrasting predictions deriving from the two approaches. In the conclusion, we come back to Cosmides 's hypothesis and reflect on how it might be really tested.

SOCIAL CONTRACT THEORY AND THE SELECTION TASK

The Theory

Cosmides' (1989) argued that, for cooperation to have stabilized among humans in evolutionary time, and not to have been made impossible by excessive free-riding or "cheating," humans must have evolved domain-specific cognitive capacities that allowed them to detect cheaters. She argued that the cognitive capacities in question consisted in a "social contract algorithm" specialized in reasoning about social contracts and allowing, in particular, to detect parties that were not abiding by the terms of the contract. It might be argued that cheater detection need not to be achieved by a mechanism so calibrated. It could have been that the solution to the problem of free-riding was already provided by other, more general cognitive mechanisms, or that such more general mechanisms were the best solution available to biological evolution at the time, even if the pressure then was just to solve this particular problem. It could be that cheater detection is achieved by several more specialized or diversely specialized mechanisms acting conjointly, and so on. Still, the hypothesis that there might be a cognitive mechanism calibrated to deal precisely with a problem that caused a specific selection pressure is, arguably, the best point where to start, the first hypothesis to test.

In nonhuman cases, cheating consists in failing to reciprocate in a standard kind of exchange that is part of the behavioral repertoire of the species. The ability to detect cheaters boils down to that of detecting quite specific behav ioral features. For instance, in the famous case of vampire bats, detecting cheaters seems to be done by detecting individuals who, in spite of having a distended stomach, fail to regurgitate for the benefit of others some of the blood they have swallowed (Wilkinson, 1984). In the human case on the other hand, there is an endless variety of forms or reciprocal interactions based on explicit or implicit contractual agreements. Therefore the detection of cheating is a cognitive challenge that cannot be based on simple behavioral cues and that must rely on an understanding of the contracts involved.



The following rule applies to these four cards and may be true or false: “If there is an A on the one side of a card, then there is a 2 on the other side.”
Which of these cards would you need to turn over to decide whether the rule is true or false?

FIGURE 9.1. The standard version of the Wason selction test.

Cosmides (1989) analyzed contracts as involving the commitment to meet a given requirement and the entitlement to receive a given benefit, and cheating as taking the benefit without meeting the requirement. No doubt, we would be surprised if we found nonhuman animals capable of deploying such a complex abstract notion. Humans, however, use and deploy in verbal communication a great variety of complex notions, many of them without particular evolutionary significance. Are the concepts of a social contract and that of a cheater exceptionally complex, or are they used with much greater ease than equally complex concepts? Does their effective deployment require ad hoc domain-specific capacities? Cosmides believes that they are and set out to demonstrate this experimentally. Before formulating criticisms of Cosmides experimental approach, let us point out how innovative it was. She brought together evolutionary considerations with the most intensively used experimental paradigm in research on reasoning, the Wason (1966) selection task. Whereas much work with the selection task had modest theoretical aims (if any), she used the task for a novel and ambitious theoretical purpose.

The Task

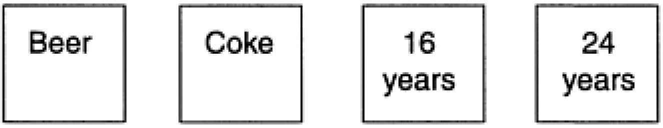
The standard version of the Wason selection task reads as follows.

> There is a deck of cards, all of which have a letter on one side and a number on the other side. Four cards are taken from the deck and placed on the table. You can see the four facing sides as follows:

In a typical deontic version, the selection task reads as follows (see Griggs & Cox, 1982).

> Imagine that you are police officer on duty. The cards in front of you have information about four people sitting in a bar. On the one side of a card is a person’s age, and on the other side is what a person is drinking:

As these examples illustrate, participants in a selection task are presented with a conditional rule of the form If an item has the feature P, then it has the feature Q (descriptive versions) or If an item has the feature P, then it should have the



Which of these cards would you need to turn over to decide whether these people have obeyed the rule “If a person is drinking beer, then the person must be over 18 year of age?”

FIGURE 9.2. A typical deontic version.

feature Q (deontic versions), and with four cards representing each an individual item. Only half of the information these four cards contain is visible, showing that the four items represented have respectively the P, not-P, Q, and not-Q feature. (The cards are accordingly called the P, not-P, Q, and not-Q cards.) The full information can be made visible by turning over the card in order to find out whether or not the P and not-P cards also have feature Q, and whether or not the Q and not-Q cards also have feature P. Participants are asked which cards it is necessary to turn over to determine whether the rule is true or false (descriptive version) or obeyed or disobeyed (deontic versions). Because, as far as these four cards are concerned, the rule is true (or obeyed) unless there are items combining the P and the not-Q features, the logically correct selection is that of the P and of the not-Q cards, either of which could turn out to provide a counterexample to (or a violation of) the rule. However, with standard descriptive versions, more than 80% of the participants fail to solve the problem (and tend to select the P and the Q cards, or just the P card). With standard deontic versions, on the other hand, more than 50% of the participants make the correct P and not-Q selection (for a recent review, see Manktelow, 1999).

Using the Task to Test the Theory

What does this have to do with cheater detection? Following Cosmides’ (1989) analysis, what you need to know, in order to determine whether individuals who have entered into a contract are cheating is whether or not they have “taken the benefit,” and whether or not they have “met the requirement” involved. These features can be treated as special cases of the P and Q of the selection task.

Cosmides then built deontic versions of the selection task where the cards represented individuals, and where features P/not-P were of the form “has/has not taken the benefit” and features Q/not-Q were of the form “has/has not met the requirement” (with the specific benefit and requirement mentioned on the cards determined by the narrative introduction to the task). Cosmides showed that most participants presented with such a task and asked who, among the four individuals represented, might be in violation of a rule of the form “if you take the benefit, then you must meet the requirement” correctly choose the individual known to have taken the benefit (and who might not have met the requirement) and the individual known not to have met the requirement (and who might have taken the benefit), that is, the two possible cheaters. For example, in one of her selection task scenarios, the male members of a tribe were described as seeing eating cassava root, an aphrodisiac, as a desirable benefit, but as having for this to meet a requirement: to have a tattoo on their face. When presented with the rule "If a man eats cassava root, then he must have a tattoo on his face" and four cards representing men of the tribe and indicating respectively "is eating cassava root" (P card), "is eating molo nuts" (not-P card), "has a tattoo on his face" (Q card), and "has no tattoo on his face" (not-Q card), most participants correctly selected the P and the not-Q cards. Other researchers have found similar results with a variety of deontic rules, and with both adults and children participants (e.g., Cheng & Holyoak, 1985; Gigerenzer & Hug 1992; Girotto, Light, & Colbourn, 1988; Manktelow & Over, 1991).

Cosmides (1989), however, argued that not all deontic rules elicit correct selections, but only those that are processed by means of an underlying evolved algorithm such as the social contract algorithm. She concluded that whereas people do not have the general logical abilities required to solve the Wason selection task in every version, and not even in every deontic version, they have a specialized ability to reason about contracts, and in particular to identify cheaters.

To evolutionary psychologists who are otherwise unfamiliar with the psychology of reasoning, this experimental work may seem like a brilliant empirical confirmation of sound theoretical claims, but the fact is that, for good or bad reasons, specialists of reasoning in general and of the selection task in particular have not been swayed (for a notable exception, see Gigerenzer & Hug, 1992). Among the bad reasons may have been hostility to evolutionary approaches. Among the good reasons is the fact that these data are open to a variety of interpretations, among which Cosmides' (1989) own is not uniquely compelling (as pointed out by Atran, 2000). In general, no selection task data have ever been seen as compelling enough to convince the scholarly community of any theoretical claim about human reasoning. The task seems to be used again and again, almost compulsively, not because it ever provides conclusive evidence on anything theoretical, but because, on the contrary, it lends itself to endless controversies.

IS THE SELECTION TASK AN APPROPRIATE TOOL FOR STUDYING HUMAN REASONING?

Sperber, Cara and Girotto (1995) (henceforth SCG) have argued that the selection task is altogether an inappropriate tool for studying human reasoning (whether domain-general or domain-specific) because in all version of the task, people tend to be guided not by any form of reasoning but by context-sensitive intuitions of relevance (see also Evans, 1989). Intuitions of relevance are activated by the pragmatic mechanism involved in comprehending the task (just as they are by any comprehension process). People trust these intuitions and select cards accordingly. Different wordings and contexts yield different intuitions and therefore different selections.

SGG's approach is in no way hostile to evolutionary psychology. In fact, the relevance-guided comprehension mechanism involved in the selection task is viewed as an evolved module specialized for the comprehension of communicative intentions, and more specifically as a submodule of a theory-of-mind mechanism (Sperber, 2000; Sperber & Wilson, 2002). This specialized comprehension module preempts any downstream domain-general or domain specific reasoning mechanism people might have otherwise appealed to, including a cheater detection module, if there is one.

What happens, SCG claimed, is that, in standard descriptive versions of the task, the intuitions provided by the comprehension module are misleading, whereas in most deontic versions, including those used by Cosmides (1989), they point to the correct selection. SCG discussed how to manipulate content and context and thereby affect comprehension, intuitions of relevance, and selection of cards. They provided and tested a recipe to construct versions of the task where a majority of participants would give the logically correct response. The recipe aims at making the P-and-not-Q cases contextually more relevant than the P-and-Q cases. For this, all you have to do is the following.

1. Choose P and Q so as to render the P-and-not-Q cases easier to mentally represent than the P-and-Q cases.
2. Create a context where knowing whether there are P-and-not-Q cases would have greater cognitive effects than knowing whether there are P-and-Q cases.
3. Present the rule "if P, then Q" in a pragmatically felicitous manner.

SCG argued that all previous genuine versions of the selection task where a majority of participants had given a correct response happened to conform to this recipe. They produced several descriptive versions of the task that elicited a higher percentage of correct responses than had ever been found before with such versions.

Consider as an illustration the following problem.

> Imagine that you are a journalist and that you are preparing a piece on the small principality of Bagurstan. The reigning Prince studied Economics in Oxford and has imposed a radical form of liberalism upon his country. In Bagurstan, people retire at 65, students get a salary, but they have no social security, no right to work, no unemployment benefits, no minimum salary, no maternity leave. Yet, the Prince claims that in Bagurstan there are no serious economic or social problems. Economic mechanisms alone allow everyone to find a satisfactory solution. For instance , the Prince asserts, in my country, if a person is of working age, then this person has a job

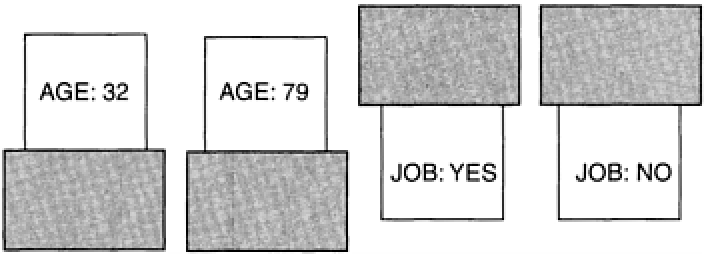


FIGURE 9.3. Information cards on four Bagurstani civil servants.

> Before you, on the desk of the Bagurstani civil servant you are interviewing, are four information cards corresponding to four Bagurstani subjects. The top part of each card indicates the age. The bottom part indicates whether the person has a job. Unfortunately, some papers conceal the bottom of two of these cards, as well the top of two other cards. Which cards should be uncovered to determine whether what the Prince says *(if a person is of working age, then this person has a job)* is true, at least as far as these four Bagurstani are concerned.

This version of the task does follow the relevance recipe: The conditional statement is uttered in a context in which it is likely to be understood as a denial of the existence of people of working age and without a job. Moreover, such a combination of features is easily representable, given that it is a highly salient and lexicalized category ("unemployed"). As predicted by SCG, most participants (70%) solved the task, by correctly selecting the individual of working age (and who might not have a job), i.e., the P card, and the individual with no job (and who might be of working age) i.e., the not-Q card.

By contrast, only 25% of the participants solved a control condition identical in every respect except that the Princes assertion was, "Of course, if a person is older than 65, then this person is without a job." With such a conditional rule, the problem does not conform to the recipe anymore: The case of a person older than 65 and with a job, a combination of features that is neither salient nor lexicalized, is not easier to represent than that of a person older than 65 and without a job, who normally falls in the well-known category of retired people. Moreover, the Prince's statement is understood as granting that there are people without a job in his country, namely people of retirement age, and not as denying that there might be a few people of that age that are still working.

Girotto, Kemmelmeir, Sperber, and van der Henst (2001) showed how deontic versions could equally well be manipulated so as to obtain at will correct selections or incorrect selections (of a type usually found with descriptive version, i.e., P and Q selections). In a within-subject design, these authors used the very same rule four times with different contexts, twice descriptively, and twice deontically. The rule was: "If a person travels to any East African country, then that person must be immunized against cholera." In the first deontic condition, participants were informed of the existence of such a rule and asked which cards it was necessary to turn over in order to check whether clients of a travel agency represented by four cards (with their country of destination on one side and their immunization on the other) had obeyed the rule. In the second deontic condition, participants were told that the very same rule was not in force anymore and were asked to check whether misinformed clients of the agency had obeyed it nevertheless. When the rule was described as being in force, 62% of the participants selected the usual P and not-Q cards. When the rule was described as not being in force anymore, only 15% of the participants selected the logically correct P and not-Q cards, whereas 71% chose the P and Q cards, representing people who might have obeyed the rule.

Findings similar to those of SCG have been reported (e.g., Green & Larking, 1995; Hardman, 1998; Liberman & Klar, 1996; Love & Kessler, 1995). Although alternative or complementary interpretations of SCG's data have been proposed (see Johnson-Laird & Byrne, 1995; Oaksford & Chater, 1995) the only extensive challenge of SCG based on experimental data is the article by Fiddick, Cosmides, and Tooby (2000) (henceforth FCT). In the following sections, we discuss this challenge in detail.

EXAMINING FIDDICK, COSMIDES AND TOOBY'S DEFENSE OF THE USE OF THE SELECTION TASK AS A TOOL TO TEST EVOLUTIONARY THEORIES OF REASONING

The Principle of Preemptive Specificity

FCT properly note that SCG's proposal

> is not intended to deny that evolved domain-specific inference mechanisms, such as social contract algorithms, exist, or that they play an important role in mental life. […] The authors simply argue that, in explaining content effects on the Wason selection task, once relevance effects are subtracted, either (1) there is nothing else to explain, or (2) relevance effects inevitably confound experiments, rendering the Wason task useless as a means of detecting the operation of content-specialized inference mechanisms. (p. 4)

FCT then go on to try and defend their use of the selection task against SCG deflationary methodological claims. They do so by means of a variety of theoretical arguments and new experimental results.

FCT's main theoretical argument is based on what they call the "principle of preemptive specificity" according to which

> human cognitive architecture should be designed so that, when two or more alternative inference systems are activated in a competing manner by the same input, the more specialized system will tend to override the more general one, dominating performance for that input. (FCT, p. 5)

They agree that a relevance-based comprehension mechanism may well handle the selection task problem in the general case, when no more specific mechanism is available. But in the cases where the Wason task can be interpreted as a cheater detection problem, then the more specialized cheater detection mechanism should preempt the more general comprehension mechanism.

The "principle of preemptive specificity" is an interesting and plausible hypothesis about cognitive architecture, but does it apply here? More specifically, are the comprehension module and the cheater detection module activated in a competing manner by the same input? As we pointed out, a human cheater detection mechanism must be activated not just by perception of the behavior of possible cheaters but also by a conceptual representation of the contractual relationship between the parties. In Cosmides' selection tasks, participants must comprehend the text of the problem in order to have access to information about the social contract involved. In other words, comprehension processes will anyhow take place before the task can be recognized as a cheater detection one, if it ever is. SCG's claim was that, because of the general structure of the selection task, mere comprehension of the problem automatically provides participants with intuitions of relevance about the cards. Because the question asked is about the very relevance of the cards, participants have in mind what feels like an answer to the problem before any further reasoning mechanism (whether a general reasoning ability, if there is one; or a cheater detection module, if there is one) gets fed the terms of the problem. Contrary to FCT's argument, the comprehension mechanism and the cheater detection mechanism cannot be "activated in a competing manner by the same input" because the input of the cheater detection mechanism has to be the output of the comprehension mechanism.

At this point we may invoke another general principle of good cognitive design: Human cognitive architecture should be designed so that, when a problem has already been given a solution by one mechanism, no other mechanism is activated to solve it a second time. Suppose, however, this principle does not apply to the case at hand, that, for some reason. Then, participants will have two solutions activated in their mind—one by comprehension, the other by cheater detection—which may be different or identical. SCG explained why, in general, the comprehension mechanism makes the P and not-Q cards stand out as the relevant ones in standard deontic selection problems, just as would a cheater detection mechanism. Thus, they claimed, there is indeed the risk of a confound between the comprehension and the hypothetical cheater detection mechanism (if the latter is activated at all). FCT granted that there might be such cases of confound. They pointed out that,

> However, the confound works both ways: content specific cognitive adaptations may be doing the work attributed to conversational pragmatics on deontic versions of the selection task. Unconfounded experiments are needed to demonstrate which effect dominates. (FCT, p. 24)

Their article presents several such experiments; that is, experiments where relevance theory on the one hand and social contract theory on the other hand should, according to them, predict different selection of cards. Such experiments might in principle help decide which of the comprehension module and the cheater detection module preempts the other.

Instructional Blindness or Pragmatic Interpretation?

There are, however, major methodological problems with FCT's use of the selection task, and therefore with their data. To explain the problems—and how they might have arisen in the first place—let us underscore a peculiarity of the choice of the selection task by Cosmides and her collaborators as a means to establish the existence a cheater detection mechanism. According to FCT

> the definition of cheating does not map onto the logical definition of violation (the latter being a true antecedent paired with a false consequent). Cheating is a content dependent concept: there must be an illicitly taken benefit. This, and only this, counts as cheating. Logical categories and definitions of violation form an orthogonal representational dimension. (FCT, p. 15)

Now, in a standard deontic Wason selection task, participants are asked to select the cards that could represent violations of the conditional rule. Depending on the formulation of the rule, these cards may or may not also represent potential cheaters. Cosmides' (1989) idea is that the evolved look-for-cheater strategy is so powerful (and attention to explicit instructions and logical properties so powerless) that participants will select the cards that represent potential cheaters, whether or not they constitute the correct solution. In other words, the selection task is being used to show that people are unable to follow its instructions. In standard descriptive tasks where no domain-specific module is activated, people fail to obey the instructions, whether they pay attention to them or not. In social contract problems, they follow the mental "instructions" of their cheater-detection module and just ignore the explicit instructions. Even when they seem to succeed at the task, participants are in fact doing something else.

It is peculiar to use, as one's experimental paradigm of choice, a task whose instructions, one believes, participants are unable to obey. However, if this allowed establishing that, presented with a Wason selection task, participants suffer from a kind of "instruction blindness" compensated by "looking-for-cheater" intuitions, it might be of genuine and indeed striking cognitive interest. This is clearly FCT's intent, who claim to show that

> the inference mechanisms activated by social contracts and precaution rules short-circuit any logical SIAs [spontaneous inferential abilities] and contentgeneral pragmatic factors that may exist, causing domain-appropriate inferences to be made even when these violate rules of logical inference. (pp. 62–63)

Is this, however, what is shown by the experiments of Cosmides and her collaborators, and in particular by the experiments of FCT? There are two problems with this interpretation, one serious and one critical. Let us first evoke the merely serious problem.

It is one thing to point out the conceptual difference between cheating and logical violation; it is another to establish experimentally whether people tend spontaneously to look for cheaters rather than for violators. To do this by means of the selection task, it is necessary to find versions of the task where looking for violators and looking for cheaters results in different selection of cards. According to Cosmides (1989), this occurs in the case of "switched rules." Switched rules are obtained by switching the antecedent and the consequent of the original (unswitched) conditional rule (with possible minor linguistic adjustments, e.g., of tense). With an original rule of the form "if A, then B," the swiched rule is of the form "if B, then A." This relationship corresponds to what in classical logic is called "conversion." Since Aristotle, it is known that a conditional and its converse are not logically equivalent. For instance, from a logical point of view, when B is true, "If A then B" is necessarily true, whether or not A is also true, whereas "If B then A" may be false, if A happens to be false. On the other hand, as we will soon illustrate, an original rule and its converse or "switched" counterpart, in some contexts, may be understood as expressions of the same social contract. In such cases, the same actions may constitute cheating both with switched and unswitched rules. Therefore, switching the rule may allow one to dissociate logical violation, which necessarily changes with switching, from acts of cheating, which may remain the same. Comparing participants' selection of cards with switched and unswitched rules should demonstrate whether they are looking for logical violations or for cheaters. If participants are looking for violations, then they should select different cards in the two conditions; if they are looking for cheaters, they should select the same cards in both conditions, or so the argument goes.

Here is FCT's own example of a switched rule.

> …assuming 'you' are the potential cheater, 'If I give you $10, then you give me your watch' would be standard, and 'If you give me your watch, then I'll give you $10' would be switched. To detect cheaters, one needs to choose the benefit accepted card and the requirement not satisfied card, regardless of their logical category. Choosing these cards results in a logically correct answer, 'P & not-Q', for a standard social contract. But for a switched social contract, the same choice results in a logically incorrect (but adaptively sound) answer, 'not-P & Q.'(FCT, Fig. 1, p. 17)

But how should participants understand the instruction to look for potential violators of the rule, "If you give me your watch, then I'll give you $10?" Is there any sensible logical and literal interpretation of this instruction? Interpreted literally and logically, the only behavior that could violate the rule is that of the individual who has made the contractual conditional promise and who could break it by taking the watch and not giving the $10. The promisees, on the other hand, are not in a position to break the promise. However, in FCT's example, the promisor is not represented by any of the cards. The cards represent rather four promisees. So, the logically correct response to the instruction, "Indicate only those card(s) you definitely need to turn over to see if any of these people are violating the rule" (FCT's own formulation in Table 9.1), contrary to what FCT assert would not be the P card (has given the watch) and the not-Q card (has not received the $10), but no card at all. Again, the people represented by the cards cannot logically violate a promise they have not made. Why then do participants choose the not-P card (has not given the watch) and the Q card (has received the $10) with such switched rules? For obvious pragmatic reasons: They interpret the rule and the instruction so as to make good sense of the problem. This is a textbook case of the very common phenomenon of "invited inference" and more specifically of "conditional perfection" (Geis & Zwicky, 1971; Horn, 2000; van der Auwera, 1997) where a conditional statement is interpreted biconditionally, as implying in this case, "if I give you $10, then you give me your watch". This biconditional interpretation is even reinforced by the very instructions of the task. It is only with this enriched interpretation that participants can make sense of the instruction to identify potential violators out of four people to whom a promise has been made. Then, the logical selection based on the pragmatically interpreted rule and the selection of potential cheaters are achieved by the selection of the same not-P card (has not given the watch) and Q card (has been given the $10).

Switched rules were meant to provide crucial evidence for the social con tract theory, by prying apart the identification of violation and that of cheating. What we have just shown (see also SCG p. 86; Sperber & Girotto, 2002) is, at the very least, that switched rules are pragmatically infelicitous and awkward and that results obtained with such rules are open to a quite sensible pragmatic interpretation different from FCT's. Even if FCT were to stand by their interpretation, the fact remains that the evidence obtained with switched rules data can be sensibly interpreted in a manner that takes away its evidential force in favor of social contract theory.

With social contract rules, separating cheating from formal violation of the rule is not easy and cannot be achieved by mere switching of the rule. One case where this separation has been genuinely achieved is Girotto et al. (2001). As reported, these authors showed that participants select the usual P and not-Q cards, when the deontic rule, “If a person travels to any East African country, then that person must be immunized against cholera” is presented as being in force. The cards selected represent both potential “logical” violators and potential cheaters, the two being equivalent. However, if, in an otherwise similar scenario, the same rule is presented as being not in force anymore, participants incorrectly select the P and Q cards. Note that, in this condition, the P and notQ cards are the logically correct selection but do not represent potential cheaters: A rule that is not in force can still, logically, be obeyed or disobeyed, but disobeying it is not a case of cheating.

These results (which were replicated in a between-participants design) are quite consistent with social contract theory, because participants show themselves to be interested in cheaters and not in violators per se. However, they are also quite consistent with the relevance approach, because when the rule is in force, what is relevant is the case of violators who may incur sanction, whereas when the rule is not in force, what is relevant is the case of people who have undergone an unnecessary immunization and may complain. Moreover, if both the social contract and the relevance approach can explain why participants select P and not-Q cards in the first and not in the second condition, only the relevance approach explains why they specifically select the P and Q cards in the second condition (rather than answering at random).

Selection Task Versus Categorization Task

Let us now turn to the other, not just serious but critical problem raised by FCT’s methodology. To show that people are blind to the “logical” instructions of the selection task and obey rather the mental instructions of their social contract algorithm, it is essential that they be presented only with the standard instructions of the task and in particular that they not be explicitly asked to look for cheaters. If they are explicitly asked to look for cheaters and if that is what they do, the most parsimonious explanation of their behavior is that they have understood the instruction and have followed it. There is no obvious reason to invoke a dedicated mental mechanism. In general, in a deontic Wason selection task, participants should not be given any description of the cards to be selected, other than the fact that these cards must be relevant to establishing whether or not the rule is obeyed. Otherwise, it cannot be decided whether they are trying to follow the Wason task standard instructions, or whether they are selecting cards meeting the additional description. As SCG pointed out, a number of past experiments with deontic tasks (e.g., Cosmides, 1989; Light, Girotto, & Legrenzi, 1990; Manktelow & Over, 1991) have failed to respect this obviously sound methodological principle.

Far from heeding SCG's warning, FCT are going one dramatic step further. Rather than just adding to a standard Wason task instruction a direct question about cheating, as Cosmides (1989) had done, they altogether drop the standard instruction and just ask participants which of the four cards might represent a cheater (or a person endangering oneself in their Experiments 2 and 3, which are about precaution and not about social contracts). This is not a Wason selection task anymore. It is what we call an "FCT selection task." In spite of superficial similarities, the two selection tasks—the Wason and the FCT—are cognitively quite different. In the true Wason task, the question is about the truth or falsity (in the descriptive versions) or the respect or violation (in the deontic versions) of a conditional rule. The only fail-safe way of answering it involves applying conditional reasoning to four hypothetical cases, treating the rule as the major premise, and information displayed on the visible side of the card as the minor premise in a conditional syllogism. What made the task apparently relevant to the study of reasoning is its surprising difficulty. More specifically, participants' failure to select the not-Q card could be seen as revealing their inability to perform a modus tollens syllogism, and their tendency to select the Q card could be seen as a revealing a disposition to commit the affirmation-of-the-consequent fallacy.

In FCT's task, a conditional statement may be part of the narrative, but it is not mentioned in the question. The question is of the form: Which of the cards could represent an item falling under a given category (that of a cheater or that of a person taking risks, for instance)? This is a categorization task and not a conditional reasoning one. If, for instance, the category of a cheater is defined by the co-occurrence of the two features: "has taken the benefit," and "has not fulfilled the requirement," then selecting the cards that might represent cheaters requires just picking those cards that display either one of these two features (and that might have the other defining feature on the other side).

More generally, one could construct FCT selection tasks with any category characterized by two features. Is there any reason to think that such tasks would be of particular psychological interest? Should we expect people to generally fail at the FCT task, as they do with the Wason task, and to succeed when the material used activates domain-specific evolved mechanisms? We investigated the matter in Sperber and Girotto (2002). We compared three FCT tasks, one where the relevant category was that of a cheater, one where it was that of a glider that can be defined by the two features: "flying vehicle," and "with no engine," and a third arbitrary category that we could not resist calling "Wason selection" and that was characterized by the two features "food" and "nonItalian." All three categories involved a positive and a negative feature so that the correct selection was that of a P and a not-Q card.[1] Only one of the categories we used, that of a cheater, might plausibly be subserved by an evolved specialized mechanism. Whereas the defining features of a cheater and a "Wason selection" were explicitly mentioned in the problem text, those of a glider were not, which might have made the selection of the cards potentially representing a glider more difficult. In spite of these differences, we predicted that participants would perform quite well in all three conditions.

Our prediction was confirmed: 68% of the participants made the correct selection in the cheater condition, 73% in the glider condition, and 91% in the "Wason selection" condition. We concluded that the FCT selection task is trivially easy. Participants do as they are told, whether the category is an evolutionarily significant one or not, whether it is one already known to the participants or not, and whether they are reminded of the defining features of the category or not. FCT tasks look interesting only if they are mistaken for genuine Wason tasks, which are puzzlingly difficult. When a Wason task condition is compared with an FCT task condition, as FCT do in their Experiment 2, the difference in performance is great, but this should not be more surprising than, say, the difference in performance of young children in an adding versus multiplying the same two numbers. In both cases, the simplest algorithm for one task is objectively simpler than the simplest algorithm for the other. Only a difference in the opposite direction would be surprising and of genuine psychological interest.

Looking for Cheaters Versus Looking for Exchanges

In another experiment, we replicated FTC Experiment 1 and added two conditions to ascertain again whether or not, in FCT selection tasks, cheaters are more easily detected than items of any other category. FCT used two versions of a scenario where a South American farmer meets neighboring villagers who offer to give some of their corn for some of his potatoes. In one "conditional" condition, each villager expresses his offer conditionally: "If you give me some potatoes, then I will give you some corn." In another "want" condition, the offer is implicit: The villagers say, "I want some potatoes," and the farmer replies "I want some corn." The cards represent four villagers and indicate: "You gave this person potatoes" (P card); "You gave this person nothing" (not-P card); "This person gave you corn" (Q card); and "This person gave you nothing" (not-Q card). Participants were then asked to "Indicate only the card or the cards you definitely need to turn over to see whether any of these people have cheated you."

In both conditions the majority did as they were told and selected the P and not-Q cards that represent possible cheaters. What was the point of this experiment? To show that the presence of a conditional rule is not necessary for cheater detection to operate. When the situation describes an exchange, and when moreover participants are instructed to take the perspective of a party to the exchange, then, according to FCT, the social contract algorithm is activated and participants look for cheaters, with or without a conditional expression of the contract. We believe that, because this is an FCT selection task, the presence or absence of a conditional statement is irrelevant, provided that the narrative is intelligible. We believe, however, that participants select the cards corresponding to potential cheaters just because they are asked to do so. There is no need to invoke a look-for-cheater strategy at work. To confirm our interpretation, we replicated FTC's experiment and added two conditions that differed from theirs only in the last words of the instructions. Our instructions were: "Indicate only the card or the cards you definitely need to turn over to see whether any of these people have made an exchange with you." We predicted that participants in these two conditions would select the P and Q cards; that is, the two possible honest exchangers.

The result confirmed our prediction. In the replications of FCT's two conditions, the most frequent selection was that of the possible cheaters (33% in the conditional version and 43% in the "want" version). In our versions, the most frequent selection was that of the potential honest exchangers (33% in the conditional version and 53% in the "want" version). With such an exchange scenario, participants seem to be no less disposed to look for an honest exchanger than to look for a cheater, provided that this is what they are asked to do. (In all conditions, and in particular in the "conditional" ones, the rate of good performance is rather low, possibly because the scenario was not as clear to our participants as it may seem to be.)

As we said, and hope now to have demonstrated, FCT's experimental evidence suffers from a major methodological problem. The task used is not a Wason but an FCT selection task. FCT selection task experiments are irrelevant to assess SCG's explanation of the Wason selection task. Moreover, it is unclear to what they might be relevant.

Still, social contract theory is based not so much on these new experiments as on rich earlier experimental data. Apart from the methodological issues we have just discussed, there are also more empirical differences between FCT and us, regarding the expected outcome of various selection task scenarios. Because we dispute that selection task data are of relevance to the psychology of reasoning, we do not see these differences in prediction as having significant theoretical import. However, we believe that testing predictions may help decide between FCT's and our interpretation and evaluation of the Wason selection task. We do this in the following experiments.

NEW EXPERIMENTAL TESTS OF FIDDICK, COSMIDES, AND TOOBY'S PREDICTIONS

Experiment 1

FCT raise an interesting issue in their discussion of SCG. SCG had explained the fact that most people correctly select the P and not-Q cards in most deontic versions of the Wason task, by arguing that realistic deontic rules such as social laws achieve relevance by singling out and forbidding specific states of affairs represented by this selection of cards. For instance the drinking age rule: “If a person drinks beer, then that person must be over 19 years old” (Griggs & Cox, 1982) implicitly distinguishes four cases that there would be little reason to distinguish in the absence of the rule:

drinking beer and being over 19

not drinking beer and being over 19

drinking beer and being under 19

not drinking beer and being under 19

Out of these four cases, the rule singles out the third one—drinking beer and being under 19—by forbidding it. The three other cases are not in violation of the rule and are of no particular relevance. In general, social laws make relevant not behavior that conforms to them but behavior that violates them. Girotto et al. (2001) showed how this too can be manipulated by pragmatic means.

FCT (p. 30) point out, however, that this reasoning

> does not extend easily to the class of private exchanges (trades) as distinct from social laws. In formulating their argument that the communicative intent is to forbid, SCG analyzed social laws, where their argument more plausibly fits […]. But social contracts expressing a trade lack key features upon which SCG’s analysis of deontic rules is based. Their purpose is to enable mutual access to goods through a trade, not to forbid access to a good, and this has implications for what subjects should find relevant. […] a consistent application of relevance theory to private exchanges should lead subjects to answer “P and Q” or “P, Q, and not-Q” for trades, rather than “P and not-Q.”

We are grateful to FCT for this observation, which, we think, is essentially correct. There are interesting exceptions, of course; in some cases of trade, the risk of being cheated is more salient than the opportunity of obtaining goods, as we will illustrate. More generally, we believe that any type of rule can be manipulated to give logically correct or incorrect selections. Still, we accept that our account of the Wason selection task commits us to predict that most subjects will select the P and Q cards, or the P, Q, and not-Q cards in standard cases of exchange. Now, it might seem that, if not FCT’s new results, at least earlier results of Cosmides (1989) and others show that this prediction is dead wrong. Experiments have repeatedly shown that the majority of participants select P and not-Q in such cases, just as they do with social laws.

At this point, we must either concede that something is wrong with the relevance theory approach to the selection task, or else argue that something is wrong with the experimental evidence. In general, experimental evidence trumps theoretical claims; this, after all, is the reason for seeking experimental evidence in the first place! However, in this case, we have already demonstrated that many experiments performed by Cosmides, Tooby, and their collaborators either are not Wason selection tasks but FCT selection tasks, or are an unfortunate mix of the two (which we will call "FTC/Wason tasks"), with participants being simultaneously asked two questions, a Wason one and an FCT one, as if it was the same question asked twice. Therefore, in order to clarify the issue, one should compare genuine Wason selections tasks and mixed Wason/FCT tasks with private trade scenarios. We predict, as FCT correctly pointed out we should, a high rate of P and Q, or P, Q, and not-Q selections with the genuine Wason tasks; we predict, on the other hand, a high rate of P and not-Q selections with mixed Wason/FCT tasks asking for cards representing possible cheaters, as has been found previously in the literature. We believe that when, in these Wason/FCT tasks, participants choose the cards that could represent cheaters, they are answering the easy FCT question about cheaters, and not the more difficult Wason question as to whether or not the rule is being respected. If this is correct, the responses should be different when only the Wason question is posed.

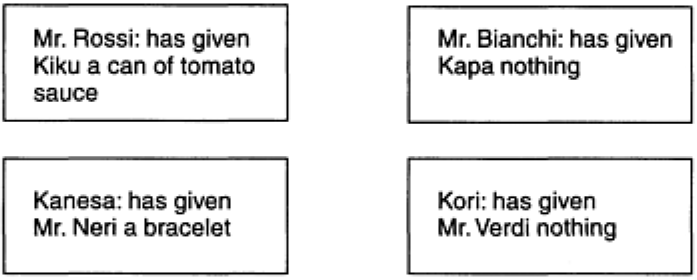
In Experiment 1, we compared two conditions:

1. A genuine Wason selection task, where participants are asked which cards should be turned over to see whether the conditional rule expressing the conditions of a private trade was respected. Even though the trade has the canonical requirement-benefit structure, we did not expect participants to look for cheaters, given that they were not asked to do so. We expected them rather to look for potential instances of exchange.
2. A mixed Wason/FCT selection task with exactly the same scenario as in the preceding condition, except that participants were asked both whether some cheating had taken place and which cards should be turned over to see whether the rule was respected. This is of course methodologically flawed, but it reproduces a flaw found in previous experiments, such as Cosmides' (1989) Big Kiku experiment discussed in Sperber and Girotto (2002). We predicted that we would find results similar to those found in these experiments, that is, a high rate of P and not-Q selection.

Method

Participants. The participants in all our experiments were humanities undergraduates at Udine University (Italy), who participated in the experiment voluntarily. In Experiment 1, 60 were tested. They were randomly assigned to one of two equal-sized groups (N=30): "private trade (Wason version)," "private trade (Wason/FCT version").

Material and Procedure. The study was carried out in Italian with native speakers of the language. Each participant had to solve one problem. The participants were tested in groups, but they worked on the problem individually,



Mr. Rossi: has given Kiku a can of tomato sauce

Mr. Bianchi: has given Kapa nothing

Kanesa: has given Mr. Neri a bracelet

Kori: has given Mr. Verdi nothing

Indicate only the card or the cards you definitely need to turn over to see whether [there are tourists who have been cheated by the natives, and], as far as these four encounters are concerned, the rule “If a tourist gives a native a tomato sauce can, the native gives him/her a bracelet” has been respected.

FIGURE 9.4. Cards that represent encounters between tourists and Boro-Boro natives.

at their own pace. The text was identical in both the Wason version and the Wason/ FCT version, except for the words in brackets in the final instructions, which appeared only in the Wason/FCT version. The problem read as follows:

> A group of tourists visits the island of the Boro-Boro people. Some tourists and natives agree to perform the following exchange: If a tourist gives a native a can of tomato sauce, the native gives him or her a bracelet. Below you find four cards, each of which represents a different encounter between a given tourist and a given Boro-Boro native. On the one side, the card indicates whether the tourist has given the native a can of tomato sauce. On the other side, the card indicates whether the native has given the tourist a bracelet.

Results and Discussion

Table 9.1 presents the percentages of the main selection patterns in the two conditions.

As predicted, a majority of participants selected P and Q, or P, Q, and notQ cards in the Wason version of the task, whereas no one selected these cards in the Wason/FCT version ($\chi^2(1, N=60)=19.9$, $p<.001$). Similarly, a majority of participants selected the P and not-Q card in the Wason/FCT version, whereas

TABLE 9.1. Percentage of the Main Selection Patterns in the Two Conditions of Experiment 1 (*N*=30)

	Condition	
Pattern	Private trade (Wason version)	Private trade (Wason/FCT version)
P, not-Q	0	57

	Condition	
Pattern	Private trade (Wason version)	Private trade (Wason/FCT version)
P, Q	63	0
P, Q, not-Q	10	0
P	13	0
Q	7	0
not-Q	0	23
Other	7	20

Note: Each of the patterns indicated in the Other cells was produced by fewer than four participants.

no one selected these cards in the Wason version ($\chi^2(1, N{=}60){=}23.3, p< .001$).

It could be argued, however, that the direct exchange described in the scenario did not leave much room for cheating, and that this is why, in the Wason version, participants did not select cards representing potential cheaters. Note that the same argument could be leveled against FCT's Experiment 1 [replicated by Sperber & Girotto (2002) and discussed in the preceding], but to make sure anyhow, we performed a modified version of Experiment 1 with a scenario where the risk of one party being cheated by the other was a genuine and obvious one.

Experiment 2A

Experiment 2 came in two parts. In Experiment 2A, the method was exactly the same as in Experiment 1, with two groups of 30 participants assigned to a genuine Wason task and to a mixed Wason/FCT task. We used the same scenario as in Experiment lexcept that we replaced the rule "If a tourist gives a native a can of tomato sauce, the native gives him or her a bracelet" with the rule "if a native gives a tourist a bracelet, the tourist will send him or her a picture from Italy." We modified the cards accordingly. The instructions read (with the added part of the FCT/Wason version in square brackets): "Indicate only the card or the cards you definitely need to turn over to see whether [there are natives who have been cheated by the tourists, and], as far as these four encounters are concerned, the rule 'If a native gives a tourist a bracelet, the tourist will send him or her a picture from Italy' has been respected."

In this scenario, there is a genuine and obvious risk that a tourist who had accepted a bracelet might, out of negligence or dishonesty, fail to send a picture from Italy. Nevertheless, in the Wason version, we assumed that the very fact that such exchanges would have taken place between tourists and Boro-Boro natives might be more relevant to the majority of the participants than the fact that some of the tourists might not have performed their part of the deal, and that they would therefore select the P and Q cards. We also expected that, when, in the Wason/FCT version, participants were asked directly about cheating, then they would select the P and not-Q cards.

The results corroborated our predictions. In the Wason version of the task, 50% selected the P and Q cards, whereas only 13% made this selection in the Wason/FCTversion($\chi^2(1, N=60)=7.70$, $p<.01$). Similarly, in the latter version, 47% selected the P and not-Q cards, whereas only 17% selected these cards in the former ($\chi^2(1, N=60)=4.93$, $p<.05$). On the other hand, contrary to what one might also have expected, there were no P, Q, and not-Q selections; that is, participants were interested either in the exchanges or in the cheatings, but not in both. Overall, as we anticipated, the results of Experiment 2A were very similar to those of Experiment 1, in spite of the much greater plausibility of cheating in the modified scenario. This confirms the prediction, suggested to us by FCT, that private trades in true Wason tasks elicit more incorrect P and Q selections than correct P and not-Q selection. The impression from earlier data (e.g., Cosmides, 1989) that private trades, with their paradigmatic requirement-benefit structures, elicit selections of cards representing potential cheaters (P and not-Q in the regular versions, and not-P and Q in the "switched" version) was based on the methodologically unsound use of mixed Wason/FCT versions of the task.

Experiment 2B

From the pragmatic point of view we are defending, we did not doubt that it is possible to devise a genuine Wason selection task based on a private trade such that participants select the P and not-Q cards, that is, the cards representing potential cheaters. All that needs to be done is create a context where the risk of cheating is particularly salient and is more relevant than the possibility of a regular exchange. Consider for instance the following problem.

> A new cure for insomnia is being advertised by the Morpheus drug Company. The Company's offer is: "If a person sends 40 euro to the Morpheus Company, then the company sends that person a bottle of the cure."
>
> An survey has been done on people who buy drugs against insomnia. Below you find four cards, each of which represents one of these persons. On one side, the card indicates whether the person represented by the card has sent 40 euro to the Morpheus Company. On the other side, the card indicates whether the Morpheus Company has sent a bottle of the cure to the person represented by the card.

Indicate only the card or cards you definitely need to turn over to see whether,as far as these four people are concerned, the rule "If a person sends 40 euroto the Morpheus Company, then the company sends that person a bottle ofthe cure" has been followed.

Mr. Rossi

Has he sent 40 euro to
the Morpheus Company?

Yes

Mr. Bianchi

Has he sent 40 euro to
the Morpheus Company?

No

Mr. Neri

Has the Morpheus
Company sent him
the cure?

Yes

Mr. Verdi

Has the Morpheus
Company sent him
the cure?

No

Indicate only the card or cards you definitely need to turn over to see whether, as far as these four people are concerned, the rule "If a person sends 40 euro to the Morpheus Company, then the company sends that person a bottle of the cure" has been followed.

FIGURE 9.5. A survey of people who buys drugs agains insomnia.

Given that this describes buying of goods by customers from a commercial company, the occurrence of actual buyings is more banal and less relevant than that of one-time barters between individuals. Moreover, given the lack of confidence (at least in Europe, at present) regarding both mail trade and the selling of miracle drugs, the risk of consumers being cheated is particularly salient, and the offer of the Morpheus company is likely to be read as implying a commitment not to fail to send the cure to those who send the money. Because of these factors, such a problem would be more likely to elicit the selection of cards representing possible cheatings than most examples of private trade. We tested this prediction by asking a new sample of participants (*N*=25) to perform the Morpheus Company selection task. As predicted, and as found in the Wason/FCT versions of the Boro-Boro problems, most participants (44%) correctly selected the P and not-Q cards, whereas only 16% selected the P and Q cards. This shows that, even when asking whether the rule has been followed rather than whether it has been violated, even without instructing participants to adopt the perspective of a potential victim of cheating, and of course without instructing them to look for possible cheaters, they do so in contexts in which the possibility of cheating is sufficiently salient and plausible to be more relevant than the occurrence of actual exchanges. However, it is not the case that private trades in general provide such a context.

Experiment 3

There is another difference in prediction between FCT and SCG. FCT (p. 41, n. 24; Appendix A) argue that not all social contracts, but only those where the requirement comes in compensation of a benefit should yield the selection of the P card (benefit received) and the not-Q card (requirement not fulfilled). SCG predict that any statement of a social law that achieves relevance by forbidding violations (rather than by promoting conformity) should trigger the selection of the P and not-Q cards whether or not the requirement it expresses comes in compensation of a benefit. As shown by Girotto, Blaye, and Farioli (1989), even children do solve selection problems presenting such social laws (e.g. "If a bee is sick, then it must stay out of the hive"). Repressive social laws are cases in point. For instance, we expect that a Wason selection task using the rule "If a person has been condemned by a tribunal, then that person must always carry his/her I.D. card" would yield a high rate of P and not-Q selections. Yet, the requirement of carrying one's I.D. does not come as a prize paid for the benefit of having been condemned.[2]

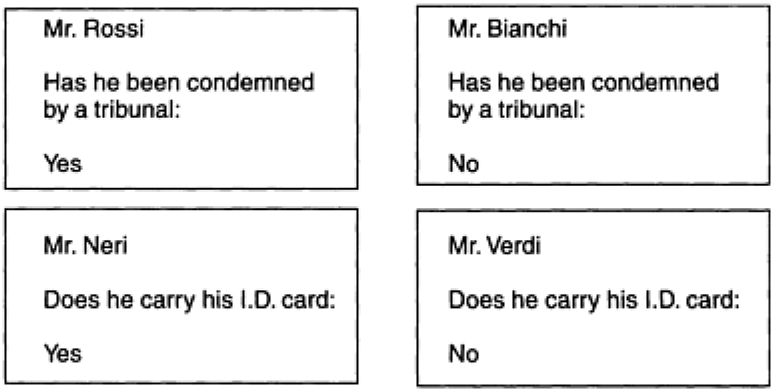
We tested this prediction in Experiment 3. The method was exactly the same as in Experiments 1 and 2, except that we used the following scenario:

> In your country, a new law has been introduced, in order to improve public security. According to this law:
>
> "If a person has been condemned by a tribunal, then that person must always carry his/her I.D. card."
>
> An investigation has been done on randomly selected people. Below you find four cards, each of which represents one of these people. On the one side, the card indicates whether the person has been condemned by a tribunal. On the other side, the card indicates whether the person carries his/her I.D. card.

Note that this is a genuine Wason selection task. Even though the question was about the law being followed rather than being violated, even though participants were not instructed to take the perspective of a law-enforcing character, and even though, of course, the requirement expressed by the social law does not come in compensation of any benefit, 47% of the participants selected the P and not-Q cards, and none of them selected the P and Q cards. This result confirms that, in genuine deontic Wason selection tasks, what drives selection of cards is not the requirement-benefit structure of the rule, but the relative relevance of conformity versus violation.

Discussion of the Experiments

In some deontic Wason selection tasks, predictions based on relevance theory differ from FCT's predictions. The present results suggest that, in those cases,



Mr. Rossi

Has he been condemned by a tribunal:

Yes

Mr. Bianchi

Has he been condemned by a tribunal:

No

Mr. Neri

Does he carry his I.D. card:

Yes

Mr. Verdi

Does he carry his I.D. card:

No

Indicate only the card or cards you definitely need to turn over to see whether, as far as these four people are concerned, the law "If a person has been condemned by a tribunal, then that person must always carry his/her I.D. card" has been followed.

FIGURE 9.6. An investigation of randomly selected people.

the relevance-based predictions are the correct ones. Impressions to the contrary are based on a faulty methodology using FCT tasks, or mixed Wason/FCT tasks.

Because we don't believe that the Wason selection task is a good tool to investigate anything other than the pragmatic interpretation of conditional statements (and it is not such a good tool for that either), we emphatically do not intend these results to serve as evidence for any claim about the mental representation of private trades or social laws. We are well aware—this has been the whole point of SCG and Girotto et al. (2001)—that any kind of rule can be contextualized so as to favor many kinds of selections. We see these results as showing just three things:

1. It is not the case that social contracts with a requirement-benefit structure making cheating possible automatically elicit a look-for-cheater procedure.
2. It is not the case that social laws without such a requirement-benefit structure necessarily fail to elicit a search for violations.
3. Mixing true Wason and FCT instructions is likely to yield trivial results based just on the FCT part of the instructions.

CONCLUSION

What does all this mean regarding Cosmides' (1989) hypothesis about the existence of a social contract algorithm? It just means that this hypothesis has not been tested yet! The search for evidence went the wrong way Of course, the fact that the search for evidence was centered on the selection task influenced the way the hypothesis itself was formulated and refined. So, time has come to rethink and retest the hypothesis. Let us just make some suggestions as to how this might be done.

Being able to conceive of, and enter in to contractual agreements involves not just higher cognitive abilities but also higher communicative abilities. For all except the simpler kinds of contractual agreements, it requires a language. Although it might be an interesting line to explore, no one is suggesting that human communicative abilities and in particular human language have evolved as adaptations specifically geared to the passing of contracts. Everybody assumes on the contrary that, from the start, the contents that could be linguistically communicated were quite diverse, implying that at least a good part of the cognitive abilities needed to conceive of these contents were contemporaneously available.

A standard and plausible view is that the cognitive abilities in evidence in linguistic communication involve the possession of conceptual primitives and the capacity to combine these conceptual primitives into more complex concepts. Conceptual primitives may need cognitive inputs to be activated and finetuned, but they are each based on a specific evolved disposition. Complex concepts resulting from the combination of primitive ones need not have a distinct evolutionary history. New combinations can be devised all the time. This, after all, is what combinatory ability is for. Still, there may exist evolved dispositions to combine some primitive concepts in specific ways if the resulting complex concept is of particular evolutionary significance and is to be used in special ways.

Cosmides does not discuss the issue of the primitive or complex character of the concept of a contract and of the associated concept of a cheater. She just treats both concepts as complex ones, combining two simpler ones, that of a right (the benefit) and that of a duty (the requirement). A good case can be made for considering the concepts of right and duty to be primitives (see Holyoak & Cheng, 1995; Politzer & Nguyen-Xuan, 1992), and part of the evolved conceptual repertoire of humans (and arguably only of humans).

What Cosmides' (1989) approach implies is that the complex concepts of a contract and a cheater are not just a combination of primitive concepts of rights and duties, but that their deployment is based on a specific evolved disposition.[3] Given the evolutionary significance of these concepts, this is not implausible. But in which way is the deployment of these concepts more than the deployment and combination of their primitive parts? Cosmides' hypothesis is that in the use of these concepts, humans are capable of drawing specific kinds of inferences that are not available to them in the case of other complex concepts. These inferences may mimic the "modus tollens" form in their special domain, but they are not genuine domain general modus tollens inferences. She takes her experimental data to provide crucial evidence for such a hypothesis.

At this point we doubly disagree. We believe that what makes modus tollens inferences easy or hard is not the specific subject matter (e.g., contracts versus colors) but the pragmatics of the utterances in which these notions are used.

People spontaneously perform modus tollens inference in all cognitive domains when this is the way to give a relevant interpretation to the utterance they are processing (see Politzer, forthcoming). Moreover, we have argued that, when they identify potential cheaters by selecting individuals who are known either to have taken the benefit or not to have met the requirement, Cosmides' (1989) experimental participants merely perform simple Boolean combinations of concepts. The ability to use such simple combinations of features (including negative features) for categorization is found across domains: A bitch is a female dog, a Barbarian was a non-Greek human being, a stool is a seat without a back, a glider is an aircraft without an engine. We have shown (Sperber & Girotto, 2002) that detecting potential instances of such categories, whether it be an evolutionarily important category such as that of cheaters, or an unimportant one such as that of gliders, is trivially easy.

This is not the end of the story. Even if our criticisms are justified, it would be wrong to dismiss Cosmides' (1989) basic hypothesis that there may be cognitive adaptations dedicated to the detection of people cheating on social contract. Even if our criticisms were mistaken and Cosmides and her collaborators had actually uncovered such an adaptation in reasoning about cheaters, one should consider the possibility that the risk of being cheated put selective pressure not just on reasoning with the concept of a cheater, but also, and maybe more importantly, at other levels.

To begin with, cheaters have a vested interest in doing their best to hide the fact that they are cheating, or, at least, to hinder as much as possible access to evidence of their cheating. This can be done in a variety of ways: by making vague or ambiguous contractual agreements, by making the benefit seem less valuable, by making the requirement seem more costly, by invoking special circumstances such that failure to meet the requirement does not count as cheating, and so on. Detecting cheaters may requires forms of strategic reasoning that are much richer than what is needed in order to put together "has taken the benefit" and "has not met the requirement."

As Cosmides and Tooby (1989, 1992) themselves have noted, the risk of cheating is particularly high when there is a time gap between the taking of the benefit and the fulfillment of the requirement. The potential victim of cheating must keep track of past transactions, which is not always easy. To give but one typical example, among the Dorze of Southern Ethiopia, a society where one of us has done ethnographic fieldwork, people had to remember dozens or even hundreds of "gifts" made or received over several years, and that were all meant to be reciprocated when the proper circumstances would arise. Until recently, they were doing so without the help of writing. As this case illustrates, what makes detecting cheaters difficult in many cases is the fact that clear evidence of cheating is hard to find or keep track of.[4]

Given all this, there may be more than one type of cheater-detection mechanism. One such mechanism particularly likely to have evolved would result in an automatically high level of attention to, and recall of, details of interaction that might be evidence of cheating. The prediction that evidence of cheating is more carefully attended to and better remembered than others is experimentally testable (for an example, see Mealey, Daood, & Krage, 1996). Such a prediction would be worth testing across cultures. Other likely sources of evidence are to be found in experimental developmental psychology and the study of moral development and of moral emotions in particular.

There are many methods by means of which evolutionary psychology hypotheses such as Cosmides' (1989) might be seriously tested. Experimenting with the Wason selection task is not one of them. Investing massively in a single experimental paradigm is unsound (and don't count using patients with brain lesions [see Adolphs, 1999] with Wason or FCT selection tasks as a genuinely different paradigm). Cognitive psychology and neuropsychology have developed a rich battery of sophisticated experimental tools. If evolutionary psychologists want to exert a major influence on the field, they must share in, and contribute to this sophistication, and not overuse an oldish experimental paradigm. Moreover, the use of diverse and sophisticated methods would be likely to contribute to the enrichment and refinement of evolutionary psychology hypotheses.

NOTES

1. We had done another experiment with two arbitrary features, both positive. The text of the problem reads as follows: "There is a game consisting of a deck of cards, each of which has a letter on one side and a number on the other side. In this game the cards with a vowel on one side and an even number on the other are called 'jokers.' Below you find four cards. Two cards show the side with the letter. The two other cards show the side with the number." The cards displayed were: E, G, 7, and 4. The instructions were: "Indicate only the card or cards you definitely need to turn over to see whether among these cards there are any jokers." One hundred percent of the 35 participants correctly chose the card with an E and that with a 4. The experiments reported in the text suggest that having the relevant category defined by a positive and negative feature introduces a modicum of difficulty, but not enough to make the task really interesting.
2. FCT also suggest that there is another evolved mental mechanism dedicated to hazard management. This mechanism will cause people to select the P and not-Q cards in a selection task with a rule of the general form "if you engage in a dangerous activity, then take the appropriate precaution." However, carrying one's I.D. is not a precaution taken against the "activity" of having been condemned. The only danger for the people to whom the rule applies is that of being in violation of the rule. Surely the rule can not be seen as a precaution against a danger that is created by the very existence of the rule itself.
3. In fact, it would not be inconsistent for Cosmides (1989) to argue that the concepts of a contract and that of a cheater are not acquired by combining simpler concepts but are themselves primitives and are linked to the concepts of rights and duties by meaning postulates or inference rules.

4. The difficulty in keeping track of past events may be the source of errors in other reasoning domains. In particular, Girotto and Gonzalez (2001, 2002) pointed out that individuals may produce erroneous frequency predictions in situations in which they acquire information through a sequence of actual observations (e.g., Gluck & Bower, 1988). These results contrast with the finding that individuals tend to make correct frequency predictions in verbal problems presenting numerical symbols, rather than actual events, and with the claim that the human mind contains an evolved mechanism to make accurate frequency predictions (e.g., Cosmides & Tooby, 1996; Gigerenzer & Hoffrage, 1995). Along with the present remarks, these results warn against testing evolutionary hypotheses about reasoning abilities by using standard verbal problems and suggest developing alternative methods.

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10

Clinical Psychology and Evolutionary Psychology

Strange Bedfellows?

RICHARD SIEGERT

*E*volutionary psychology (EP) is a new approach to the study of human behavior that argues that our evolutionary history provides the fundamental framework for developing theory and research. Evolutionary psychologists regard the human mind as an information-processing device that evolved over millions of years to meet specific environmental challenges. Thus, we should begin our study of human behavior equipped with an understanding of the nature of the type of mind that is most likely to have evolved.

The growth and development of evolutionary psychology has been little short of spectacular (Barkow, Cosmides, & Tooby, 1992; Buss, 1999; Cosmides & Tooby, 1989). Since the early 1980s EP has rapidly replaced its predecessor, sociobiology (Wilson, 1975), and become a powerful paradigm for generating theory and research on such varied phenomena as language (Pinker, 1997), pregnancy sickness (Profet, 1992), autism and theory of mind (Baron-Cohen, 1995), mate selection (Buss & Schmitt, 1993), step-parenting and marital conflict (Daly, Singh, & Wilson, 1993), and rape (Thornhill & Palmer, 2000). EP's strengths include its grounding in evolutionary biology, which provides a unifying theory for understanding the behavior of organisms, and also the ability of the resulting theory to generate testable predictions concerning a variety of human behaviors. A basis in evolutionary biology means that EP shares the same scientific foundations as all other disciplines that study living organisms.

The term evolutionary psychology can be confusing, however, given its current popularity and the diverse range of viewpoints that have been included under it. Caporael (2001) has argued that there is not one evolutionary psychology, but rather, a range of different evolutionary psychologies. One of these perspectives is that associated with the Tooby and Cosmides manifesto, which views the mind as an evolved information-processing device, highly modular in its organization. Caporael refers to this school as "inclusive fitness evolutionary psychology," and sees its adherents as the direct descendants of sociobiology. However, she also identifies at least four other general approaches that she calls "general evolutionary approaches” (e.g., Donald, 1991), “sociality theories” (e.g., Dunbar, 1993), “multilevel evolutionary theories” (e.g., Boyd & Richerson, 1985) and “developmental systems” approaches (e.g., Oyama, 2000). In Caporael’s classification scheme these different approaches sit on a continuum, with Tooby and Cosmides’ modular viewpoint at one extreme, and the developmental systems approach represented by Oyama (2000), at the other. I focus on “inclusive fitness evolutionary psychology” because its advocates have written most directly and frequently on the relevance of an evolutionary perspective for the study of psychopathology (e.g., Buss, 1999; Gaulin & McBurney, 2000).

CLINICAL PSYCHOLOGY AND EVOLUTIONARY PSYCHOLOGY

Despite the rapid rise and popularity of EP in recent decades, its impact on the field of clinical psychology appears to have been minimal. For example, in a PsycLIT search for the years 1989 to 1999, using the key words *evolution* (and alternatively, *evolutionary), clinical,* and *psychology,* we hit only five references — none of which concerned evolutionary theory and clinical psychology, but rather were all concerned with the evolution of the profession. A search for *evolutionary psychopathology* produced two relevant references. In writing this chapter we also combed the indexes of some half-dozen contemporary textbooks of clinical psychology. References to Darwin and evolutionary explanations of behavior were uncommon, and occurred mostly in relation to the early history of the field.

There are a number of historical and ideological reasons for the relative unpopularity of Darwinian perspectives on human nature in clinical psychology. These have been detailed elsewhere (Siegert & Ward, 2002, in press). This trend was not to change until the early 1970s, when animal models of psychopathology became popular again. In particular, Seligman’s notion of biological preparedness, and mounting evidence that not all fears were equally conditionable, suggested that evolutionary concepts might have a place in the clinic after all (Menzies & Clarke, 1995; Seligman, 1971).

The present discussion has focused on clinical psychology and evolutionary theory, but a similar picture has been noted for the discipline of psychology in general. Durrant (1998) has noted a decline in evolutionary thinking in general psychology from the early part of the 20th century and then a renewal of interest beginning in the 1960s. A similar picture has also been observed in the history of psychiatry with interest in evolutionary theory being rekindled only by the publication of an evolutionary explanation of depression (Price, 1967) and the enormously influential work of Bowlby (1969) on attachment in infants.

If clinical psychology (CP) is indeed a science, then it must in part, be a natural science. Its primary subject matter is the behavior of one unique species of mammal. Moreover, if we reject evolutionary theory as a foundation stone of CP, then what alternatives are there? It may be that many clinicians view such distal factors as natural selection as insignificant compared to proximate factors such as an individual's developmental history, personality, social skills, attachment style, and social support. A clinician might well argue that we are only interested in proximate causes because they are the only ones that we can influence or manipulate now. In the present chapter I argue that such a viewpoint is both short-sighted and unscientific. It is also not in the best longterm interests of CP or its clients. We can only treat mental disorders to the extent that we have developed comprehensive explanatory models of those disorders that can serve to guide therapeutic strategies. To exclude ultimate causes or evolutionary explanations altogether seems to be saying that we already understand most mental disorders perfectly well in terms of proximate variables alone. This seems a dubious claim to say the least. It also seems to suggest a sharp dichotomy between proximate and ultimate causes.

A more useful and sophisticated framework that might avoid this oversimplification is Tinbergen's fourfold distinction amongst explanatory questions about a specific behavior (Tinbergen, 1963, cited in Sterelny & Griffiths, 1999, p. 19). Tinbergen distinguished among four kinds of explanation in the evolutionary analysis of behavior:

> (1) the evolutionary history of a behavior; (2) the current use of the behavior in the life of the animal, which may involve a change from (1); (3) the development of the behavior over the life of the organism; (4) the psychological and other mechanisms used in the control of the behavior.

A fully comprehensive explanation of behavior would ideally explain behavior at all four levels. When it comes to human behavior, I shall argue that our best chance of understanding (3) and (4), and when necessary intervening, is by understanding (1) and (2) as well.

Indeed, a closer dialog between clinical psychology and evolutionary psychology could lead to new and more refined theories of psychopathology and more effective psychological therapies. This dialog has already begun in psychiatry, which as discussed, also largely ignored Darwinian ideas until quite recently (e.g., McGuire, Marks, Nesse, & Troisi, 1992). However, before expanding on the implications and potential benefits of a synthesis of CP and EP, I outline some of the major developments in the new field of evolutionary psychopathology. These provide evidence of the potential advantages that clinical psychology might also gain from rediscovering its Darwinian heritage. It is also an emerging field in which psychologists have been major contributors.

EVOLUTIONARY PSYCHOPATHOLOGY

Although the impact of EP on CP may be limited, there has been a surge of interest in the relevance of evolutionary theory for the study of psychopathology in general. This new synthesis is often called evolutionary psychopathology[1]

(EPP) (e.g., Baron-Cohen, 1997; Gilbert, 1998a, McGuire & Troisi, 1998). I first summarize the essential features of EPP, and then illustrate its theoretical fertility by describing its application to the study of depression.

What Is Evolutionary Psychopathology?

Evolutionary psychopathology is the application of evolutionary theory to conceptualizing, classifying, and determining the etiology of, mental disorders. It is multidisciplinary in nature and draws upon the methods and theories of evolutionary biology, developmental and clinical psychology, behavioral genetics, and psychiatry, to offer a radically new perspective on mental disorder. A central feature of many psychological disorders is that the behaviors that cause the most distress frequently have a self-defeating or *maladaptive* quality. The person with a phobia avoids a situation that he or she know rationally is not a danger, even when this avoidance may prevent him or her from advancing his or her career. The person with schizophrenia hears voices that only he or she can hear and these voices persistently criticize his or her behavior. The person with depression becomes withdrawn and unable to make even simple decisions for him- or herself. EPP attempts to explain the etiology of such disorders by first considering which evolved psychological mechanisms might be involved.

Typically, in explaining these kinds of problems people have tended to assume that they represent the breakdown of a normal adaptive function. Gilbert (1998a) comments that “In the best Platonic traditions of medicine…pathologies are alien to normal functioning” (p. 353). Perhaps the most recent influential example of this tradition, has been Wakefield’s conception of mental disorder as “harmful dysfunction.” According to this definition a mental disorder must have two features. First, there is a value term based upon social norms. Second, dysfunction is a scientific term referring to the failure of a mental mechanism to perform a natural function for which it was designed by evolution (Wakefield, 1992). Wakefield states that, “Dysfunction is a purely factual scientific concept” (p. 383).

Such a definition of mental disorder has several advantages over alternative approaches to defining mental disorder as Wakefield has argued extensively. However, it may be incorrect in assuming that there has necessarily been a breakdown or failure in some natural function or mechanism that arose through evolution. There is an implicit assumption that the adaptive mechanism must always be advantageous and in the interests of the organism’s health and survival. This is one possible explanation but not the sole possibility according to EP.

Evolutionary Psychopathology: Modular Madness or Mad Modules?

Evolutionary psychopathology offers a number of alternative hypotheses as to how psychological disorders might occur. One is that the psychopathology may represent a mechanism that was previously adaptive under quite different conditions from our modern environment (see the following). Another is that the psychopathology might only represent a byproduct or side-effect of an adaptive mechanism, rather than the evolved mechanism itself. Another possibility suggested by Murphy and Stich (2000) is that pathology might result from problems in the interactions among modules[2] or behavioral systems. A fourth possibility is that psychopathology could arise, not from a dysfunctional modular system, but rather as a result of the environmental activation and calibration of the system. We will briefly consider the first two of these explanations of psychopathology in more detail.

Gilbert (1998a) suggests that “many states that we label ‘psychopathology’ may represent the activation of (previously) adaptive strategies” (p. 353). This is certainly true for some of our evolved physiological mechanisms. For example, humans have a sensitive apparatus for detecting sugar in foods and an evolved taste preference for sweetness. No doubt this was a useful adaptation in prehistoric times when sugar in ripe fruit was an important source of energy for huntergatherers, but in modern urban society our “sweet tooth” is a source of numerous health problems in the guise of obesity, dental cavities, diabetes, etc. Perhaps this is also true for some of the mental mechanisms that have evolved in humans. For example, evolutionary theory predicts that males should be more prone to the emotion of jealousy than females (Cartwright, 2000). In terms of parental investment, in the ancestral environment the consequences of a mate’s “infidelity” would have been far more serious for a male than a female. For a male, a female mate’s liaison could mean he spends the rest of his life expending his energy and resources on another man’s child. For the female, however, “given the certainty of maternity,” the cost is merely in the fact that “a male partner may be expending resources elsewhere when they could be devoted to herself and her offspring” (Cartwright, 2000, p. 282). Certainly, there is substantial evidence that jealousy is a major factor in male crimes of violence (Wilson & Daly, 1992). It is conceivable then, that what was once an adaptive strategy, i.e., mate guarding motivated by sexual jealousy, is now considered excessive by modern standards and labeled as “pathological.”[3]

The other evolutionary explanation for psychopathology is that it may represent a byproduct of an evolved adaptive mechanism, rather than the mechanism itself. Thus, it can be seen as analogous to the way that effective drugs also can have harmful side-effects. For example, Symons (1979) has suggested that rape may be a byproduct of a number of evolved characteristics of male sexual behavior including: greater arousal to visual stimuli, greater autonomous sex drive, and a greater desire for varied sexual partners.

Evolutionary psychopathology is concerned with “what animals are designed to do” (Gilbert, 1998a, p. 357). To understand how things go wrong we need to first comprehend the conditions under which behavioral and psychological mechanisms evolved as adaptations that increased the individual’s chances of replicating. This is made difficult by the fact that most human evolution took place over about 6 million years, during which our ancestors lived mostly in small groups of hunter-gatherers, under environmental conditions very different from our current one. Moreover, adaptations may appear to be inefficiently organized when they represent compromises between competing functional requirements. They are trade-offs between competing functions or demands upon the organism (Gilbert, 1998a). Nesse and Williams (1997) describe some of the diverse ways in which an adaptive cognitive mechanism might be implicated in any form of psychopathology thus:

> …many psychiatric symptoms turn out to not to be diseases themselves but defenses akin to fever and cough. Furthermore, many of the genes that predispose to mental disorders are likely to have fitness benefits, many of the environmental factors that cause mental disorders are likely to be novel aspects of modern life, and many of the more unfortunate aspects of human psychology are not flaws but design compromises. (p. 3)

Thus, in beginning to understand the causes of a mental disorder, we must first try to be clear which adapted cognitive mechanisms are implicated in the psychopathology. However, we cannot simply infer that the disorder represents a breakdown of these adaptive mechanisms. We must also consider the possibility, either that it represents a functioning adaptive mechanism “designed” for the hunter-gatherers of 2 million years ago, or alternatively, that we are simply observing the byproducts of the relevant mechanisms. To further complicate this picture, many adaptations are not so much elegant design solutions to adaptive demands, but rather compromises between conflicting environmental demands.

Notwithstanding the promise that an evolutionary perspective on psychopathology offers, much of EP remains highly speculative and in need of empirical support. Consequently, in the next section, I will briefly outline how an evolutionary perspective has begun to shed new light on one specific mental disorder, namely, depression. There are numerous other examples that, space permitting, I could also have included, such as autism (Baron-Cohen, Leslie, & Frith, 1985), panic disorder and agoraphobia (Nesse, 1997), anorexia nervosa (McGuire & Troisi, 1998; Mealey, 2000), dysthymic disorder (McGuire & Troisi, 1998), phobias (McGuire & Troisi, 1998), personality disorders (Millon & Davis, 1996) and homicide (Daly, Singh, & Wilson, 1997).

AN EVOLUTIONARY PERSPECTIVE ON DEPRESSION

Depression is a mental disorder characterized by symptoms that can include sadness, pessimism, despair, guilt, anhedonia, fatigue, sleep disorder, feelings of extreme worthlessness, concentration difficulties, and sometimes suicide. It is an extremely common illness. The lifetime prevalence reported in surveys of community samples typically varies from 5% to 12% for males and from 10% to 25% for females (APA, 1994). The high prevalence of depression, and the fact that it frequently involves somatic symptoms (i.e., reduced appetite, reduced libido, and sleep disturbance), means biological explanations have been influential upon models of etiology and modes of therapy (Shelton, Hollon, Purdon, & Loosen, 1991). Perhaps as a consequence, evolutionary explanations have been relatively popular for explaining at least some aspects of depression (e.g., Gilbert, 1998b; McGuire & Troisi, 1997; Price, 1967). In fact, within the field of evolutionary psychopathology there are a number of different accounts of depression and the interested reader is referred to McGuire and Troisi (1997) for an overview. We will consider just one model of depression based upon evolutionary concepts, namely the Social Competition Hypothesis advanced by Price, Sloman, Gardner Jr., Gilbert, and Rohde (1994). Furthermore, I will only address unipolar depression but for an evolutionary explanation of bipolar disorder, see Wilson (1998).

The Social Competition Hypothesis of Depression

Price, Sloman, Gardner Jr., Gilbert and Rohde (1994) have offered an evolutionary account of depression in which depression is considered to have evolved in relation to social competition as an

> unconscious, involuntary losing strategy, enabling the individual to accept defeat in ritual agonistic encounters and to accommodate to what would otherwise be unacceptably low social rank. (Price et al., p. 241)

Such a model is based upon the idea that depression serves an adaptive function and that the function of depression is in relation to social competition.

Price et al. hypothesize that depression evolved as a “mechanism for yielding in competitive situations” (p. 242). It is considered an involuntary mechanism that inhibits aggression toward more dominant individuals and signals to those competitors that the individual is not a threat. It is also posited to produce a state of mind conducive to giving up and accepting the situation, which Price et al. call “voluntary yielding.” Price et al. suggest that this mechanism of depression evolved out of “ritual agonistic behavior,” which they point out is the primary form of social competition underlying sexual selection in most vertebrates. They argue that agonistic behavior is closely related to a self-concept known as *resource-holding potential,* which equates with the fighting capacity of an individual as perceived by that individual and by others. Price et al. even go as far as to hypothesize that self-esteem evolved out of resource-holding potential.

In some ways this all seems rather removed from our contemporary social context where physical combat is rarely used in settling disputes. However, the theory becomes less esoteric when we note Price et al.'s comment that "ritual agonistic behavior is not the main form of human social competition" any longer but rather that "competition by attraction has largely replaced competition by intimidation" (pp. 245–246). Thus we can no longer gain social status simply by physically dominating our fellows (except perhaps in the sporting arena and certain criminal subcultures), but rather we must rely upon our interpersonal skills, personality, physical attractiveness, etc. Therefore, it follows that depression may represent a failure to achieve or maintain status and control in the interpersonal realm and a subsequent withdrawal both physically and psychologically

It is also worth noting that Price et al. state that the simplest hierarchy is the two-person relationship. They suggest that in many dyads, the depressed person occupies a permanently "one-down" position in the relationship, which serves to avoid a psychological "arms race." The depressed persons status is chronically diminished, whereas the partner's is magnified, and this is associated with cognitive distortions in the depressed partner. Such a scenario may already be familiar to many clinicians reading this chapter, but the evolutionary account, we believe, is quite novel. Thus, although the evolutionary model of depression brings a new theoretical perspective to the clinic, the issues of status, self-esteem, dominance, and control within relationships, which the model predicts will be salient in depression, are familiar to most clinicians working with people with depression.

Another interesting corollary of the social competition hypothesis is the suggestion from Price et al. that "depression functions to inhibit aggression" (p. 249). This hypothesis is somewhat reminiscent of the traditional psychoanalytic notion that depression involves anger turned inward against the self, a hypothesis that has not been empirically supported, and some evidence even exists suggesting that depressed people may express more anger than controls (Davison & Neale, 1998). The social competition hypothesis suggests a more complex picture. In particular, it states that expressing hostility up a hierarchy has very different meaning and consequences from expressing it downward. Price et al. state that "only hostility to equal- and higher-ranking people is inhibited, whereas hostility expressed to lower ranking people is often increased" (p. 249). Consequently, we expect to see more complicated formulations than that simply repressed hostility leads to depression. For example, a man who finds it difficult to be assertive at work with superiors and senior colleagues may express his hostility to his wife or children.

There are admittedly some problems for this model. An obvious one is how to explain the frequent observation that depression is more common among women than men, given that agonistic behavior is generally considered more common among males. Price et al. suggest, in relation to this issue, that agonistic behavior may be more conspicuous, rather than more frequent, among males. They also cite evidence that the gender difference decreases as equality between the sexes increases, an observation consistent with a social competition hypothesis. Another problem with this theory is that depression is not restricted to persons of low rank or social status (although it is more common among people of lower socioeconomic status) (Davison & Neale, 2001). Many people in subordinate positions in society never experience depression, whereas depression can affect politicians, rock stars, and business moguls. Stevens and Price (2000) respond to this criticism by arguing that it is the individuals perception of their social value or "social attention holding power." They note that very successful individuals who become severely depressed frequently regard themselves as worthless, incompetent failures, even in the face of considerable objective evidence to the contrary.

Certainly, an evolutionary theory of depression promises considerable heuristic power in generating testable hypotheses about the phenomenon. Nesse (2000) has suggested a number of specific and testable hypotheses that arise from evolutionary accounts of depression such as the social-competition hypothesis. For example, he suggests that depression is precipitated "not so much by life events that involve loss but by those that are humiliating or entrapping" (p. 17). Another hypothesis that he suggests could be easily tested experimentally is that "Low self-esteem should be an especially prominent part of those depressions that arise from inability to yield in a status competition" (p. 18). These are just two of several testable hypotheses that Nesse advances. The point at issue here is that evolutionary explanations of psychopathology are not just about the telling of "just-so" stories. Their usefulness must also be tested empirically, and this is occurring.

In summary, Price's social competition theory of depression is consistent with a wide range of data from both epidemiology and animal models of depression, and Price has suggested a number of implications for both research and treatment (Price et al, 1994). At the same time, the theory remains somewhat speculative and it would be valuable to see more experimental research testing its implications. To date, much of the evidence that Price and colleagues muster is descriptive or correlational, such as epidemiological patterns. Although this evidence is consistent with the social competition hypothesis in many respects, it does not establish the direction of causality. It is quite plausible that becoming depressed results in decreased social rank rather than the other way around. It would be interesting, for example, to examine in the laboratory whether manipulations of social rank produce decreases in mood. Indeed, empirical evidence of this kind is now accumulating, and there is a growing body of support for the social rank hypothesis in the development of depression, which is particularly evident in the work of Paul Gilbert and colleagues (e.g., Allan & Gilbert, 2003; Gilbert, 2000; Trower, Sherling, Beech, Harrop, & Gilbert, 1998). Similarly, Kip William's research on ostracism has repeatedly demonstrated that those individuals who experience rejection by other group members frequently show increased anxiety and lowered mood (Williams & Zadro, 2001).

SOME IMPLICATIONS FOR CLINICAL PSYCHOLOGY OF AN EVOLUTIONARY PERSPECTIVE

Taking an evolutionary perspective on clinical psychology has important implications for developing etiological theories, for assessment, and for treatment. I briefly address each of these domains.

Implications for Theory and Etiology of Mental Disorder

Perhaps the major contribution of adopting an evolutionary perspective on mental disorder is in relation to developing robust and meaningful etiological models. Mental disorders represent a vast and bewildering array of signs and symptoms that seem to defy any simple approach to their taxonomy. For example, schizophrenia is a common and disabling disorder; it occurs in about 1% of the population throughout the world, and yet there is no one symptom that everyone must display who merits the diagnosis. Heinrichs (1993) has called this the "heterogeneity problem." One outcome of this confusion has been a proliferation of new diagnostic labels. In fact, a major constraint on the development of the *DSM-IV* was concern at the proliferation of new diagnostic categories with each revision (see Clarke, Watson, & Reynolds, 1995).

To complicate things further, there is no generally agreed upon etiological perspective to explain this ever expanding catalogue of mental disorders. To the contrary, individual clinicians subscribe to psychodynamic, cognitive-behavioral, biological, or family systems models of dysfunction. Theoretical pluralism is the order of the day. Reflecting this theoretical agnosticism in the marketplace, the development of recent versions of the *DSM* have striven for operational definitions of mental disorders that mostly ignore the issue of etiology (save for the indisputably "organic" disorders). This has even produced a somewhat Kafkaesque situation whereby each new version of the *DSM* is followed by dozens of research studies on the technical merits (reliability and validity) of the new diagnostic categories. Such research may produce publications but it does little to explain the phenomenon of concern nor does it often generate meaningful theory. At its worst, this type of research makes the diagnostic manual itself the focus for research, rather than those naturally occurring phenomena (i.e., mental disorders) that it is supposed to facilitate research on.

An evolutionary perspective may help temper such excesses by providing a broad and unifying theory that integrates the study of psychopathology and connects it with other biological and social sciences. It allows for the integration of data from a variety of etiological models (genetic, biochemical, developmental, cognitive, etc.) through its emphasis on the role of ultimate causes and their interaction with proximate causes. Much of the debate surrounding competing explanations of mental disorder is concerned with different explanations of the proximate causes of an illness. For example, in explaining a case of depression we can look to the person's developmental history for vulnerability factors, or we can examine the current cognitions for depressogenic schemata, or we might attribute their depression to serotonin imbalance or depletion in the central nervous system. What is more, all three approaches have their value in explaining some aspects of the disorder. However, only ultimate (i.e., evolutionary) causes explain *why* humans become depressed in the first place.

Nesse (2000) has commented in this regard that, "questions about why the capacity for mood exists at all are separate and distinct from proximate questions about the psychological and brain mechanisms that cause low mood" (p. 15). Nesse argues that there are many important questions about depression that cannot be answered until we have a better understanding of the evolutionary forces that have resulted in a neuronal/endocrine/behavioral system in humans that gives them the capacity for low mood. According to Nesse, questions such as, "When does low mood become pathological depression?" will only become answerable when we are clear on the evolved function of mood states. At present Nesse argues that we can't be sure whether depression even reflects a defective mechanism, an adaptation, or a dysregulated defense. However, he mounts a persuasive argument that understanding the ultimate causes of depression will be necessary before we can gain a full understanding of the disorder.

Moreover, ultimate explanations are important because they constrain the form of proximate explanations, and in a sense, dictate what is possible. For example, if the ultimate cause of depression is related to an individuals lack of status, then relevant proximal causes might plausibly involve appraisal mechanisms or beliefs concerning the individual's lack of ability or value. Appeals to mechanisms that were associated with unrelated causes, such as anger turned inward or a loss of existential meaning, would only be reasonable (in this view) if they were byproducts of, or causally activated by, the more central proximal causes. In this case, these are self-appraisal mechanisms and core beliefs concerning self-worth and status.

Unless a theory of depression can invoke ultimate as well as proximate causes, and attempt to detail their interaction, it can at best only be a partial explanation of the phenomenon. Thus an evolutionary perspective on mental disorder offers greater theoretical unity and integration and a more comprehensive account of the phenomenon of interest. It also provides a framework for more rigorous theory development. Explanations for a disorder must not only make logical sense within themselves (i.e., show internal coherence), but also must be consistent with the broader facts of hominid evolution (i.e., show external coherence). As an example, Thornhill and Palmer (2000) have argued that Freud's Oedipal theory would have quickly died if required to meet this criterion. Put bluntly, any species in which all males experienced a desire to mate with their mothers (conscious or unconscious) would have rapidly bred itself into extinction.

Classification is essential to our understanding of psychopathology and to the construction of good theory (Millon, 1991). A reliable and valid classificatory system also is essential for designing and evaluating treatments, and in the prediction of future risk. An approach to classification and etiology from an evolutionary perspective has been advanced by McGuire and Troisi (1998). They suggested grouping disorders that have "similar functional consequences within the same behavior system" (p. 151). They argue for four such behavior systems: the *reproductive behaviour system,* the *survival behavior system,* the *kin investment behavior system,* and the *reciprocation behavior system.* Thus, because anorexia nervosa, dysthymia, sexual and gender identity disorder, hypoactive sexual desire disorder, sexual abuse of a child, and histrionic personality disorder all decrease the frequency of normal sexual functioning, they are clustered together within the *reproductive behavior system* category. Depression, it is argued, comes under a fifth category of *mixed behavior systems* —in which two or more behavior systems are likely to have been compromised.

According to McGuire and Troisi, the principal advantage of such an approach is that groupings based upon functions of a behavior system, or functional failures, are explicitly tied to causal hypotheses concerning etiology. They note that their provisional classification system is informative in two important ways. First, the disorders grouped by behavioral system all share similar features. For example, the disorders grouped under the *survival behavior system* are all characterized by strong feelings of personal danger and exaggerated responses to fearful situations. Second, the disorders within each group are likely to share similar ultimate but not proximate causes. McGuire and Troisi note that their model is most consistent with complex, multiple variable explanations of mental disorder. For example, they review three different evolutionary accounts of depression, one that emphasizes decline in status, one that emphasizes interpersonal conflict, and one that emphasizes reaction to loss. They note that these three different evolutionary explanations for depression are highly compatible rather than mutually exclusive or conflicting accounts.

In summary, I have described recent examples of an evolutionary approach to the definition and to the classification of mental disorder. Both are characterized by a strong theoretical framework that stands in contrast to the theoretical agnosticism of the *DSM-IV.*

Some Implications for Treatment

Perhaps the most fundamental influence upon treatment will spring from adopting a rather different concept of mind from that advanced under the

Standard Social Science Model. Instead of a general computing device molded largely by culture, we now envisage a more specialized biological computer that has evolved to perform a number of specific functions. Thus in designing therapeutic interventions we may need to be more specific about which cognitive systems they are designed to influence or interact with. Admittedly, one implication of a strongly modular viewpoint could be that therapy is unlikely to alter a condition such as anxiety or depression, because modules are putatively fast, automatic, domain-specific, etc. Such modules might be thought to be relatively inflexible, difficult to access therapeutically, and resistant to new learning.

However, this pessimistic stance assumes a "massively modular" mind, and as noted earlier, there are different perspectives on how modularity might look. Nonetheless, it does suggest that therapists might need to consider very carefully the best way to provide appropriate "input" to a particular cognitive subsystem. To take one example that has been thoroughly examined empirically—the treatment of anxiety disorders. There is a wealth of robust empirical studies that demonstrate that anxiety problems are best treated behaviorally by exposure-based methods—preferably *in vivo* (Chambless & Ollendick, 2001). In other words, you don't get over your phobia of dogs by sitting in your shrink's office talking about it. You will get over this fear by looking at pictures of dogs, then touching a toy dog, then gradually approaching a caged dog, and so on, and on, in a graduated fashion. This graded exposure technique is fast and efficient in the treatment of phobias and other anxiety problems. The important point here is that no amount of talking about the problem, or so-called "topdown processing," will alleviate the anxiety. We literally have to "walk the talk," preferably with a warm and encouraging therapist beside us. Our "fear-alarm" systems do seem fairly modular in nature. We can't talk them out of being afraid. That 's the point about phobias—the fear is irrational, and the person with a phobia is only too aware of that fact, but he or she still feels afraid. However, we can provide the right kind of input to the "fear-alarm" system in graduated doses and see new learning occur.

A quite different example of this kind of specificity exists in some recent work with autistic children. A considerable body of evidence has accumulated to suggest that autistic children are particularly poor at comprehending the workings of other people's minds—so called mind-reading ability. Moreover, Baron-Cohen (1995) has proposed that this might reflect a failure in a module specialized for understanding the intentions, beliefs, desires, and motivations of other people. Such a specialized ability would have had obvious survival value for humans who existed for millions of years in small social groups as hunter-gatherers. Some recent work has attempted to train these abilities in people with autism. The results to date are admittedly somewhat mixed (e.g., Hadwin, Baron-Cohen, Howlin, & Hill, 1997; Reinecke, Newman, Kurtz, Ryan, & Hemmes, 1997). What is clear from this work, nonetheless, is that an understanding of the disorder in terms of the specific cognitive mechanisms involved has direct implications for developing new treatment approaches.

Another implication for treatment is that we may also have to adopt a different and more complex model of mental disorder. Current models of pathology in CP frequently conceptualize mental disorders as either illnesses or learned behaviors. For example, panic disorder can be regarded as a mental illness. It has specific signs and symptoms, there is some evidence for genetic correlates, and it can be treated with drugs. Alternatively, it can be viewed as primarily a behavioral problem in which vulnerability factors, combined with a particular learning history, mean that the individual learns to respond to certain environmental cues with panic. Both of these approaches to anxiety begin with the assumption that high levels of anxiety are abnormal, unhealthy, and pathological.

However, Nesse and Williams (1995) argue that from an evolutionary perspective both approaches may be in error in regarding strong emotions as strictly pathological. For example, in ancestral environments anxiety functioned as an alarm system that aroused us to possible threats and forced us to take immediate action. In other words, anxiety had (and in many cases still has) survival value. If you are sleeping in the wild and waken at night to the sound of a snapping twig, there is survival value in taking immediate action (freezing, fleeing, or fighting). As Gilbert (1998b) has argued, in such situations there is little cost for false alarms.[4] However, to remain calm and demand further evidence of a real threat before responding could well result in death. Thus in many situations we react with emotions that are both immediate and powerful, because those emotions have previously had adaptive survival value.

Nesse and Williams assert that many aspects of mental disorders, such as anxiety, are actually defensive systems in the same way that a cough is a defense against accumulated phlegm in the lungs. This is not to suggest that all anxiety is useful or that we understand anxiety as well as we understand the common cough. It does suggest, however, that anxiety is normal, that it has been adaptive in past environments, and that we cannot simply assume it is the byproduct of some aberration in brain chemistry. No doubt the same arguments could apply to other disorders as well. In fact, searching for abnormalities in the brains of anxiety sufferers may be a complete red herring. Nesse and Williams caution that this could be analogous to studying the purpose and function of coughing by investigating the respiratory systems of people with severe coughs. That is, a cough is a defensive reaction to a multitude of infectious and immune disorders, not a disorder in its own right. Again, an evolutionary perspective implies a radical shift in our thinking concerning the nature of mental disorder. Many of the “maladaptive behaviors” that fill the pages of undergraduate texts on abnormal psychology may actually be behavioral systems that had survival value and have been selected by evolution over millions of years.

Gilbert (1998b) makes a similar argument in relation to cognitive distortions, noting that human cognition evolved to react rapidly to threats, both social and nonsocial in nature. Thus much of human cognition is not fundamentally rational in nature and, as EP suggests, it is also domain specific and fast. Gilbert argues that in depression, for example, it is not so much a question of learning to think in an irrational or depressogenic style, but rather that the information processing algorithm concerned has been activated. In other words, cognitive therapy might be not so much about learning some "natural and normal" rational mode of thought that we somehow missed out on—but rather more about learning to turn the volume down on an overactive but quite natural defensive system.

Gilbert suggests that this might explain how people with no history of depression or vulnerability factors can nonetheless become depressed and display the full spectrum of cognitive distortions, in response to traumatic head injury or certain medications. One implication of this notion, according to Gilbert (1998b, p. 459), is that "cognitions are far more socially contextualized than currently considered." He seems to be saying that we can all become depressed because of our evolutionary past, but we should look at the social environments of those who do become depressed to see what activates this potential, rather than focus too much on the thinking style of the depressed person. In this regard, it is interesting to note how much importance proponents of an evolutionary perspective on depression, place on the depressed person's current relationships and interpersonal environment—especially when one considers how critics of evolutionary psychology (e.g., Rose & Rose, 2000) constantly accuse them of a narrow biological reductionism. In fact, the message from evolutionary psychology is that we all have the potential for depression because of our species' history, but if we want to treat a depressed person we need to help him or her explore the current social nexus with a view towards developing more satisfying relationships and greater interpersonal effectiveness. Consequently, an evolutionary perspective on therapy might entail social skills or assertiveness training, marital or couples therapy, and procedures that enhance self-esteem and confidence.

A further implication for treatment that Gilbert mentions is that some therapists may want to share evolutionary explanations with their clients or patients. He suggests that cognitive therapy can be explained partly as learning to switch off or attenuate normal defensive mechanisms. This means there is less need to hold up some ideal standard of rational thinking as the norm, and in doing so emphasize the patient's own thoughts as irrational, distorted, and inadequate. Rather, such distorted cognitions can be normalized so as to reduce the client's existing feelings of failure, inadequacy, and unworthiness. At the same time it can be clearly stated to the client that although a tendency toward such thinking is normal in its origins, in the current environment it is self-defeating because it is too intense, too prolonged, and too pervasive.

SUMMARY AND CONCLUSIONS

The present chapter has argued that evolutionary psychology is a major development in contemporary psychology that has been largely ignored by clinical psychology. The history of clinical psychology shows that Darwin's ideas were quite influential in the early history of clinical psychology, especially in the mental testing movement, but also in both the early Freudian and Behaviorist theories. However, by the 1920s and 1930s this influence had begun to disappear in clinical psychology, and it did not re-emerge until the early 1970s, when animal models of psychopathology become increasingly popular. Similar trends can be observed in psychiatry and the social sciences. The value of incorporating evolutionary concepts into the understanding of mental disorders was illustrated with reference to the emerging field known as evolutionary psychopathology. The merits of evolutionary explanations for mental disorders were illustrated with reference to the social competition model of depression. Evolutionary psychopathology is a multidisciplinary field in which several of the major contributors have been psychologists, however, its impact on the field of clinical psychology remains to be seen. I conclude that clinical psychology can no longer afford to ignore the important theoretical insights offered by evolutionary psychology and evolutionary psychopathology. In particular, an evolutionary perspective on mental disorder has important implications for theories concerning etiology, diagnosis and assessment, and therapy. It is time for a closer dialog between these two scientific disciplines and I hope that we will soon see the emergence of an evolutionary clinical psychology.

NOTES

1. Clinical psychology is both an academic discipline and also a profession. The academic side of clinical psychology involves teaching and research that occur primarily in University departments. The great majority of clinical psychologists, however, are practitioners who are rarely active researchers. The two strands of clinical psychology, the academic and the clinical, are united by a common commitment to a scientist-practitioner model of the discipline. Such a model views the clinical psychologist as trained in both clinical skills and the principles of scientific research. By contrast, evolutionary psychopathology is an emerging field of enquiry which has largely grown out of academic and research departments from a range of fields including psychiatry, clinical psychology, behavioral genetics, anthropology, and developmental psychopathology. We discuss evolutionary psychopathology here to illustrate the potential of an evolutionary perspective on mental disorder—a perspective that has been largely ignored by clinical psychology.
2. As noted earlier in this chapter there are several different perspectives on evolutionary psychology and they do not all demand adherence to the doctrine of modularity. Moreover, it is evident that notions of modularity also vary considerably. For example, the evolutionary psychology of Tooby and Cosmides (1992) is frequently thought to represent a "massively modular approach," the so-called "Swiss army-knife model." By contrast, Sperber (1994) proposes a more flexible modularity in which modules might be richly interconnected. In this view processing within a specific module is domain-specific and self-contained, but other modules might then have access to the outputs. Sperber suggests that this allows

for meta-repreentational thought and can account for the flexible and plastic nature of human cognition and behavior.

3. The author is grateful to the editors for suggesting this example.
4. A problem with all evolutionary explanations of behavior is that we simply don't have a precise knowledge of the ancestral environments. It could equally well be argued that in a hostile environment false alarms could have been very costly—by giving our position away to real enemies or predators.

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11

Evolutionary Psychology and the Challenge of Adaptive Explanation

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INTRODUCTION

I (R. G.) often start my undergraduate lectures on evolutionary psychology with the following question, “Who thinks humans are animals?” After a few moments of reflection (and a bit of prompting from me that people are certainly not vegetables or minerals), most students agree that humans are indeed animals. I then ask, “How many people think humans are the product of evolution?” Typically around 80% of the class think humans are products of evolution. However, when I then ask, “Given that humans are products of evolution, how many of you think that human behavior can be explained in evolutionary terms?” only a few brave souls raise their hands. This ambivalence to evolution and evolutionary explanations is not restricted to Auckland undergraduates. Many biologists, who routinely explain all sorts of biochemical, morphological, and behavioral aspects of organismal diversity in evolutionary terms, balk at explaining human behavior and cognition in the same way (Ahouse & Berwick 1998; Gould, 1997; Lewontin, 1998). Coyne (2000) expresses this view with considerable vigor.

> The latest deadweight dragging us (evolutionary biology) closer to phrenology is evolutionary psychology, or the science formerly known as sociobiology. If evolutionary biology is a soft science, then evolutionary psychology is its flabby underbelly.

Why are many evolutionary biologists comfortable giving evolutionary explanations to differences in lactate dehydrogenase activity in humans, but decidedly uncomfortable with evolutionary explanations of human rape? To some it appears that these evolutionary biologists suddenly become creationists or cultural determinists when the topic turns to human behavior (Sarich, 1995).

However, another possibility exists. The view we defend in this chapter is that there is nothing wrong *in principle* with taking an evolutionary approach to human behavior or cognition. In practice, however, the impoverished view of evolution and psychology adopted by many evolutionary psychologists, and the weakness of their empirical science, is frankly rather embarrassing (see Lloyd, 1999; Lloyd & Feldman, 2002 for a similar perspective on evolutionary psychology).

Before proceeding any further we should emphasize that the target of our critique here is *not* a broad, comparative evolutionary approach to psychology (evolutionary psychology or "evolutionary psychology in the round")- Instead, our attack is confined to the specific program of Evolutionary Psychology associated with the "Santa Barbara church of psychology" (Laland & Brown, 2002). Evolutionary Psychology (EP) is a nativist approach to cognition that views the human mind as a collection of modules design by natural selection to solve the problems faced by our Pleistocene ancestors. This program was christened in the *Adapted Mind* book (Barkow, Cosmides, & Tooby, 1992), and proselytized to the lay public in Steven Pinker's (1997) modestly titled book, *How the Mind Works.* Its followers have applied EP doctrines to everything from social reasoning to preferences for green lawns and certain genres of erotic fiction ("slash" novels—see Symonds & Salmon, 2001). EP's narrow focus on unique human adaptations, a monomorphic view of the mind, massive modularity, and a cartoon view of the Pleistocene environment have all been the subject of considerable critique (Fodor, 2000; Griffiths, 2001; Hull, 2002; Irons, 1998; Karmiloff-Smith, 2000; Samuels, 1998; Stolz & Griffiths, in press). Here we assess how successful EP has been in meeting the challenge of that most quintessentially Darwinian task—adaptive explanation.

THE CHALLENGE OF ADAPTIVE EXPLANATION

EP is adaptationist and proud of it. The standard explanatory strategy followed by Evolutionaary Psychologists has been dubbed "reverse engineering" (Pinker, 1997). In normal engineering humans attempt to design solutions to problems. In "reverse engineering" Evolutionary Psychologists take current features of human cognition and posit that they are adaptive solutions shaped by natural selection to problems posed by life back in our environment of evolutionary adaptedness (Tooby & Cosmides, 1992). This might be a good explanatory strategy if three criteria were commonly satisfied.

1. All traits were adaptations.
2. The traits to be given an adaptive explanation could be easily characterized.
3. Plausible adaptive explanations were difficult to come by.

If these criteria apply, then a plausible adaptive explanation of a trait would have a high probability of being correct. Taken to an extreme, if only one plausible adaptive explanation was possible, then any adaptive explanation encountered would have to be correct. Sadly, life is not so easy for evolutionary biologists. The challenge of adaptive explanations is that all three of these criteria are frequently violated.

Are All Traits Adaptations?

Evolutionary theory has changed considerably since Gould and Lewontin (1979) attacked the Panglossian program that regarded "natural selection as so powerful and the constraints upon it so few that…its operation becomes the direct cause of nearly all organic form, function, and behaviour" (pp. 582–583). Even the most ardent adaptationists do not defend the claim that all features of organisms are the direct products of natural selection. (See Dawkins, 1986 for a defense of what Godfrey Smith [2001] terms "explanatory adaptationism" rather than panadaptationism). Nowadays processes such as genetic drift, pleiotropy, and epistasis are well known and the evolutionary literature is replete with discussions about spandrels and exaptations and analyses of developmental constraints and phenotypic integration. The debates within evolutionary biology are thus not about the existence of these phenomena, but rather about their relative importance and our ability to disentangle their effects.

What Is the Trait?

The correct characterization of traits to be given adaptive/historical explanations is by no means obvious or straightforward. Lewontin (1978) illustrated this with the now-classic example of the human chin. Although the chin may seem like an obvious trait, present in humans and absent in our ape relatives, the chin is actually not a discrete trait at all. It is the consequence of different degrees of neoteny in the dentary and alveolar growth fields of the lower jaw. Individuating traits in terms of the problems they are solutions to does not solve this difficulty. The same issues apply to correctly characterizing environmental problems. For example, Sterelny and Griffiths (1999) observe that mate choice could be considered as either a single problem (choose a good mate) or an ongoing mosaic of problems (choose a mate with the ideal waist-to-hip ratio, choose a mate who is kind, choose a mate who is rich, choose when to be unfaithful to your mate, choose when to desert your mate). The correct classification of traits to be given adaptive explanation is thus an important and nontrivial issue.

Are Adaptive Explanations Hard to Come By?

If the first two criteria pose some difficulties, the third is even worse. Plausible adaptive explanations are far from rare. Don Rosen once quipped that only two factors seem to constrain adaptive explanations—the inventiveness of the author and the gullibility of the audience (Rosen, 1982, p. 271). Although this might be a little harsh, it is certainly not uncommon to have numerous plausible adaptive explanations for a trait. For instance, the series of large bony plates found on the back of the dinosaur *Stegosaurus* were variously proposed to be an adaptation to avoid predation, attract mates, or cool the body (Lewontin, 1978). Similarly, speculations on the adaptive significance of evolutionary increases in hominid brain size include ecological problem solving (Clutton-Brock & Harvey, 1980), the aquatic ape hypothesis (Morgan, 1982), serial cognitive processing in throwing objects (Calvin, 1983, 1993), the social brain hypothesis (Byrne & Whiten, 1988), and thermoregulation (Falk, 1990). The fact that a plausible adaptive explanation can be conceived of does not therefore mean that it must be correct. In evolutionary biology this is where the real work starts, not ends.

Therefore, given the obvious reality of adaptive design (Lewontin, 1978), how do evolutionary biologists tackle these issues? How do they meet the challenge of adaptive explanation? A quick scan through recent issues of the journals such as *Evolution* or a flick through books such as Rose and Lauder's (1996) *Adaptation,* or Orzack and Sober's *Adaptation and Optimality,* reveals that evolutionary biologists today deploy a combination of engineering style optimality models, developmental studies, and phylogenetic analyses to tackle the challenges of adaptive explanation. Developmental and phylogenetic studies help appropriately identify traits and investigate the range of phenotypic variation available for selection to act on. They can also be used to analyze the extent to which traits are coupled, and thus be independently optimized by selection, and infer the relative timing of evolutionary novelties. Optimality models help identify aspects of good design in an explicit quantitative manner (Orzack & Sober, 2001; although see Gray [1987] and Kennedy & Gray [1993] for some concerns about the empirical success of this approach). Most significantly of all, comparative tests based on explicit phylogenetic methods are now routinely used to discriminate between competing adaptive as well as non-adaptive explanations.

A few examples will emphasize this point. Consider the following "promiscuous primate" hypothesis. Profet (1993) proposed that menstrual bleeding, far from being an inconvenient quirk of biology, is actually an evolved mechanism to rid the uterus and oviducts of sexually transmitted pathogens. She noted that this hypothesis predicts that species with promiscuous female mating systems should have more pronounced menstrual bleeding. This was a strikingly novel and plausible hypothesis. Unfortunately for true believers in the infallibility of reverse engineering, this prediction is not correct. Strassman (1996) conducted a rigorous comparative test of the "promiscuous primate" hypothesis. She mapped promiscuity and menstrual bleeding on to an estimate of primate phylogeny based on morphological and molecular data. She used a concentrated changes test to see if menstrual bleeding was more likely to evolve in promiscuous species. She found no statistical association between the two variables. Strassman argued that the phylogenetic distribution of menstrual bleeding was consistent with an alternative mixed adaptive and incidental byproduct explanation. She suggested that given the high cost of maintaining endometrial tissue, it would be more efficient to regress and regenerate the endometrium in a cyclic fashion. As an incidental consequence of this cycling, pronounced menstrual bleeding would occur in species with too much tissue and blood in the endometrium to be reabsorbed. This, she argued, explained why menstrual bleeding correlated with phylogeny rather than promiscuity. The copious bleeding in chimps and humans was a consequence of the large size of the uterus in relation to the body mass of these closely related species.

It could be argued that although physiology might involve a complex mix of adaptation and spandrels, surely for obvious morphological or behavioral adaptations it must be much easier to characterize the selective pressures that have shaped them. The "hammerhead" or "cephalofoil" of hammerhead sharks provides a salutary warning to those who think that evolutionary explanation ends with a plausible story. This structure is a paradigm organ of apparent adaptive complexity The lateral expansion of the head involves substantial changes to the visual, olfactory, and electric field sense organs. It had been suggested that directional selection led to incremental increases in the relative size of the cephalofoil through time. Martin (1993) tested this plausible hypothesis by mapping relative head width on to an mtDNA phylogeny of Hammerhead sharks and their close relatives (Figure 11.1). In contrast to the incremental expansion scenario, he found that the basal *Eusphyra* genus had the largest relative head width. Relative head width remained reasonably constant over the rest of the tree except for the derived bonnet head lineage *(Sphyrna tiburo)* in which it was reduced. Martin (1993) suggested this surprising result might reflect the interplay of two selective forces: one to increase lift and manueverability, and the other to enhance prey detection by separating the sense organs. He outlined a way this more complex adaptive scenario could be investigated using comparative methods.

Let us be very clear here. We are not making an anti-adaptationist argument. We are simply arguing that a plausible story is not enough to meet the challenge of adaptive explanation. Lest it seem that we are arguing that evolution is always perverse ("natural selection works in mysterious ways"), we finish this section with a case drawn from our own work where a plausible scenario was supported by phylogenetic analyses. Van Tets (1965) suggested that wingwaving displays found in Pelecaniforms were derived initially from flight intention movements. These flight intention movements were incrementally elaborated into sky-pointing and then into various forms of wing-waving (Figure 11.2a). We tested this hypothesis by mapping van Tets' scenario as an ordered character state on to a tree based on osteological and DNA/DNA hybridization data (Kennedy, Specer, & Gray, 1996). The incremental evolution scenario fits the tree extremely well, requiring only two more character state changes than the minimal value. A randomization test showed that this result was much fewer than would be expected due to chance. Recent phylogenetic analyses of Pelecaniform mtDNA sequences (Kennedy, 1999; Kennedy, Spencer, & Gray, 2000) further

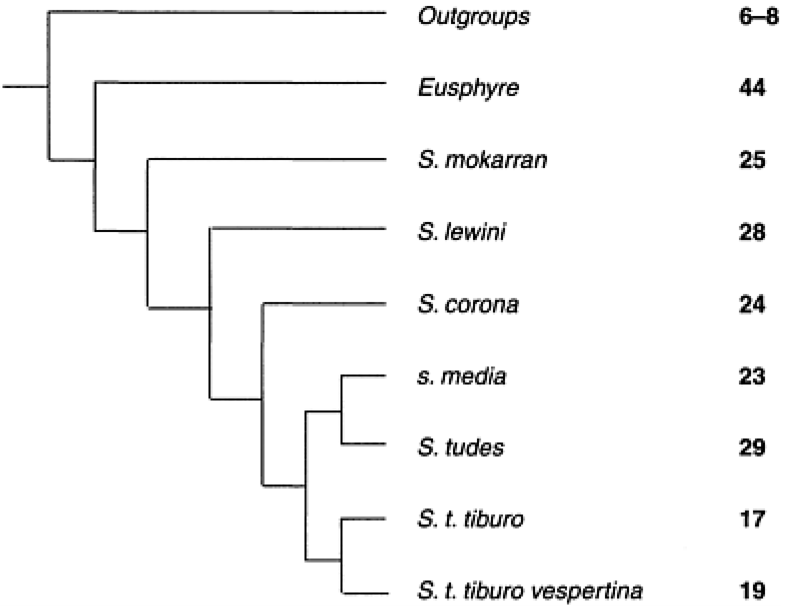


FIGURE 11.1. mtDNA phylogeny of the Hammerhead sharks (adapted from Martin, 1993). Average head widths (scaled as a percentage of body length) are shown beside each taxon. Head width does not increase from smallest in the basal lineages of the phylogeny to largest in S. t.tiburo.

support this initial result, and require only one character state change in addition to those proposed by van Tets (Figure 11.2b).

HOW DOES EVOLUTIONARY PSYCHOLOGY MEET THE CHALLENGE OF ADAPTIVE EXPLANATION?

In the introduction to his classic 1966 book on adaptation, G.C.Williams, the patron saint of Evolutionary Psychologists, notes

> The ground rule—or perhaps doctrine would be a better term—is that adaptation is a special and onerous concept that should be used only where it is really necessary.

This seems to be one doctrine that members of the Santa Barbara church are reluctant to follow. The ability of EP to test adaptive explanation in a rigorous manner by deploying comparative tests is severely constrained by Evolutionary Psychology's doctrinaire focus on unique human adaptations. In contrast to the strenuous efforts of current evolutionary biologists to use adaptive explanations with care, much of EP consists of little more than folk wisdom with a *post hoc* adaptive story (e.g., Buss, 1994). This is exactly why many evolutionary

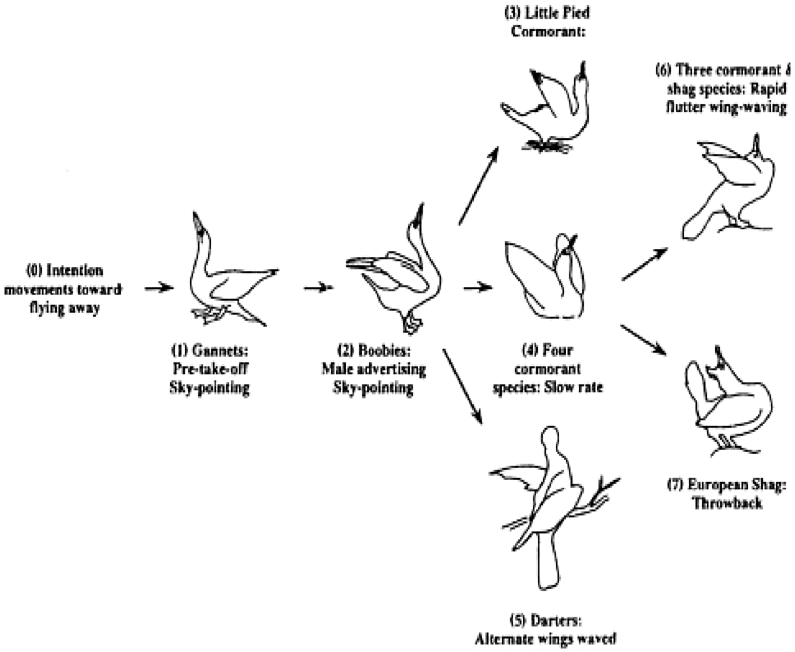


FIGURE 11.2. (a) van Tets' hypothesis for the sequence of evolutionary changes in Pelecaniform male advertising displays. (b) The most parsimonious reconstruction of display changes mapped onto a maximum likelihood tree (Kennedy, 1999). The displays are overlain on the branches they are inferred to have changed on. The numbers correspond to the displays labeled in Figure 2a. These changes all represent the gain of a new display state, except at point 8, where the cross through the display represents a loss.

biologists find their psychological cousin more than a little embarrassing. In the interests of saving evolutionary biologists from future embarrassment we would like to propose two tests—the Grandparent Test and the Lesser-Spotted Brown Gerbil Test. The grandparent test is a filter for folk wisdom with a plausible *post hoc* story. It asks, "Does this work give us any insight into human behavior and cognition beyond popular knowledge?" The Lesser-Spotted Brown Gerbil Test asks, "Would this research be publishable in major international journals if the species was a small noncharismatic mammal rather than our own?" Many studies in evolutionary psychology fail these basic tests (e.g. Buss [1994]; Thornhill & Palmer [2000]; see Coyne [2000] and Coyne & Berry [2000] for critiques). Rather than focus on the worst examples of EP in action, we would like to examine two areas where EP appears to have passed at least the Grandparent Test, and perhaps the Lesser-Spotted Brown Gerbil Test as well. Two of the areas where EP has gone beyond mere folk wisdom, and appears to have generated real insight into human psychology are attractiveness judgments and cheater detection.

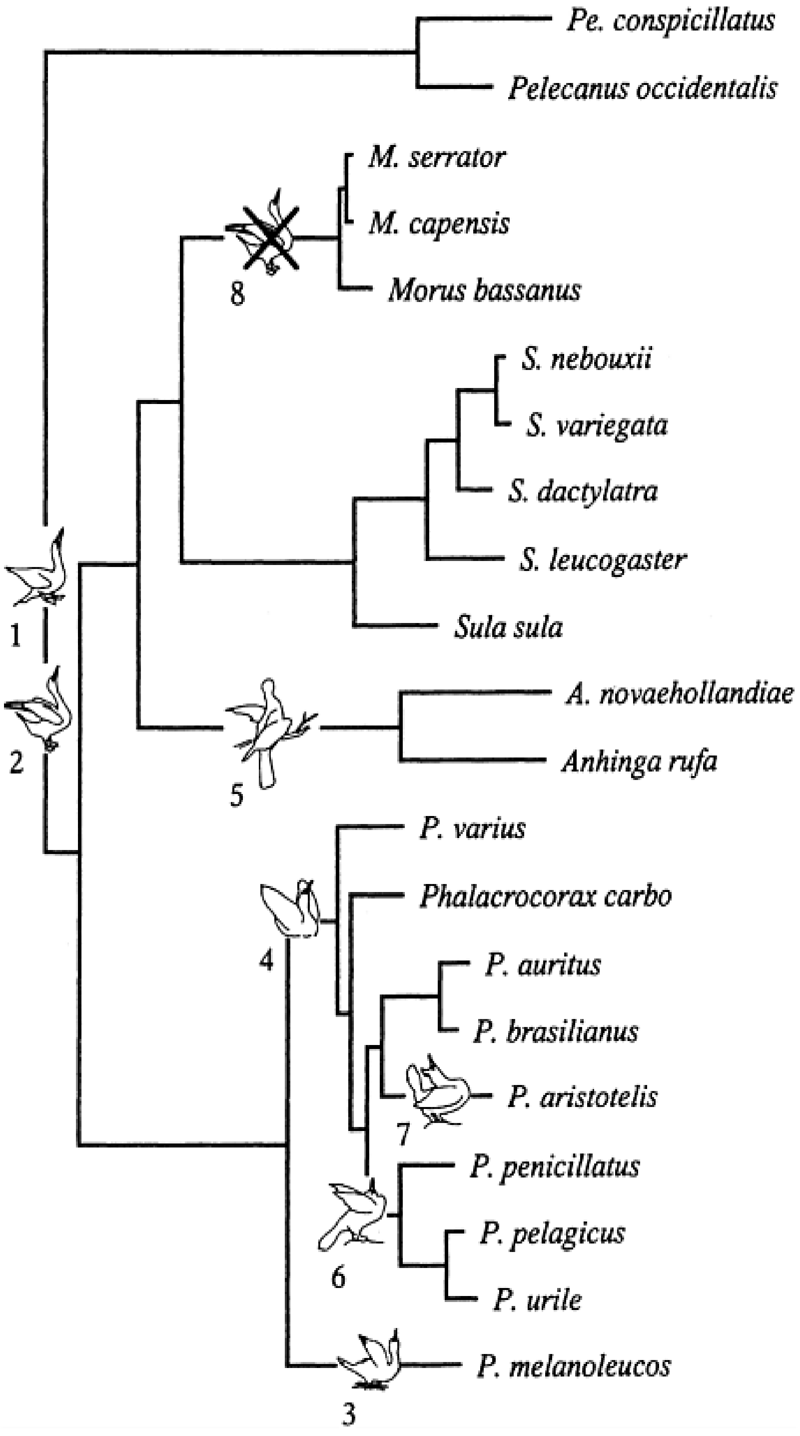


FIGURE 11.2a

ADAPTIVE PREFERENCES FOR FEMALE BODY

SHAPE?

A quick scan through films and fashion magazines reveals that slim is beautiful for women in current Western culture. For example, the body-mass index of fashion models is substantially lower than that of normal women (Tovee et al., 1997). Folk wisdom therefore suggests that weight should be of primary importance in Western male judgments about female body shape. Not so, says Evolutionary Psychologist Devendra Singh. Although Singh (1993) agrees that weight has some influence on attractiveness judgments, he argues that there is a more reliable cue to a woman's potential reproductive success: her waist-to-hip ratio. He cites evidence showing that low waist-to-hip (WHR) ratios are correlated with youthfulness, good reproductive physiology, and long-term health. He suggests, therefore that, "WHR could act as a wide first-pass filter, which would automatically exclude women who are unhealthy or have low reproductive capability" (Singh 1993, p. 304). Singh goes on to argue that

> WHR, the first filter, should be culturally invariant in its significance and its relationship to female attractiveness. The fact that WHR conveys such significant information suggests that men in all societies should favour women with a lower WHR over women with a higher WHR for mate selection or at least find such women sexually attractive. (1993, p. 305)

At first glance this appears to be an excellent example of EP in action. It certainly flies past the grandparent test. Although the idea that an "hourglass" female body shape is attractive is not exactly novel, Singh goes beyond folk wisdom in three important ways. First, he predicts the optimally attractive WHR (0.7). Second, he predicts that this variable is of primary importance (it will act as a "first-pass filter"). Finally, he predicts that it will be a culturally invariant feature of male psychology.

Does this relatively novel, quantitative claim about an evolved psychological basis for male mate selection pass the Lesser-Spotted Brown Gerbil Test? Again, at first glance it does. Singh and colleagues have marshalled an impressive range of empirical studies purporting to show that, regardless of the existing cultural variability of the desirability of plump or thin body types, there is a male preference for female figures with a WHR of 0.7. They appear to demonstrate that this preference is robust across cultures and varying age groups (Henss, 2000; Singh, 1993; Singh & Luis, 1995, Singh & Young, 1995). The primary way they have investigated WHR preferences is by asking their subjects to rank line drawings of women (in bathing suits) on the basis of their attractiveness and a number of other similar variables, such as healthiness and youthfulness. The drawings contained 12 different body shapes—four waist-to-hip ratios (0.7, 0.8, 0.9,1.0) x three weight classes ("underweight," "normal," "overweight")- Across all the weight classes WHRs of 0.7 seems to be the most preferred.

Putting quibbles aside about the ecological validity of this task, even researchers studying the behavior of the lesser-spotted brown gerbil would be impressed with such a robust effect. However, they might also wonder whether this result might be an artifact of the experimental design. Two major methodological concerns have recently been raised about the studies based on Singh's line drawings. Tovée and associates (1999) pointed out the line drawings varied WHR within each weight category by altering the width of the torso. This not only changes the WHR of the stimuli but also the apparent BMI. The stimuli thus confound WHR differences with changes in BMI and so the apparent preference for a WHR of 0.7 could simply be preference for slim body shape. Tovée and colleagues (1998, 1999) investigated the relative importance of these two variables using color photographs of real women dressed in leotards and leggings with their faces obscured. Strikingly, they found that although WHR and BMI were both statistically significant predictors of attractiveness ratings, their effect sizes were vastly different. Variation in BMI accounted for around 74% of the variance, whereas WHR account for about 2%. Folk wisdom 74, Evolutionary Psychology 2.

Tassinary and Hansen (1998) have noted another problem with Singh's line drawings—there are no WHRs below 0.7. It is possible therefore that men might prefer even lower WHRs, but are constrained to select the predicted value of 0.7 by the absence of these body shapes. Tassinary and Hansen (1998) presented their subjects with a set of 27 newly created line drawings, each varying systematically and independently in weight, waist size, and hip size; the lowest WHR being 0.5. They found no consistent preferences for any particular WHR, but found that overweight figures were consistently ranked as least attractive. Tassinary and Hansen's new stimuli have been criticized as less naturalistic and appealing than Singh's (1993) original line drawings (Henns, 2000). It is also possible that the large number of stimuli they used may have reduced the reliability of their participants' rankings because of discrimination problems (Mehrens & Lehman, 1978), and so reduced the impact of any WHR preference. To investigate this possibility, we (MH and RG) (Heaney, 2000) conducted a study with line drawing based on Singh's (1993) original stimuli, but manipulated the waist to include figures with WHR's as low as 0.5 (Figure 11.3). The overwhelming preference of our 147 University of Auckland undergraduate male subjects was figures in the "normal" weight range with a WHR of 0.5. The next most highly ranked shape was a WHR of 0.6 in the "normal" weight range. Only eight out of the 147 subjects assigned their top rank to figures with a WHR of 0.7 in the "normal" weight range. Just two men preferred any of the figures in the "overweight" class.

Given that WHRs of 0.6 and 0.5 are not normally attainable—without the help of a corset or some other form of body enhancing gadgetry—evolutionary explanations might struggle to account for this seemingly "nonadaptive" preference. Why then, were stimulus figures in the "normal" weight range with waist-to-hip ratios of 0.5 and 0.6 ranked as the most attractive body shapes? One

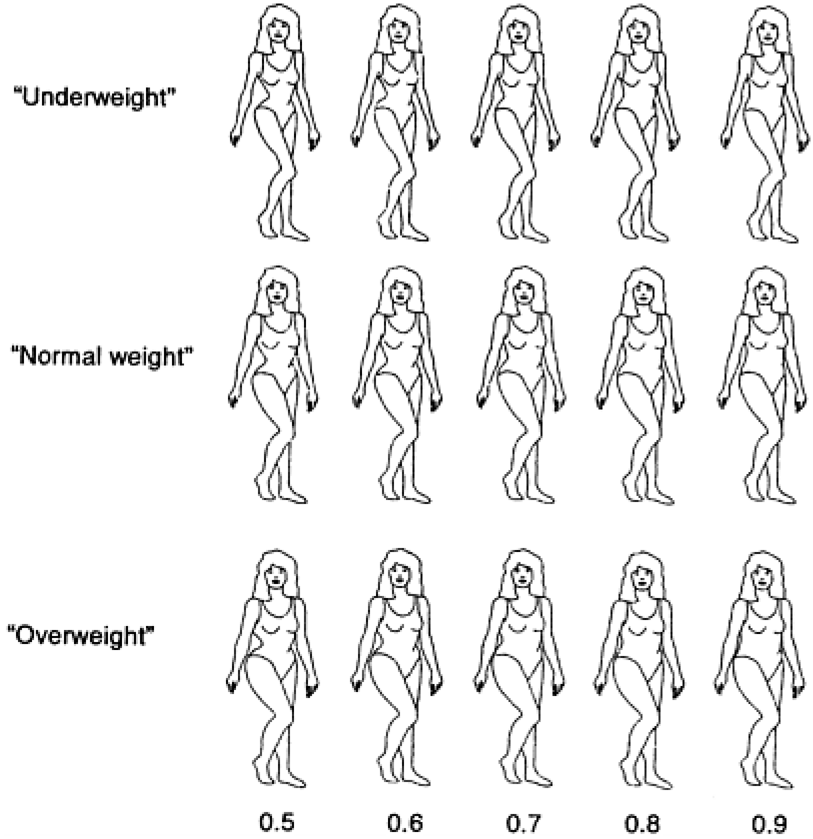


FIGURE 11.3. The line drawings used for Heaney's (2000) study of male WHR preferences. These drawings are adapted from Singh (1993). Unlike Singh's figures, they include waist-to-hip ratios below 0.7. The waist-to-hip ratio range is from 0.5 to 0.9.

potential biological explanation is that this task may tap into a generic psychological mechanism of enhanced responding to exaggerated features, or "supernormal" stimuli (Eibl-Eibesfeldt, 1970). Enhanced responsiveness to supernormal stimuli has been documented in a variety of species. Male fireflies, for example, prefer models of fireflies that contain a larger illuminated area and larger amounts of yellow than is contained in the light of females of their own species. Similarly, when given the choice, the ringed plover prefers to roll eggs that are four times larger than its own into its nest, even though the egg is too big for the plover to sit on and incubate adequately. Finally, the parasitic behavior of the European cuckoo, whereby it lays its eggs in the nests of other birds who then proceed to feed and raise the cuckoo hatchling, often to the detriment of their own young, provides another good example of species responsiveness to supernormal stimuli. The capacious open mouth of the young cuckoo elicits stronger feeding reactions in the foster parent than does the hungry, open mouths of its own young (Eibl-Eibesfeldt, 1970). The WHR of healthy reproductively aged women typically ranges from 0.67 to 0.80, whereas in healthy men it ranges from 0.85 to 0.95 (Singh, 1993). The preference for extremely low WHR's found in our study therefore may be caused by this generic psychological response to supernormal stimuli. WHR is a signal sexual difference. Exaggerating this difference elicits stronger preferences. No specific evolved psychological mechanism for adaptive mate selection needs to be invoked.

Defenders of the EP faith might object that this generic response to supernormal normal stimuli will produce adaptive outcomes when faced with the normal range of female body shapes. This response misses the point repeatedly made by Gould and Lewontin (1979). Not all useful outcomes are produced by specific adaptations. There is no need for natural selection to mold a specific WHR selection mechanism when a generic mechanism already produces the same result. This argument does not deny that enhanced responding to supernormal stimuli may be an adaptation. It merely emphasizes that it is more likely to be an adaptation for a general domain of evolutionary "problems," rather than a specific adaptation for mate choice. This domain-general mechanism might exhibit considerable plasticity in the stimuli that trigger it. In some cultures, or for some men, it might be triggered by low WHRs and in others large breasts. The relevant input signaling sexual difference could be shaped by fashion. Culture and biology could work together.

The final nail in the coffin for the WHR theory has been the recent demonstration that WHR preferences are far from culturally invariant. Yu and Shepard (1998) found that Matsigenka men in an isolated area of Peru prefer the "overweight" figures. Within weight classes they preferred figures with the highest rather than the lowest WHR. One man from Yomybato commented that the "overweight" figure with a WHR of 0.9 was healthy, whereas the others must have had diarrhea or fever or were "almost dead." Similarly, Westman and Marlowe (1998) found that the Hadza men of Tanzania did not prefer a WHR of 0.7. Rather, Hadza men did not even consider WHR when selecting potential mates: They were more interested in the weight of potential mates and expressed a preference for heavier females.

None of problems that have been discovered with the WHR theory mean that the initial idea was sloppy science. It was certainly better than much of the folk psychology with *post hoc* adaptive stories that passes for evolutionary psychology (e.g., Buss, 1994). What has been poor is the lack of critical evaluation of the evidence for this theory. Paper after paper and book after book have touted this research as an excellent example of EP in action (Buss, 1999; Miller, 2000; Miller & Todd, 1998; Pinker, 1997). If EP really is to meet the challenge of adaptive explanation, then its proponents should require more from their classic cases than a plausible story.

ADAPTIVE REASONING?

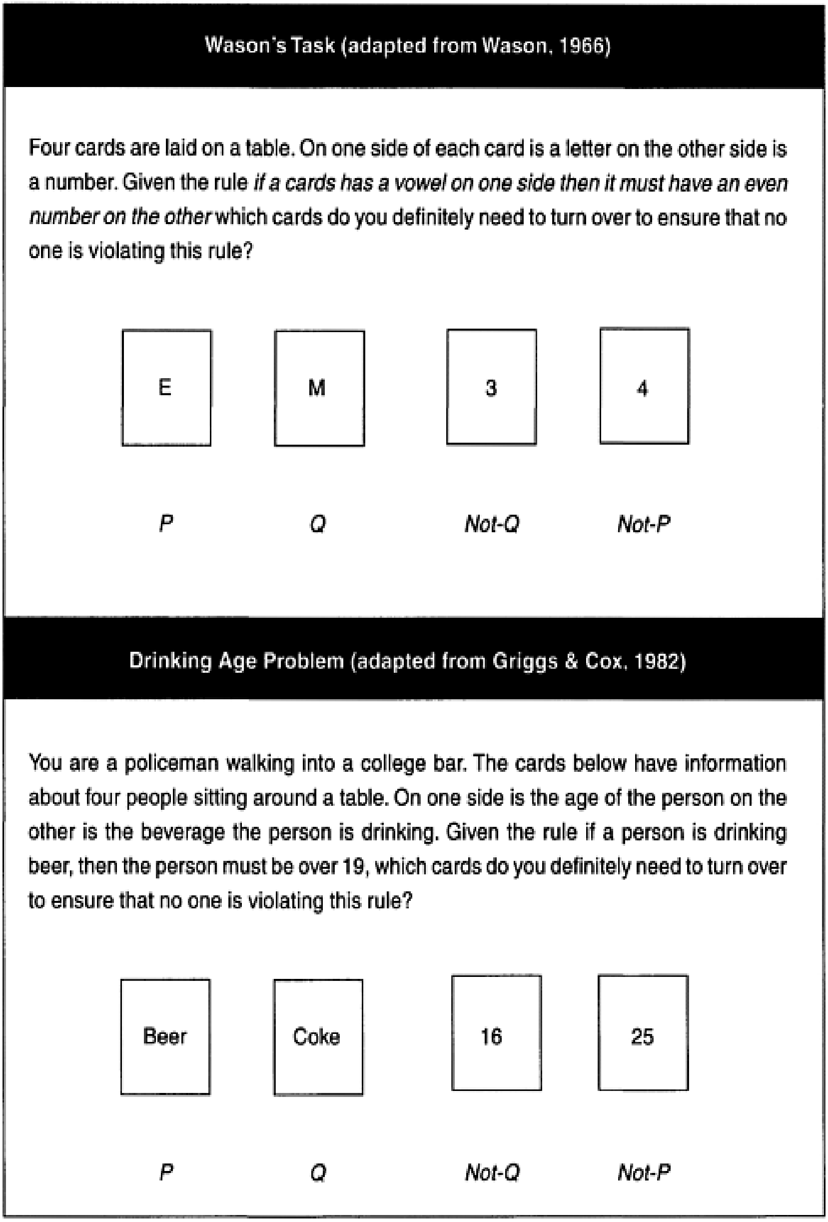
To understand how an organ such as the kidney works, it is beneficial to understand its purpose and what factors influenced its design. EP posits that the mind contains similar cognitive "organs" whose functions can be best understood by knowing their evolutionary "purpose" or selective history. Perhaps the most prodigious example of this logic in action is found in the social exchange algorithm credited to Leda Cosmides (1989).

Cosmides' theory of a social exchange algorithm grew from the work of cognitive psychologists on a test called the Wason Card Selection Task. When given the original Wason Task, a generalized *if P, then Q* rule (Figures 11.1 and 11.4a) people do poorly (Wason, 1966). This is of interest because perfect performance can be attained by the application of two simple rules. Turn over the E *(P)* to check there is an even number *(Q)* on the other side *(Modus Ponens)* and turn over the 3 *(not Q)* card to check there is not a vowel *(P)* on the other side *(Modus Tollens)*. People tend to select the E and the 4 ("affirming the consequent"). Either the human mind gets confused by this rather simple propositional calculus or the presumptuous human mind has decided that it's less important to disprove the law than to check its strength.

When faced with a specific situation, such as breakers of social laws, where it becomes more appropriate to select violations in the rule, rather than adherence to the law, people adopt an ethologically appropriate strategy and seek out these lawbreakers. This was shown when Griggs and Cox (1982) found a version of the Wason task where people did provide responses that coincided with the logically correct selection. This task was orientated around a drinking-age problem (Figures 11.1 and 11.4b). In this task, rather than making the typical mistake and ensuring that all people who have met the requirement (being over 19) are enjoying the benefit (drinking beer), people check that no one who did not meet the requirement (people under 19) are enjoying the benefit (beer).

This effect was later explained in evolutionary terms by Cosmides (1989). Game Theory modeling had shown that for cooperation between individuals to hold, spread, and persist within a population one must be able to detect people who are cheaters (Axelrod & Hamilton, 1981). Cosmides argued that due to this consistent pressure humans had evolved a cognitive mechanism for cheater detection. She made certain predictions about how such a mechanism should behave. It should only be evoked specifically by social exchange situations, where there is a requirement, benefit, and cheater (as these were identified as the important variables by the game theory modeling). This mechanism also should be able to detect violators even in novel social exchange situations where it fails to detect violations in nonsocial exchange situations.

This model is an example of the potential of evolutionary psychology. It focuses a large body of research under the banner of a unifying theory, makes clear predictions, and appears to provide novel insight into human cognition. It flies past the grandparent test. However, the cheater detection effect is not the



Wason's Task (adapted from Wason, 1966)

Four cards are laid on a table. On one side of each card is a letter on the other side is a number. Given the rule *if a cards has a vowel on one side then it must have an even number on the other* which cards do you definitely need to turn over to ensure that no one is violating this rule?

E	M	3	4
P	*Q*	*Not-Q*	*Not-P*

Drinking Age Problem (adapted from Griggs & Cox, 1982)

You are a policeman walking into a college bar. The cards below have information about four people sitting around a table. On one side is the age of the person on the other is the beverage the person is drinking. Given the rule if a person is drinking beer, then the person must be over 19, which cards do you definitely need to turn over to ensure that no one is violating this rule?

Beer	Coke	16	25
P	*Q*	*Not-Q*	*Not-P*

FIGURE 11.4. An adaptation of (a) Wason's (1966) original task, and (b) the Griggs and Cox (1982) "drinking problem" version of the task. The letters below the cards in italics indicate the role of each card in the *if P, then Q* rule and would not normally be present.

uncontested triumph for EP it is sometimes claimed to be. In the next two sections we argue that experiments on this effect are seriously confounded, and so open to alternative explanations. We conclude that the designation of cheater detection as an adaptation is premature.

A Mechanism Not Dependent on Requirement, Benefit, and Cheater

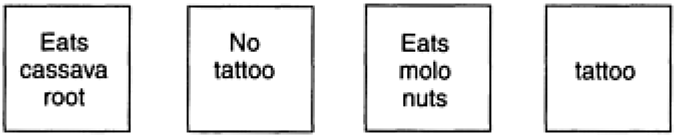
It is worth noting that, although the original Wason task was a measure of logical reasoning, by introducing the cheater context one has changed the nature of the task. People simply pick the cards based on the context scenarios, regardless of the logical instruction (Cosmides, 1989) and even in the absence of it (Fiddick, Cosmides, & Tooby, 2000). The card selection is determined by the context and not by any application of the *if, then* rules. So the question becomes are there any other context factors that could be causing people to pick out the correct cards? Maybe people, in some contexts, try to test the strength of a law (if a car is red, then it is fast; if a car is fast, then it is red), whereas in others they are prompted to test for exceptions to the rule (if a car is red, then it is fast; if a car is slow, then it should not be red). Both are valid strategies depending on the context in which they are placed.

Knowing that the rule is largely irrelevant, and that selection is made on the basis of the context, an alternative hypothesis to a social exchange algorithm explanation is that cheater detection scenarios have some general properties that cause exception-testing cognition (see Table 11.1 for some examples). It is the context of the scenario that determines which aspects of the law are relevant to test (Sperber, Cara, & Girotto, 1995). This explains the cheater-induced correct selection on the Wason task, but does not support the idea that there is an evolved device for the detection of cheaters. If these properties were general to many cognitive processes, then this would discredit the idea that an

TABLE 11.1. A summary of some context variables proposed by Libermann and Klar[a] and Sperber, Cara, and Girotto[b] that enhance P and not-Q selection in a manner not specific to cheater detection

	Nature of the effect	Possible reason for effect
Probabilistic versus deterministic"	"if P then *usually* Q" versus "if P then *definitely* Q"	There is not point in selecting not-Q because this will not disprove the rule.
Bidirectional	"if P, then Q" then "if Q, then P"	Relationships sometimes only make sense if reciprocal. Encourages the selection of the Q card.
Clarity of the Violating Instance" or cognitive	P-and-not-Q are the clear violating instances.	In many context there can be a range of P, Q relations

importance of P-and-not-Q pairb		that might be ecologically important.
Ease of representation of P-and-not-Q pairb	If the P-and-not-Q combination is a distinct definable category	Makes the P-and-not-Q option more likely to jump out
Rules are pragmatically felicitous'"	The rule actually makes sense.	



evolved algorithm was responsible for cheater detection. If the removal of these properties from a cheater scenario caused the cheater detection effect to disappear, then empirical support for Cosmides' theory evaporates.

In accordance with what they termed relevance theory, Sperber, Cara, and Girotto (1995) have presented a three-part "recipe" to ensure correct card selection in the Wason task. This recipe specifies that: the P-and-not-Q case is easier to mentally represent than the P-and-Q case (underage drinkers versus legal-age drinkers); the P-and-not-Q case should be of more importance than P-and-Q case (breakers of the law versus followers of the law) and that the rule should be clear and unidirectional (there is no implication that legal-age drinkers *should* be drinking beer). In an earlier paper, these authors have shown that cheater facilitation can be demonstrated in the absence of the cheater effect if these specifications have been met (Sperber et al, 1995).

Given that, at the very least, the results of cheater detection experiments are able to be interpreted in a manner that does not require cheater detection cognition, why is this theory so frequently touted as the jewel in EP's crown? Perhaps part of the answer is that the benefit of only altering one variable and holding everything else constant seems to have escaped the investigators of this field. Cosmides controlled for familiarity and content by creating complex, wordy scenarios of both cheater detection and descriptive natures. An example of such is the Cassava root problem used by Cosmides (1989) and again by Gigerenzer and Hug (1992). In the cheater situation, tattoos are given to men when they marry and they may then eat the aphrodisiac cassava. In the noncheater version men get tattoos when they marry and usually move to the south where the cassava root primarily grows. The same cards (shown below) were presented for both scenarios accompanied with the rule “if a man eats cassava root, then he must have a tattoo on his face.”

An examination of the cassava root problem looking at extraneous influences on selection strategy revealed several confounds. Liberman and Klar (1996) noted several differences between the cheating and normal scenarios. First, in the noncheating scenario, there is no specific violating instance: It could be a man with no tattoo eating a cassava root or a man with a tattoo eating molo nuts that is the exception to the rule. Second, whereas the rule for cheating is strict and bears no exception, the rule for noncheating employs such terms as "usually" and "primarily." (Both of these confounds serve to reduce the importance of the P-and-not-Q alternative.) Third, the noncheating rule is more easily interpreted as bidirectional. If a man lives in the north he will eat the cassava root and if he eats cassava root he will live in the north. In the cheating version it is clear that everyone wants to eat the aphrodisiac cassava and no one wants to eat the bad tasting molo nuts.

When these confounds were reversed, although preserving the basic cheater architecture (cheater, requirement, benefit), Liberman and Klar (1996) were able to completely dissociate the cheater detection effect across the range of scenarios used by Gigerenzer and Hug (1992), with noncheating scoring the 70% (typical of cheating conditions), and cheating scoring 30% (typical of noncheating conditions). When more vigorous efforts are made to keep everything but the essence of cheating between control and experiment, the effect completely disappears across the five studies used by Gigerenzer and Hug (1992), including the cassava root scenario used by Cosmides (1989). Clearly, this indicates that the enhanced performance is not the result of the cheating scenario but of the often-associated confounds.

Cheater scenarios have intrinsic to them the properties that seem to evoke exception-testing cognition (summarized in Table 11.1); a cheater is more *salient* than a law follower, and the breaker of a contract is of more importance than someone who has met the requirement for something but not yet cashed in on the benefit (which also has the benefit of making the rule clear and unidirectional). It is for these reasons, and not sloppy experimental control *per se,* that cheater scenarios have and will produce correct selection. But the finding that the "recipe" alone can produce correct selection and that the removal of the "recipe" in cheater scenarios results in incorrect selection, indicate that the observed phenomenon was not the result of a cheater detection module, or even cheater detection itself, but a correlation with other variables, best summarized by the Sperber et al "recipe."

A further problem lies in the perception of what the altered performance is actually measuring. Sperber and Girotto (this volume) argue that because the people ignore the question in social Wason tasks, such tasks are best conceptualiaed as two-feature categorization tasks, where subjects pick out people who have taken the benefit and those who have not met the requirement. They compared this to other two feature tasks (a glider would be a "flying vehicle" and "have no engine") and found that people have very little difficulty in responding accurately.

Evidence for a Modular Evolutionary Adaptation?

The saga of cheater detection contains another cautionary tale for evolutionary psychology Essential to the design of the mind is the ability to create new cognitive processes in response to the environment *within one's lifespan.* In situations where the same environmental pressures that allegedly shaped a module to suit life on the savannah are still active today (as they are with cheater detection), much more stress needs to be placed on proving that the resulting cognition cannot be the product of the flexible mind. Cosmides and her supporters present two arguments in support of the notion that cheater detection is a specifically evolved characteristic rather than a product of learning. First, they claim that evolved modules function independently of general processing, are automatic, generally opaque to awareness, and domain specific. Second, they argue that evolved modules are common to humankind and found across cultures (Cosmides & Tooby, 1997). The implication that learning (or broader experiential effects) cannot produce similar outcomes is spurious.

A classic example of the distinction between implicit and explicit cognitive function is to ask bicycle riders what they would do if their bike started tilting to one side. Many bikers respond that they would lean to the other side to right themselves. This is incorrect and would result in the person falling off the bike every few meters. Surprisingly, people seem to be quite capable of riding over long distances with no mishap. When placed on a bike, if it starts to tilt, people turn the handlebars, using their momentum to right their center of gravity, avoiding the fall. This function is independent of general processing, exists without awareness, and is specific to the bike-riding domain. So do we have an evolved bike-riding module? The answer is obviously no. Obvious because there is an apparent learning phase to bike riding where people do fall off every few meters (and, yes, because of the paucity of bicycles in the Pleistocene). In an area where the learning phase is less apparent, and the Pleistocene pressure more plausible, such "modules," created by the most fundamental of learning processes, can readily but incorrectly be heralded as evolved traits.

Cosmides and Tooby's second method of inferring adaptation is equally problematic. Just because a behavioral trait is found across cultures does not necessarily mean it is a product of adaptive evolution. Comparative psychologists have emphasized for decades that species-specific behaviors can arise through species-specific patterns of experience (Gottlieb, 1976; Gray, 2001; Lerhman, 1970). People the world over eat soup out of a bowl and not off of a plate because gravity acts the whole world over and people adjust their behavior in light of this. The whole world over there is a benefit to cheating (providing you don't get caught) and a benefit to being able to know when you're being cheated. The fact that cheater detection is cross-cultural does not automatically mean it is an evolutionary adaptation.

Over the last 10 years cheater detection theory has arguably been the best example of a Stone Age module in our modern mind. The idea was very seductive. However, as we have repeatedly emphasized much more than a plausible story is needed to meet the challenge of adaptive explanation. Perhaps the most disturbing indictment for EP as a discipline is that in the last 10 years there has been no convincing argument as to why this relatively simple ability has to be the product of evolution and not learning.

CONCLUSION

In its enthusiasm to repudiate behavioral creationism and social construction EP has embraced a cartoon version of Darwinism. However, we are not suggesting that psychologists should abandon Darwinism and the power of adaptive explanation. On the contrary, we believe that the future for evolutionary psychology lies in taking the challenge of adaptive explanation much more seriously. Dispensing with the current exclusive focus on unique and allegedly universal human adaptations is an essential prerequisite for this improved adaptationism. Hypotheses about unique features (autapomorphies in the jargon) cannot be subject to comparative tests. A broader evolutionary psychology would include comparative tests both across a range of species, and within our own species (Griffiths, 2001). Behavioral and cognitive evolution did not begin, nor abruptly end, in the Pleistocene. It would also be helpful if Evolutionary Psychologists abandoned their *a priori* commitment to other dogmas such as massive modularity and the monomorphic mind. Evolutionary biologists know that the extent of both phenotypic integration and heritable variation are empirical issues, and so should Evolutionary Psychologists. Finally, in the move from Evolutionary Psychology to evolutionary psychology, psychologists could use studies of behavioral and neural development to characterize appropriate traits for adaptive explanation in the same way the evolutionary biologists currently link developmental and evolutionary analyses. In short, in rising to the challenge of adaptive explanation, evolutionary psychologists need to act less like evangelists and more like current evolutionary biologists.

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Author Index

Subject Index