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WHAT'S INSIDE THIS ISSUE

FEATURE *Essays*

Biology and Rational Choice

John R. Alford, Rice University
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These are exciting times for those interested in the connection of biology to behavior. Consider the following findings that were reported in the first half of 2005 alone. Sexual foreplay for fruit flies is fairly predictable. A male first approaches a waiting female, bumps or taps her with his leg, rubs his wings in such a way as to make music, and then begins to lick her. The remarkable recent discovery is that when a single "male" gene is artificially inserted into the genome of a female fruit fly, the female performs the precise ritual described above even though in nature females never act in this fashion. For their part, when males receive the female version of the gene they become both more passive and more sexually interested in males (Stockinger et al., 2005). That a single gene can so clearly induce such a broad range of different behaviors, from leg-tapping to wing-singing to body-licking, and can also affect preferences, in this case sexual preferences, is stunning.

The fruit fly gene just described is a protein-coding gene. A large portion of the human genome is composed of non-protein-coding introns, nucleotide sequences that do not code for specific proteins and therefore have been given the undignified moniker of "junk DNA." But recent discoveries indicate these lengthy portions of DNA will need to be taken more seriously. Scientists have

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Science, Anti-Science, and Rational Choice

John Orbell, University of Oregon

In the recent past, rational choice has been under considerable fire, in particular from those whose approach to understanding political phenomena is non-formal and atheoretical, and whose approach to data involves interpretative narrative and "qualitative" observation. The fire has been so intense that there are even stories of beleaguered clutches of rational choice types sitting in small groups at conferences discussing with fear and trembling "What are we going to do?"—a far cry from the days of heady confidence that theirs' was the inevitable path of the future.

If the dispute within the discipline (if not within individual departments) is simmering down, I suspect it is due, in part at least, to the APSA's publishing *Perspectives*, thus providing more "room at the top" for those who were, in rational choice's heyday, feeling unfairly excluded from the *APSR*. If that's correct, it is a nice political solution to "who gets what, when and how" in the profession, and should keep the lid on things, at least for a while.

But it leaves unaddressed a more serious intellectual problem: *The almost total disconnect between the concepts and methods of Political Science and those of the life sciences, most notably Cognitive Science and Evolutionary Biology*. The past twenty-five years have seen major theoretical and empirical revolutions in these fields, revolutions that have transformed our understanding of what it means to be human and, certainly, what it means to be *political*. But what small recognition there has been of these developments in Political Science[1] has been, with only a very few recent exceptions, far from the

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A LETTER FROM THE *Editors*

Dear Readers:

In the "Letter from the Chair" in the Winter 2005 issue of *The Political Economist*, Lin Ostrom raised the lantern on research from the life sciences that challenges rational choice assumptions of human behavior. In this issue, we pursue this topic further with two lively and informative essays from experts in evolutionary biology and the cognitive sciences.

In the first essay, John Alford (Department of Political Science, Rice University) and John Hibbing (Department of Political Science, University of Nebraska) survey research on the biological basis of social and political behavior. Their review covers laboratory experiments, evolutionary/biological explanations, twin studies of the genetic heritability of behavioral tendencies, and neurological scans on specific areas of brain activation in political decision-making. In the second essay, John Orbell (Institute of Cognitive and Decision Science, University of Oregon) considers the theoretical and methodological implications of developments in the field of evolutionary psychology, emphasizing the distinction between "selfish genes" and "selfish phenotypes."

While each essay covers different territory, we find it compelling that both conclude that the rational choice enterprise is at risk of becoming irrelevant if its practitioners fail to incorporate the "model of the mind" now emerging from the life sciences. Furthermore, both Orbell and Alford and Hibbing argue that formal methods are not only fully compatible with the implications flowing from biology and the neurosciences, but may even be essential for the enterprise to succeed.

We think these papers give a clear picture of new research frontiers in the life sciences and define an agenda for political economy. We wish you, as always, "happy reading!"

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A LETTER FROM THE *Chair*

Dear Members of the Political Economy Section:

Congratulations to William Roberts Clark for his excellent work in organizing the 2005 Political Economy Section Panels and to all of the Panel Chairs who worked hard on organizing these panels. Many colleagues commented on the quality of our panels and were enthusiastic about the D.C. meeting.

At our noon meeting on Saturday, September 3, attended by around 50 members of our section, we heard good news from our treasurer, Robert Franzese, that our section is in reasonable financial shape. We had a good discussion about ways to improve attendance even further at the Political Economy Section Panels at the 2006 meetings. By investing some of our funds carefully, we should also be able to generate support for several modest travel grants for graduate students or from young faculty in universities or research centers that are not able to support travel to the APSA meetings. These travel grants could help domestic and international scholars who would not otherwise be able to participate in our section panels and other activities at future APSA meetings. The possibility of developing some kind of incentives for people to be at the panels themselves was also discussed, as well as whether we should continue to have a luncheon meeting in the future as we did for the first time this year.

Joelle Schmitz has agreed to chair a committee that explores these options. Please send Joelle [redacted] your suggestions about ways of organizing this new initiative—where to announce, criteria for selection, level of support, etc. She needs your input in the

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found key features of the social behavior of voles (a small rodent) to be governed by repetitive microsatellites found in the introns (Hammock and Young, 2005). Voles with longer repetitive nucleotide sequences in a region near a gene that creates vasopressin receptors will produce more receptors and therefore will make better use of vasopressin, a hormone common in males and known to be relevant in pair bonding and offspring-tending behaviors. Though hereditary, these microsatellites are much more susceptible to mutations than are protein-coding genes so evolution can occur much more rapidly and, therefore, inter- as well as intra-species variation in behaviors influenced by repetitive microsatellites (presumably most social behavior) will be greater. Some evidence exists that human autistics, not known for their nurturing tendencies, have shorter repeat sequences than do neuro-typicals. The message is that not only do genes influence behavior but genomic material outside of the genes does too.

Economist Ernst Fehr recently asked 178 undergraduates in Zurich to play a version of the trust game. Before the experiment began, however, each subject was administered a dose of mist from an inhaler. Unbeknownst to these individuals, half were receiving a placebo and the other half a dose of oxytocin, another hormone but this one associated more with females (it is released in large quantities during childbirth and lactation). Oxytocin fundamentally altered trusting behavior in these subjects, with those given the placebo trusting all their endowment to the other player just 21 percent of the time but those given the oxytocin trusting with all their endowment 45 percent of the time (Kosfeld et al., 2005). People vary in their level of trusting behavior not just because of their life experiences but also because of genes (and probably repetitive microsatellites) that shape their ability to produce and utilize oxytocin.

Finally, employing a large-scale twin data base, we recently reported evidence that political orientations such as liberalism and conservatism appear to have a significant genetic component (Alford, Funk, and Hibbing, 2005). Rather than being solely the product of parental socialization and other environmental circumstances, we found, consistent with much other research in behavioral genetics, that political and social attitudes are perhaps 40-50 per cent

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next several weeks. She and her committee will report to the Executive Committee of the Political Economy Section in November so that we can announce this new program early in January of 2006.

We also heard a report from Rick Wilson and Liz Gerber (the co-chairs of our panels for 2006) on future Political Economy panels at the 2006 APSA meeting in Philadelphia. Please see their report in this issue on the plans for the 2006 meeting. Also please see the announcement that David Leblang (Chair), Amy Poteete, and Bernhard Mueller have agreed to be members of the 2005 Best Paper Award Committee.

It was a treat to see everyone at our meeting and to hear the good discussions of future plans. I am already looking forward to the 2006 meetings.

Now to this issue of our newsletter. *The Political Economist* continues to be a stimulating venue for discussions about approaches and theories of Political Economy. Continuing this tradition, the two lead articles are on evolutionary theory and its relevance for the study of political science. One is by John Orbell, "Science, Anti-Science, and Rational Choice," and the other by John Hibbing and John Alford, "Biology and Rational Choice." Let me urge any of you, who may have seen the titles of their articles and skipped over them as being a little strange for our discipline, to dig in and read these two excellent presentations. Let me predict that evolutionary theory will become one of the core theories organizing much of our research in the future. We have made considerable strides in explaining behavior in static situations. Only a small proportion of action in the intersection of politics and economics is static in nature! As more effort is devoted to explaining change, an essential question will be how institutional arrangements, political beliefs, and behavior co-evolve. Further, we need to dig in and understand the biological foundations of human behavior (as well as the ecological systems that humans try to govern). One does not have to agree with all of the theoretical and empirical analyses presented in these two articles to recognize the potential impact that evolutionary processes have on human behavior. It is not necessary to assume that all political behavior is explained by evolutionary processes. On the other hand, to presume that the genetic structure of the human animal has no impact on decision making about cooperation with neighbors, views towards leaders, and more general political views is ignoring the important research summarized in these two articles.

Thank you: John, John, and John. By the way, can you now explain what it is about your first name that led you to explore the implications of evolutionary theory for our discipline?

Thanks also to all of the colleagues who have given me suggestions for how to improve my Madison Award lecture before it is published in *P.S.* in January of 2006. I do have about one more month before I must finalize it, so I would appreciate further comments from members of our section. The draft article is located on the Workshop webpage at: http://www.indiana.edu/~workshop/publications/docs/W05_23_Ostrom.pdf

Elinor Ostrom
Chair of the Political Economy Section

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mainstream Political Science, and its substance has been eclipsed by the vituperative disciplinary arguments mentioned above. Those arguments have been, in large part, between advocates of rational choice and of advanced statistical methods[2] on the one hand, and advocates of historical-interpretative approaches and of qualitative methods on the other, with the former carrying the banner of "science" and the latter carrying the banner of "anti-science." Yet the irony of those fights is that the "science" banner carriers have substantially failed to make connections with developments in the life sciences, while some of the discomfort that the "anti-science" banner carriers feel with respect to rational choice could be eased by their awareness of "models of mind" that are emerging from those same life sciences. In other words, if the rational choice, "science" folk have substantially failed to build bridges to the sciences that should concern them most, the anti-science folk, not liking what they see in rational choice's model of mind, are throwing out the "science" baby with the rational choice bathwater.

I will illustrate by reference to the field I know best, the evolutionary psychology of cooperation. The problem of cooperation is, of course, classically captured by the Prisoner's Dilemma, an incentive structure that offers players the choice between a dominant and a dominated incentive, with the former (defection) being associated with a collectively *sub-optimal* equilibrium and the latter (cooperation) with the collective optimum. Since a rational person will, by definition, take a privately optimal alternative, rational individuals *will* defect, making the "problem of collective action" from a rational choice perspective how to organize private incentives in a "compatible" manner—viz., so that rational individuals will, when advancing their private welfare, also advance the collective or social welfare. In fact, considerable improvements in our understanding of institutional design and the ways in which people resolve public goods problems have been made by scholars working within this mode (See, for example, Ostrom, 1990).

The behavioral assumption underlying this work seems just common sense; as Hardin (1977) classically observed, one should not expect people to act against their own self-interest—and, certainly, one should not design institutions based on the assumption that they *will*. And, if the self-interest assumption is to some extent wrong, then "errors" (people sometimes, in fact, acting against their own self-interest) will likely be far less damaging than "errors" made from the assumption of altruistic motivations (people sometimes, in fact, acting against the collective welfare). Selfish rationality seems not only plausible, but appropriately *conservative* with respect to efforts to design our world.

Yet evolutionary psychology, in conjunction with myriad laboratory studies of PD behavior, suggests that rational egoism is an insufficient model of human nature raising the possibility that an empirically more defendable model might help those of us interested in human institutions to make more subtle and accurate predictions to behavior.

Yet evolutionary psychology, in conjunction with myriad laboratory studies of PD behavior, suggests that rational egoism is an insufficient model of human nature,[3] raising the possibility that *an empirically more defendable model might help those of us interested in human institutions to make more subtle and accurate predictions to behavior*. To understand this rapidly developing and cross-disciplinary enterprise, it is critical to understand the distinction between "selfish genes" (Dawkins, 1976) and "selfish phenotypes." In brief: Genes that do not code for structures (cognitive, emotional, etc) that advance their own relative success through natural selection will be selected out (and conversely). But coding for a phenotypic propensity to—at least sometimes—cooperate with others *can* be a very effective way for genes to ensure just such relative success.

Elsewhere (J. Orbell, Morikawa, Hartwig, Hanley, & Allen, 2004), my colleagues and I have proposed a distinction between "rationality in action" and "rationality in design." By the former, we mean simply the standard model of rationality as currently employed in much political science theorizing: A rational actor chooses so as to advance personal self-interest. By the latter, we are evoking evolution as a "designer" (Dennett, 1995) that, across many generations of selection on genes, produces a phenotype with cognitive and emotional systems that are, in aggregate, well designed to promote behavior that maximizes the individual's probability of having offspring that are relatively successful in populating their world. In terms of the previous paragraph, therefore, "rationality in design" *could* produce a phenotype with a substantial propensity to act in a cooperative manner; our simulation findings propose one evolutionary path by which that could have happened.

Two fundamentals of evolutionary thinking in psychology must be made specific. First, the proposed model of mind is *modular*, not *general purpose*. Modularity implies multiple more or less special-purpose systems, each having evolved to address some particular adaptive problem that the animal confronted during its remote past—that is, to direct attention to adaptively important information in the environment, and to prompt (perhaps via appropriately designed emotional systems) behavior that, at least in that ancient environment,

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heritable with only a small portion of the remainder, maybe 10-20 per cent coming from parental socialization. Genes apparently influence not just instinctive behaviors but also complex social attitudes.

What do findings of this sort mean? They mean that, more than previously realized, a significant portion of behavior in all species, including homo sapiens, has biological roots. Social conservatives will have to come to terms with the fact that sexual preference is not merely a choice. Feminists will have to come to terms with the fact that differences in behavior across genders are not entirely the product of social construction. Those who want people to get along politically will have to come to terms with the fact that being conservative or being liberal is often genetically based and therefore unlikely to be jawboned or reformed away. And behavioralists, at the risk of committing egregious specification error, will have to incorporate biological and genetic variables alongside their beloved environmental variables.

Biology to Rational Choice's Rescue or on the Attack?

But what are the consequences of these new findings and of Darwinian biology generally for rational choice? In one sense, they could be helpful. Traditionally, a major complaint directed at rational choice is its failure to specify the nature and source of preferences. The practice of merely asserting preferences and proceeding from there understandably bothers those outside the rational choice community. Biological theory puts forward a source of preferences. It holds that life will respond to the environment in a fashion that increases the chances of its continuation. Further it offers an explanation for variations across species and within. As is evident in the research used to introduce this essay, genotypic variation produces incredible phenotypic variation. Thanks to the pioneering work of Maynard-Smith, some variants of game theory also assume variation. "Nature presents us with a hawk playing against a dove. What will each do?" Though "hawk" and "dove" or "cooperator" and "defector" are undoubtedly overly simplistic, nature does indeed present us with different human phenotypes. Biology offers an account of how and why "nature provides" variation. Increasingly, we will be able to predict behavior based on hormonal assays or DNA workups, particularly if we have information on the interaction of these biological features with the organism's environment. At these levels rational choice and biology are complementary.

Rational choice is often misrepresented as predicting that humans will engage in strict maximizing behavior but of course it can incorporate all manner of preferences—as long as they do not violate the assumption that individual humans behave instrumentally...Biology may identify deeper problems for rational choice.

At other levels, however, biology may create serious problems. For rational choice, "hawks" and "doves" are often distinguished only by differences in preferences. Biological discoveries indicate human differences extend beyond preference all the way to the processes by which decisions are made. Substantively, the decisions autistics make in dictator and ultimatum games are not that much different than neuro-typicals' decisions. But the decision making process autistics employ is not at all the same. When they are deciding, autistics display substantial cortical but minimal limbic activity. They are busy thinking through how neuro-typicals react in such situations and using rules to determine the way they think they are supposed to act. Neuro-typicals, on the other hand, use their limbic system and very little cortex. They do not need rules because feelings guide their decisions. Even with just the naked eye it is easy to spot people who are deciding emotionally compared to people who are deciding via a rule-based cognitive system and the level of trust they tend to inspire changes accordingly. All this not because of the outcome of the decision but because of the way it is made.

It is important to note that the source of these differences is not varying levels of information or different societal experiences. Rational choice sometimes appears very behavioristic in that it predicts choices will be automatic responses to environmental circumstances, often circumstances created by the choices of others. Know the individual's preferences and information level and choices can be predicted. Like behaviorism, rational choice has never displayed the slightest interest in looking inside the black box of the brain. Dennis Mueller is correct in observing that "homo economicus bears a close resemblance to Skinner's rat" (1986). Research in biology indicates vast differences in the way people deal with identical problems—differences in processing not in preferences. Brains vary dramatically in number of receptors for vasopressin, serotonin, oxytocin, etc., and also in the amount of these neurotransmitters and neuromodulators produced and the conditions under which they are released. Variations in the nucleotides of promoter regions of genes have even been used to predict with some accuracy how people will react to stressful situations (Caspi et al., 2003). But most rational choice practitioners seem to view genetics and brain physiology as irrelevant to the social science enterprise. If this is true, then rational choice grossly misunderstands the nature of human differences and will be left out of the most promising discoveries of the 21st century.

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was probabilistically adaptive. The theoretical argument for modularity is strong (Cosmides & Tooby, 1994; Sperber, 1994). The multiple adaptively relevant tasks that an animal must address will inevitably have diverse information requirements; information relevant to solving the "finding a mate" problem, for example, will be notably different from information relevant to solving the "escape from this predator" problem, and the same applies to related emotional systems (lust, perhaps, vs. fear). Since there are many more ways of being wrong in response to a particular problem than of being right, a cluster of specialized systems will be far more efficient—as a response to a defined set of problems—than a single, unspecialized system. Broadly modular architectures will, therefore, be at an adaptive advantage in competition with general purpose mechanisms, and thus should be expected in all animals, including humans.[4]

Second, at least in humans and other advanced primates, such specialized cognitive systems are likely to operate on a *contingent* basis interacting with the particular structure of information in the environment —the design of such systems, that is, will often incorporate a significant measure of *flexibility*. In the case of mechanisms associated with cooperation, therefore, such a system might be organized: "IF [environment = an individual who is close kin] THEN [emotions supporting cooperative behavior]; and conversely: IF [environment = an individual who is *not* close kin, or who belongs to a different group] THEN emotions *not* supporting cooperation, or perhaps supporting hostility]. Such flexibility of response could be a needless "evolutionary expense" in a stable, very predictable environment, but could be highly efficient in an environment that is, itself, in a constant state of flux (for an important development of this theme, see Marcus, 2004).[5] One obvious such environment is that provided by human sociality, involving, in Humphrey's (1976) classic term, never-ending and recursive games of "social chess."

In this context, therefore, we note that "rationality in action" is *domain general*. As Simon (1985) has emphasized, the standard model is simply an algorithm defining the normative criteria for value-maximization,[6] one that is powerless actually to *predict* behavior until information about the substance of those values is specified. By the same token, that algorithm is assumed to process with indifference information *of any kind, across any domain* that is passed through it. Although it is seldom spelled out in these terms, a rational actor will assess probabilities of various contingencies, will evaluate possible outcomes, and will compute expected values with equal facility *across all substantive domains*.[7] On the other hand—as Cosmides and Tooby have pointed out in their epistle to the economists (1994)—a model of choice that recognized greater speed and accuracy of information processing across some domains than across others would be significantly "better than rational" for theorists interested in understanding behavior across such a diversity of domains. Knowing, for example, that there are special purpose, domain specific mechanisms for reasoning about cheating in exchange relationships, as these authors have documented (1992), we might expect fairly rapid and accurate processing in the peculiar domain of interpersonal exchange; but we might expect much less rapid and accurate processing of information in domains where the EEA provided no occasion for an extended process of design by natural selection—as an example, perhaps, in the highly impersonal and peculiarly modern domain of the stock market.[8]

In this context, a generation of laboratory experiments on behavior in PD games has documented two broad findings: (1) People often *do* cooperate, even in one-shot, non-iterated games where all decisions are made in complete anonymity, thus where there is no basis from which tit-for-tat and other reciprocal relationships could emerge, and where the possibility of interpersonal pressures such as "altruistic punishment" and similar sanctions is eliminated (Caporael, Dawes, Orbell, & van de Kragt, 1989; Ledyard, 1995); (2) Rates of cooperative behavior in such studies vary substantially with experimental manipulations; in the series of n-person studies that I conducted with Dawes and van de Kragt, for example, we never observed fewer than 20% of subjects cooperate, and have seen as many as 90% and more cooperate—the latter in studies being, most interestingly, those that allowed a period of discussion about the problem at hand (J. Orbell, Dawes, & van de Kragt, 1988; J. M. Orbell, van de Kragt, & Dawes, 1991).

These findings document quite unambiguously that people *do* have a propensity to cooperative action, a propensity capable of being both more and less expressed in behavior under circumstances specifiable in the laboratory. To the present point, they are quite consistent with the extensive evolutionary literature that does identify several processes that could select through evolutionary time for such cooperative propensities. True, the models in question do find "selfish gene" reasons for cooperative propensities; kin altruism, the oldest and best known model (Hamilton, 1964), is based on the idea that altruistic behavior toward relatively close kin can advance the success of one's own genes, while reciprocity (Trivers, 1971) and related models (Axelrod, 1984) develop the idea that fitness costs paid at one time can be more than compensated for later. But—as discussed above—"selfish genes" are a necessary component of any argument that cooperative propensities can be expected at the phenotypic level, and the important question thus becomes: *Under what environmental circumstances—in particular, under what interpersonal*

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Level(s) of Analysis

Biology may identify deeper problems for rational choice. Rational choice is often misrepresented as predicting that humans will engage in strict maximizing behavior but of course it can incorporate all manner of preferences—as long as they do not violate the assumption that individual humans behave instrumentally. As Fiorina puts it, "the only thing all RC [rational choice] people agree upon is that their explanations presume that individuals behave purposively" (1996: 87). Ferejohn and Satz add that "social-science explanations must be compatible with intentional descriptions of human agents" (1996: 74). Human beings can be wonderfully concerned for the welfare of others but for rational choice the level of analysis must be the individual organism and the concern for others must ultimately funnel back through the individual's own monolithic utility structure and decision calculus. Rational choice can incorporate much but it is unable to deal with an individual having more than one preference structure simultaneously or with an individual cell within an organism (or with a group of organisms) possessing behaviorally-relevant preferences of its own.

Theorizing in evolutionary biology and empirical findings from neuroscience provide every indication that rational choice's conception of monolithic, purposive human agency is mistaken. Biology's alternative view focuses not on an arbitrary single level of analysis (the human organism) but recognizes the relevance of multiple levels of analysis (from sub-cellular all the way to groups of organisms). Perhaps the most oft-cited illustration of what could be viewed as sub-cellular agency involves the human sperm cell. Unlike most other cells, sperm have virtually no organelles since they have voluntarily stripped themselves down to nucleic DNA and a tail. They are, for example, completely devoid of mitochondria even though mitochondria contain DNA (this is why mitochondrial DNA is always traced through the female line). If both the sperm and the egg contributed organelles to the united cell, intracellular feuding would occur with disastrous consequences for the zygote. For this outcome to be avoided, one cell had to make the supreme sacrifice by accepting second-class citizenship while the other became the host cell, contributing everything except 50 per cent of the nucleic DNA. If analysis is limited to the sub-cellular level, it seems as though the nucleic DNA of the sperm was incredibly selfish in selling out the rest of the sperm cell for its own benefit. But if multi-level analysis is practiced, we see that this apparently selfish behavior at the sub-cellular level appears miraculously cooperative and self-sacrificing at the cellular level (with the sperm cell clearly taking one for the team). Concentration at a single level of analysis obscures cooperation that is visible when multiple levels are recognized (see Sober and Wilson, 1998; for further examples at the cellular and subcellular levels, see Ridley, 2001 on cooperation, and the ongoing work of Robert Trivers on competition). If this is true, empirical work should consistently reveal much more cooperation than single level theorizing predicts—and numerous reviews of rational choice conclude this is exactly what happens (see Thaler, 1992; Green and Shapiro, 1994; Ostrom, 1998).

Decentralized Brains

The notion that a human being is unitary enough to possess a single preference structure is belied by all kinds of recent neurological research. The core finding of brain scans as well as of work with people suffering from brain lesions is the decentralization or extreme modularity of the brain (Damasio, 1996). Neuroscience has not been able to locate a decision-making center where preferences are drawn together and evaluated prior to a single course of action being selected. Many parts of the brain seem to be involved but there is no homunculus that makes executive decisions after a cost-benefit analysis (Wegner, 2002). To be sure, portions of the pre-frontal cortex (PFC) are recruited by the anterior-cingulate cortex when sensory inputs suggest the default course of action may be inappropriate but it seems the PFC is only able to bias actions elsewhere in the brain. In other words, it can send encouragement (or discouragement) to other parts of the brain but it does not bring the decision-making process into itself prior to making a choice (see, for example, Miller and Cohen, 2001).

An example of decentralization that may be of interest to readers of *The Political Economist* involves hyperbolic discounting. Economists have long known that the value people place on receiving a desired commodity declines rapidly as the date of delivery is moved into the future but that a point is reached where further delays begin to matter less and less. For example, people would much rather receive a sum of money today than in a week but if the choice is between receiving the money in a year or in a year plus a week the difference in ascribed value is almost nil. Economists trained in classical microeconomic theory draw hyperboles to represent the relationship between time and value but have no theory as to why the relationship takes on a hyperbolic as opposed to a linear or non-hyperbolic curvilinear shape. Using MRI technology, McClure, Laibson, Loewenstein, and Cohen were able to observe brains making choices in two different situations. In the first, one of the options involved a smaller reward today or a larger reward a little later; the second also involved the smaller-earlier or larger-later option but everything was moved into the future such that even the "earlier" option would not occur for a couple of weeks. Though the gap in time between early and late was the same, when one option for receiving the reward was "today," limbic activity was intense, especially in the midbrain dopamine system and the paralimbic cortex. When

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and perhaps cultural environments—can we expect such propensities to be realized in actual behavior? Part of the answer will certainly involve incentive changes (such as provided by "altruistic punishment" arguments, incentive compatible institutional designs, enforced laws and customs, etc.), but an important part also seems likely to involve cognitive structures designed to respond in the form: IF [environment] THEN [cooperate]—even when the "environment" term does nothing to change the incentives with which individuals are confronted. A useful starting point from which to begin identifying diverse [environment] terms might be the many experimental studies that, as mentioned above, have documented highly varying cooperation rates under diverse laboratory manipulations.

In summary, I have argued that the ongoing fight within Political Science between the "pro science" advocates of rational choice modeling and quantitative methods on the one hand, and the "anti-science" advocates of interpretative and qualitative methods on the other is the wrong fight at the wrong time—at least if we grant, charitably, that it is *not* simply a political fight about the allocation of scarce values within the profession. Recent theoretical and empirical developments in Cognitive Science and, in particular, Evolutionary Psychology have pointed toward a "model of mind" that is notably different from the model that is implied in the classic model of "rationality in action" and that is available for political scientists interested in better understanding their peculiar subject matter. True, it is not as simple as "people act so as to maximize their private welfare," and thus does not provide such a simple and elegant base as rationality in action from which to model social and political processes. But it *is* well-based on continuing empirical study of how our minds evolved and (therefore) how they presently work, a scientific virtue that, I believe, should normally trump whatever elegance can be gained from deducing models of social processes from simple but empirically flawed models of mind. There is very little to be said for a situation in which the "model of mind" underlying the *science* wing of Political Science is at such substantial variance with the "model of mind" now emerging so clearly from Evolutionary Psychology and the life sciences more generally.

I intend no suggestion that political scientists should abandon the deductive and formal methods that have been introduced, so profitably, to Political Science during the past decades; those methods are, of course, quite compatible with the methods of evolutionary biology and related disciplines. I do intend to point out that the yawning gap between the life sciences and the social sciences, including Political Science, is unnecessary and intellectually wasteful, with Evolutionary Psychology, in particular, providing a bridge between the "two cultures" that has the potential, not only to resolve anomalies that exist within the rational choice paradigm, but also to provoke new and important hypotheses about how the political world works.

Neither do I expect that those who are in the discipline's "anti-science" camp will, on learning about Evolutionary Psychology and the apparently more "human" model of mind that it promotes, see the error of their ways—in a blinding flash, perhaps—and immediately begin inventing arguments about adaptive problems in the EEA, cognitive architecture, the fit between modern environments and our EEA-designed brains, and the like.[9] I do hope, however, that their acquiring minimal literacy in this new interdisciplinary paradigm will allow them to recognize that the choice is not between science and non-science, but rather between science and better science.

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"today" was not one of the options, the lateral prefrontal cortex and posterior parietal cortex showed heightened activity (2004: 505-6). In other words, the possibility of immediate gratification seems to activate the emotional part of the brain but when immediate gratification is not an option, the more reflective and cognitive part of the brain is activated.

The relationship between discounting and time is not accurately represented by a single, continuous hyperbolic curve as if the pattern were traceable to a single part of the brain. Rather, different parts of the brain are involved depending upon the time frame, meaning two separate lines should be drawn: One steeply declining line to represent the rapid drop-off in value attached depending upon whether immediate reward is an option and a second, almost horizontal line that describes all tradeoffs except those involving immediate reward. The involvement of different parts of the brain and therefore different modes of decision-making depending upon the time frame may not be neat but it is functional. Locking up immediate rewards was undoubtedly advantageous to our ancestors, living as they did in a time marked by the rapid perishability of anything valued. Evaluating differences in degree of deferred gratification was not something at which they would have had much practice or emotional investment. From this perspective, behavior is not the product of rationality mediated by mental limitations (Simon, 1957) but rather is "better than rational" (Cosmides and Tooby, 1994).

Conclusion

Biology is perfectly amenable to formal theory. Indeed, much of the best work taking place at the intersection of biology and behavior is highly formalized (see, for example, Maynard Smith, 1982; Bowles, 2004; Boyd and Richerson, 2005). Careful specification of the implications flowing from biological theory, often in the form of mathematical derivations, is essential for the enterprise to succeed. Concerns arise only when this formalization is applied to a theory that erroneously assumes behavior is purposive or intentional, that it transpires exclusively at the organism level, and that the gold standard is something called rationality. From a biological point of view, evolutionary pressures present since the beginnings of unicellular life are a far more meaningful baseline than arbitrary conceptions of human rationality present only since the Enlightenment.

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GENERAL ANNOUNCEMENTS

CALL FOR PAPERS FOR THE 2006 APSA MEETINGS: POLITICAL ECONOMY SECTION

Political economy represents a diverse subfield that includes research defined by topic - substantive studies of politics and markets - and by methodology - theoretical or empirical studies of politics that utilize tools derived from economics. We invite papers from both of these branches of political economy.

On the substantive front, we welcome paper or panel proposals that study economic policy; trade; budgeting; regulation; development; etc. These papers should be theoretically and empirically rigorous and may be situated in a domestic, comparative, or international context.

On the methodological front, priority will be given to paper or panel proposals that test, refine, critique, or support core political economy theories, especially those related to collective action; institutions and development; governmental performance; legislative behavior; voter behavior; and federal systems. Critics of economic approaches to the study of politics often argue that our abstract models are not supported by empirical evidence or that our empirical approaches (especially large-n quantitative analyses or lab experiments) lack validity. We see the 2006 APSA meetings as a ripe opportunity to pull together papers that provide strong field evidence for our theories, that validate experimental results with field evidence and other data, or that demonstrate promising efforts to develop better theoretical foundations to explain anomalous empirical findings.

Elizabeth Gerber (ergerber@umich.edu)
Rick Wilson (rkw@rice.edu)
Co-Chairs, Political Economy Section of the 2006 APSA Meeting

CALL FOR NOMINATIONS FOR THE BEST PAPER AWARD

To All Chairs of Political Economy Panels at the 2005 Meeting:

Please consult with the discussants on your Political Economy panels from the D.C. meeting and nominate one (or, at most, two) papers that you would like to see considered for the Best Paper in Political Economy Prize for 2005.

The award committee is composed of David Leblang (Chair) from the University of Colorado (leblang@colorado.edu), Amy Poteete of the University of New Orleans (amypoteete@gmail.com), and Bernhard Mueller of the Catholic University of America (86mueller@cua.edu). Please send your nominations to them via email ASAP!

EUROPEAN CONSORTIUM ON POLITICAL RESEARCH

The European Consortium on Political Research has recently approved a new standing group on Regulatory Governance. The group was initiated by a group of scholars of regulation, aiming to enhance cooperation among scholars and the state of the knowledge in this highly important field. Our group is organized under the ECPR, which is the organization of European political scientists, but we do hope to include and be able to attract lawyers, economists, sociologists and criminologists (among other) to join us in this scholarly organization. While we are located in Europe we hope to attract as many scholars as possible from other parts of the worlds as equal partners for academic exchange and full members of our group.

For more information see: http://galactus.upf.edu/regulation/reg-gov or contact David Levi-Faur (levifaur@poli.haifa.ac.il) and/or Jacint Jordana (jacint.Jordana@upf.edu)

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[1] I acknowledge with respect forty years of "Biopolitics," work by political scientists who recognized long ago the relevance of Biology (thus of evolution) for their discipline. They were right. I am, however, particularly concerned with the revolutions in Cognitive Science and Evolutionary Psychology that effectively began in the 'mid eighties.

[2] It is important to notice that, while there is considerable overlap, these are substantially different groups; many of those who use advanced statistical methods eschew rational choice and its entire doings. For many of the critics, however, they are one.

[3] The other part of the rational choice algorithm—that people act in a value maximizing manner to advance whatever values they do hold—has also been under serious fire from the "heuristics and biases" movement, led by Daniel Kahneman and the late Amos Tversky (see, e.g., Kahneman, Slovic, & Tversky, 1985); that movement, in turn, has been under fire from the "fast and frugal heuristics" led by Gerd Gigerenzer and his colleagues (e.g., Gigerenzer, 2000a). I will not address those complexities in this brief note, however.

[4] There remain, of course, many problems with respect to the specifics of modular design that are not understood—and are not likely to be understood in the near future. A case in point: Since natural selection necessarily builds "new" structures from a basis of previously-existing "old" ones, efficient design of the former will, wherever possible, incorporate mechanisms already in place, meaning that a complete analysis of any functionally specific cognitive mechanism will often (normally?) be impossible in isolation from analysis of other, perhaps structurally similar but functionally quite distinct, modules. On this and related points, see importantly Steven Mithen's *Prehistory of the Mind* (1996). How a complexity of domain specific modules might, in aggregate, facilitate something approximating domain general computation is also a continuing problem.

[5] A distinct but in many ways related body of ideas emphasizing the importance for decision making of how information is structured in the environment has been developed by Gigerenzer and his colleagues in their analysis of humans' toolkit of "fast and frugal heuristics" (Gigerenzer, 2000b; Gigerenzer, Todd, & Group, 1999).

[6] One chooses so as to maximize values weighted by the probability of their occurring.

[7] This allows, of course, for the possibility that the costs of information about such probabilities might vary across diverse domains.

[8] Notice that the tradition of behavioral decision theory mentioned above (footnote 3) proposes heuristics—understood as information processing shortcuts that, generally, lead us to deviations ("biases") from the norms of rational choice—that are similarly domain general. In particular, the finding that people tend to be risk tolerant when risky and certain alternatives are framed as involving losses but tend to be risk averse when they are framed as involving gains, is (implicitly) assumed to apply whatever the *substantive* domain confronting the decision maker. By the same token, Herbert Simon's (1955) even better known idea of "satisficing" is also (implicitly) proposed as domain general.

[9] "Culture" is often held as an alternative and superior explanatory mechanism to rationality, with the advocates of "culture" decrying the individualism that they see implied by rationality. Similar arguments are made with respect to "cultural" vs. "genetic" explanations of behavior. Nevertheless, perhaps the most dynamic frontier in the study of human evolution during the past few years addresses precisely the *interaction* between genes and culture, emphasizing that the question "is it genes or is it culture?" is totally misplaced (Marcus, 2004; Ridley, 2003) with the right question being: How have selection on genes and selection on cultural manifestations depended on each other during humans' evolutionary history? The work of Richerson and Boyd is particularly important in this respect. Among their several recent works, in particular (Richerson & Boyd, 2005).