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Economics, Biology, and Culture: Hodgson on History*

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This book addresses what the author claims, with considerable justification, to be the foremost challenge confronting the social and behavioral sciences today: the problem of historical specificity. Hodgson poses the question by asking whether we need different theories to understand social and economic behavior in different societies at different stages of their development. He answers the question in the affirmative, and criticizes the economics profession for suggesting that there is one universal model or theory equally suited to all economies and societies at all times. He faults the profession further for no longer worrying much or conducting serious debate about this issue, a development he attributes to the eclipse and eventual demise of institutionalism and historical economics in England, Germany, and the United States.

The book is most provocative as a contribution to intellectual history. Hodgson acknowledges that "the key methodological problem, of the relationship between the individual and society, dogged the historical school for its entire existence and was never satisfactorily resolved" (p. 64). On the other hand, he rejects the notion that institutionalism and historical economics died in the face of superior, more compelling evidence or arguments. He attributes the loss of influence to a set of particular political and historical events that might have been different. In a nutshell, these schools of thought died because of idiosyncratic developments at Harvard University and the London School of Economics, combined with the intellectual evisceration of German universities with the rise of fascism. The final nail in the coffin was the triumph of Keynesianism at Harvard and Cambridge: Keynes' work was almost completely devoid of any concern for the problem (while true, the sin seems far less egregious than in the case of Robbins[1]). Post-Keynesians, in continuing to aspire to a "General Theory", have done no better.

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* A review essay on How Economics Forgot History: The Problem of Historical Specificity in Social Science, by Geoffrey Hodgson. London: Routledge, 2001. The essay appears in Research in Economic Thought and Methodology vol. 22A, eds. Warren J. Samuels and Jeff E. Biddle. Amsterdam: Elsevier, 2004, pp. 373-98. Comments by David Colander, Herbert Gintis, Michael Kevane, John McMillan, Bill Sundstrom, and Gavin Wright are gratefully acknowledged.

[1] Lionel Robbins was remarkably cavalier in his discussions of the relevance of evidence in developing or testing economic theory. See Robbins (1932 [1984], pp. 78-79).

This is an ambitious work. In the process of looking backward, it raises a number of important issues relevant to how we should conduct social and economic inquiry in the future. The last part of Hodgson's book, however, which tries to lay out an agenda for such research, is, by the author's acknowledgement, the least satisfactory (p. 273). In spite of all his critical efforts, he seems unable to delineate a compelling vision of how research in institutional economics, or in the behavioral sciences more generally, should now be conducted.

Thus the central puzzle posed by this book. Why does his scholarship, and other scholarship in this tradition, not break open new vistas? Why is it unlikely to be successful in overcoming the malaise of many social scientists, both within and outside of economics, who recognize the potentially independent influence of culture, social structure, or institutional rules on behavior, but seem unable to enunciate a research program that goes beyond on the one hand description of such influences, and on the other hand periodic attacks on the "neoclassical" or rational choice orthodoxy?

My intent in the remainder of this essay is to explore why Hodgson, and so many others of both heterodox and orthodox persuasions, continue to confront such difficulty in moving forward. Perhaps there is only so far this style of intellectual history can take us in advancing social science. Perhaps also it has become too easy to settle into well established (and largely justified) critiques of rational choice theory as it is actually practiced. Getting into the rhythm of these critiques can feel like putting on a set of well worn clothes. It's an easy thing to do because they're probably right where we put them down last night.

But if we continue to wear the same outfits day after day, there begins to be a downside. When we no longer innovate, our sartorial displays cease inspiring enthusiasm in those who observe them and ultimately in ourselves as well. I am not recommending shopping for its own sake – our consumerist society contains enough exhortations to this end already. But, if I may now extract myself from this metaphor, we do need new approaches, because as social scientists, *we are making relatively little scientific progress in developing robust predictive models of human behavior*. Hodgson obviously very much wants to provide that intellectual leadership. But although this is original scholarship, one puts it down with the sense that it could have been written in the 1960s or 1970s, perhaps even earlier.

Well, what of it? In some areas of scholarship, such as political theory or philosophy, it still makes sense to start with texts written hundreds or even thousands of years ago. At the same time, it is quite evident that in areas of the natural sciences such as chemistry and biology, we have made remarkable explanatory and predictive breakthroughs in recent centuries and decades. To say of a recently published book in these fields that it has a certain nineteenth century feel to it would not be to praise it.

Some will rationalize the current state of affairs by asking whether the study of human behavior can or should be a progressive scientific endeavor. Ariel Rubinstein, for example, has argued that economic theory simply should not be "a tool for predicting or

describing real human behavior". As to what makes a "good" economic model, intuition should suffice as an evaluative criterion: if it's good enough for philosophy, it ought to be good enough for economic theory (2001, p. 616). I find this position unacceptable and believe Hodgson does as well.

Most of us work in divisions of social or behavioral sciences, not divisions of social philosophy. We are supposed to be doing *science*. Rubinstein has the courage to acknowledge weaknesses in the record of game theory, for example, in actually illuminating human interaction. The problem, of course, is more general. The social and behavioral sciences, by which I mean principally economics, sociology, anthropology, political science, and psychology, do not, either individually or collectively, have a good recent track record on this account. Balkanized, and recapitulating some of the same debates again and again, in many respects social science discourse is enervated and, to those who read it, enervating. This is as true of work in institutional economics or traditional sociology or anthropology as it is in those who favor rational choice approaches.[2]

Rubinstein's position, which is more broadly shared than many will publicly acknowledge, at least as they apply for grants from the National *Science* Foundation, is that the failure to make more progress developing a predictive or explanatory agenda should not be held against us, and in fact he suggests that it might have been detrimental had we been more successful: business students and the military would just have misused the knowledge.[3] Message to world: leave us be to develop our aesthetically pleasing models. This position is remarkably similar to those of cultural anthropologists or sociologists who characterize their work as interpretive rather than explanatory, and finesse questions about the extent to which their propositions are falsifiable. If none of the propositions we are advancing in the different disciplines can be disproved, it is a waste of time talking about unifying the social sciences, because none of us is doing science.

Not everyone is satisfied with the proposition that we have no responsibility to develop models or tools that can predict or explain human behavior, and many genuinely want to try and improve upon the current situation. It is one thing, however, to call for the unification of the social sciences, quite another (and far more difficult) to take real steps in that direction. The majority of scholars, particularly before receiving tenure, remain within disciplinary bailiwicks, continuing along well established paths. But a minority, frustrated with the inability of prevailing theoretical frameworks to explain anomalous data, or in some cases the imperviousness of such frameworks to any imaginable data, venture into new territory.

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[2] The focus here is on game theory and microeconomics. Macroeconomics, despite the disdain often visited upon it by micro theorists, and despite the inroads of abstract highly technical modeling, has, in my view, been more progressive in recent decades as a scientific endeavor, principally because explanation, prediction, and data have mattered more.

[3] Here is exactly what he says: "…I am not sure applicability is desirable. If microeconomics is useful, the first to benefit will be the MBA students who are among the last people in the world I feel obliged to assist. If game theory were indeed useful it could be used for military purposes" (2001, p. 617).

Often, however, the result is something akin to religious conversion: explorers embrace the intellectual framework of another discipline uncritically, with recent converts becoming even more fervent advocates than long time residents. Because of the history of balkanization, there is no disciplinary tradition that does not currently suffer from some form of intellectual pathology, and it's a difficult matter, from an internal or an external perspective, to separate diseased from healthy tissue. But it's naïve to think such segregation is any less important in areas outside of one's own. A good rule of thumb for adventurers is this: conditions in other regions are likely to be as complex and disordered as those domestically. This should not discourage exploration. But embracing without critical examination work in other disciplines can be as unproductive as simply ignoring it. This is true for those moving in either direction between the two main social scientific traditions: the economic/rational choice and the traditional sociological/anthropological approach.

The reality is that data and concepts useful for building a general framework for understanding human behavior are scattered throughout the social, behavioral, and natural sciences. But there is also a lot of chaff, often protected by the stiff tariffs that discourage integrative or cross-disciplinary work. Separating or distilling what is useful from what is not is challenging. As we push forward we need scholars who understand the limitations and strengths of their own tradition, and are also prepared to search out and identify the limitations and strengths in others.

Hodgson's book is principally concerned with variation over time in a region's institutional structures and the consequences of such variation for behavior within them, and with intellectual attempts to deal with this issue. If one wants to move beyond consideration of consequences to a more general theory addressing both commonalities of human institutions and their variation, one is drawn, almost inevitably, to consideration of the influences on behavior of our biology and evolutionary history. This helps explain why, to the degree that this book moves beyond intellectual history, it is as much about biology as it is about history. Hodgson's appeal to reintroduce a serious interest in biological influences on human behavior is welcome (and consistent with some of his earlier work), but weak on specifics or serious critical treatment of the cross-currents in evolutionary thinking that would be necessary to implement this.

Strengths and Limitations of Institutionalism

We must begin, of course, by understanding the strengths and limitations of our own approach. If we are to make progress, it is necessary to examine carefully the institutionalist position, to view it not just as a battering ram with which to inflict damage on currently prevailing orthodoxies, but to identify the strengths and weaknesses in its current incarnations. In doing so, we must be critical as well as constructive.

Hodgson asks rhetorically if we need different models or theories to understand different societies in different historical epochs. His is, as suggested, a rhetorical question, preparing the ground in this case for an affirmative answer, one with which many readers of his book will be inclined to agree. But there is a strong case that this initial and fundamental question should be answered in the negative, and indeed has to be answered

in the negative, if we are serious about integrating the social and behavioral sciences in a way that will foster a progressive scientific enterprise. The case for this position does not, however, involve embracing rational choice models based on the assumption that human preferences are stable, transitive, *and* narrowly selfish.[4] Such models predict that humans will operate in all spheres so as efficiently to advance their material self interest. This approach is inadequate as a foundation for addressing the political and cultural variables that define the arena within which we economize, or operate according to the counsel of our foraging algorithms (Field, 2001).

Our biological hardware is basically unchanged from what it was 12,000 years ago, at the start of the Neolithic transition. Any model that purports to explain or predict how people behave must come to terms with the fact that across cultures today, or across time in history, groups of human actors are virtually identical in their species typical characteristics. My point is not to deny variation in individual biological and cognitive characteristics, but to emphasize that variation among group averages is relatively insignificant, and certainly too small to account for the types of differences in behavior with which Hodgson is concerned.

We are, of course, more than our biology. Then, as now, we possess evolved capabilities to learn by direct observation reinforced by instruction and imitation of others. We are not blank slates: this learning has biases, in the sense that we learn more easily in certain directions than in others. These differential predispositions, the consequence of evolutionary influences on our genetic heritage, have been most clearly demonstrated in language acquisition, but it is true as well of how we learn to interact with other humans. These biases nevertheless allow a great deal of flexibility. Ultimately it is what we and our forebears have done with this capability that distinguishes us from our neolithic ancestors, and distinguishes denizens of technologically advanced societies from hunter-gatherer groups.

Learning can affect not only the means we use but also to some degree the ends we deem important. Parents are remarkably successful in passing on to their children not only language vocabularies and recipes for making cake (transforming inputs into outputs), but also religious beliefs and ethical principles. Infantrymen are routinely trained to throw themselves on a grenade to protect other members of their squad. And organized political forces in the Middle East have been able to motivate substantial numbers of individuals to act as suicide bombers. The latter two instances are particularly stark examples of individuals expressing behaviors which, from a narrowly selfish perspective, are strictly dominated strategies. Contrary to those who insist that preferences are strictly exogenous and not to be disputed, human goals can to some degree be influenced. This is particularly relevant in considering political behavior, which extends beyond realms such as electoral politics to economic interactions which often also have a political component (the main exceptions are those mediated by purely competitive markets).

[4] In a typology of rational choice approaches developed below, I distinguish this type of models from approaches that involve less restrictive assumptions about human goals.

The partial programmability of human cognitive processes and behavioral inclinations has made possible variations in human culture, which in turn help account for behavioral differences beyond those that can be swept back to differences in the non-human environment. A large part of anthropology has been devoted to documenting cultural influences on human behavior distinguishable from those of the non-human environment. Traditional anthropologists, sociologists and institutional economists have correctly rejected as a methodologically individualist fallacy the claim that we can understand everything about a society by considering the characteristics of just one individual human. Thus, Hodgson is absolutely right that "institutions are not simply human nature writ large" (p. 269).

Nevertheless, culture and social structure are not superorganic forces with powers or influence or dynamics ultimately irreducible to aggregated characteristics of individuals, in particular beliefs about how the world works, who is friend and who is foe, who deserves deference and who does not, and what are the desirable ends of human activity. Methodological individualism has been a traditional bête noire of institutionalist economics. While the institutionalist critique of economic and rational choice theory as it is commonly practiced has much to recommend it, we will make no scientific progress if we go the superorganic route and mystify culture, social structure, or institutions. Institutionalists need to make peace with the principle of methodological individualism, cease the unending critiques of reductionism, at the same time rejecting a version of methodological individualism that is not sustainable while working to develop and advance one that is. Granted, we cannot hope to understand human behavior by studying the characteristics of a single individual. But we can hope to do so by studying the aggregated characteristics of groups of individuals.

Institutions are not human nature writ large. But species typical behavioral predispositions – human universals – do govern and restrain the range of social variation. All known human groupings proscribe incest, murder within the group, and excessive within-group lying and cheating. It is of course possible that these outcomes reflect independently arrived at cultural solutions to universal challenges of coping with other closely affiliated conspecifics (members of the same species). But precisely because these challenges are universal and recurring, it is likely that the behavioral inclinations that support them have a biological and genetic substrate.

It is also likely, given what we observe in closely related species such as chimpanzees, that humans have biologically conditioned inclinations both to dominate other conspecifics, and to submit to those dominant, as well as more egalitarian tendencies that lie at the foundation of our ability to sustain democratic societies and collectively restrain would be dictators. These different and sometimes opposed impulses make human nature contradictory and human individuals ambivalent about their behavior, but they are the raw material from which we have fashioned a variety of political cultures. Variation in institutions, which I take to be formal descriptions of rules governing human interactions, are a reflection of political cultures which have varied considerably across groups and over time.

Hodgson argues, and I would agree with him, that individuals embedded in different cultures organized by different institutions will behave differently, above and beyond the differences that can be attributed to variations in the nonhuman environment. But the influence of culture or formal institutions (I do not suggest that they are coextensive) needs to be understood with reference to the aggregated characteristics of those whose behavior they influence. We cannot make progress by suggesting that these forces operate at some higher level still, and that we need entirely different models to understand behavior in different cultural milieus. Cultural variations represent differences in the patterns that emerge from aggregation of the beliefs (about states of the world and desirable ends of human activity) of individuals who comprise a group. They represent different parameter values in what must eventually be seen as a general model of human behavior.

The fundamental position of the "old" institutional economics, of which I take Hodgson to be a proponent, is that institutional variation, like the cultural variation in which it is embedded, influences human behavior in ways that cannot be swept back to variation in the non-human environment. I fully endorse this position, but the most interesting current work reflecting this point of view is not being carried forward by people who call themselves institutional economists. It moves forward under such banners as mechanism design, or among those formulating tax or regulatory policy, or among advisors to transition economies, which have proved to be real life laboratories for studying the consequences of major variation in rules. Here we find practical applications of institutional economics, but it is perhaps telling that so little of this work is today called by this name.

If institutional economists wish to have a significant impact on public policy, as did John Commons, they should be at the forefront of discussions of regulatory policy, or how to design spectrum auctions, or what kind of legal system would best serve a transition economy. If, on the other hand, institutional economics has larger ambitions to serve as a launching pad for integration of the social sciences, then it needs to move beyond its current niche habitat (largely backward looking intellectual history) and participate in a broader scientific effort to bring the social, behavioral, and historical sciences together using an approach that integrates findings from the biological and related sciences as well as evolutionary theory.

This appears to be Hodgson's aspiration, and indeed one of the more provocative parts of the book is his discussion of how institutional economists gradually abandoned Veblen's interest in a Jamesian human psychology that recognized instincts (even if we don't call them that today) and the often contradictory impulses that influence human behavior ("the original foundations of institutionalism, in Darwinism and instinct psychology, were removed" (p. 181). Still, I think Hodgson exaggerates here. He has made a case that Veblen embraced these views, but it's not at all clear that many in the German historical school, or other American institutionalists, such as Commons, did.

Whatever may have been the degree of interest of early institutional economists in psychology, if we are serious about bringing biology back into the social sciences, and

consider evolution not just as a metaphor for the differential survivability of firms or technology, it is desirable that, as economists, we educate ourselves about debates within evolutionary theory, most particularly those surrounding multilevel selection (Wilson and Sober 1994, Sober and Wilson 1998, Boehm, 1999, Field, 2001). Understanding what's at stake here is essential if we wish critically to evaluate work that has been done in the past quarter century in such areas as sociobiology or evolutionary psychology. There is much to learn from reading these literatures, but it is a mistake to suspend critical judgment in a headlong rush to embrace them. Institutions, however we agree to define them, are a subset of culture, and developing a clear understanding of the ways in which biology does and does not limit cultural variation and political behavior is essential in piecing together a framework for understanding human action that integrates genetic, cultural, and environmental influences.

Although I would expect Hodgson to be sympathetic towards such a program, he has not seriously engaged the literatures necessary in order to carry this forward. This leads me to question whether intellectual history of this sort, which sometimes (although less frequently here than in other work in this genre) bogs down in the reification and subsequent manipulation of concepts and ideas, can serve as a foundation for the kind of focused discourse necessary to overcome the isolation and fragmentation of the disciplines and subdisciplines. Relatively successful as intellectual history, this book is disappointing as a contribution to social or behavioral science.

If we are to make progress in understanding and explaining human behavior, we need a general theoretical approach which is capable of handling the variety of conditions under which humans do and have lived. Since we are partially programmable, such a model or theoretical approach will make different predictions depending on differences in the parameter values assumed. There is more at stake here than semantic differences over what we mean by the word "different."

The Central Appeal of Institutionalism

No social scientist can deny that taking a broad view of history and geography, humans exhibit and have exhibited an enormous variation in their social organization and the level of technological proficiency that has undergirded their material standard of living. If we step back from this variation and think in very general terms, there are ultimately three types of explanations for why this variation exists: a) it reflects systematic genetic differences among different human groups, b) it is the result of different but rational responses to varying environmental conditions, and c) it reflects variation in culture.

A variety of evidence suggests that the first type of model cannot provide us much traction. Although there are significant genetically determined phenotypical differences influencing physical appearance, cognitive abilities, and behavior within any group of humans, the systematic differences among *groups* are relatively small. It is quite unlikely that these differences can have had more than a small impact on observed cross sectional socio-economic variation among groups. Secondly, if we consider the historical evolution of human groups, it is also quite unlikely that there has been very much genetic

evolution affecting significant species typical behavioral or cognitive traits since the Neolithic revolution.

There has been some. The most well documented is the evolution of lactose tolerance among populations with a pastoral or dairying tradition (most adult humans, like most other mature mammals, cannot digest dairy products). These variations among groups would, since they are the consequence of dependence on dairying, have to have arisen subsequent to the agricultural revolution. Another often mentioned case involves the high frequencies of genes predisposing to sickle cell anemia in populations of African descent. The relevant gene in its homozygous form causes the debilitating disease but in its heterozygous form provides resistance to malaria. Because of its fitness enhancing effect in regions prone to malaria, natural selection has led to increases in gene frequencies in populations long resident in such regions, whereas obviously selection in malaria free zones has not permitted these genes to gain much of a foothold.

What is perhaps striking about these instances, however, is that they very nearly exhaust the set of well documented cases of *recent* environmental influences on the genetic composition of human groups.

An inescapable question is whether it is possible that there have been others of greater behavioral significance. Could it be, for example, that populations with a tradition of technological innovativeness have coevolved cognitive adaptations that make them more facile, on average, at navigating within such environments? One reason for doubting that this has been true, aside from the relatively small differences between measurements of average group IQs, is that within modern societies, those with the greatest command and proficiency with technologies have tended to have high income, wealth, or socioeconomic status. In country after country these variables have also tended to correlate with lower fertility, a maladaptive behavior with respect to natural selection and reproductive fitness, but one that would have acted against gene culture coevolution in this direction.

Until the 1930s hunter-gatherer societies existed in the interior of New Guinea completely isolated from any form of contact with more technologically advanced civilizations. Denizens of these cultures experienced relatively little difficulty communicating with intruders at first contact and have been successful at quickly learning how to operate the tools and technologies they brought with them. In sum, it is unlikely that there are or have been significant genetic variations among *groups* of humans that can account for observed differences in social organization and material standards of living (see Lynn and Vanhanen 2002 for contrary arguments).

The second general type of explanation focuses on environmental influences, a broad category into which is often put such obvious factors as temperature and rainfall, soil, and natural resources, but which can also be understood to encompass technological availabilities. There is little question that some of the differences we observe in social organization and material culture reflect responses to different environmental conditions. and it has been the dream of many that this mechanism could effectively deal with the

problem that observed variation in rule structures appears to pose for a general theory. One sees variants of this approach in the structural-functional tradition in sociology and anthropology as well as within economics, especially among early proponents of the "new" institutional economics (North and Thomas, 1973; Field, 1981).

If institutions and social structure were entirely the result of variation in climate or geography, which are almost entirely independent of the history of human cognition and endeavor, then we could legitimately view them as epiphenomenal – ultimately derivative of more fundamental givens. A passing acquaintance with historical and geographical variation reveals that such a conclusion is unjustified. Institutions and social structure often vary where environmental and technological conditions are quite similar, and sometimes remain unchanged in the face of substantial alterations in environmental conditions (Field, 1991).

It is this reality that poses the stumbling block to a theory of social and institutional variation that tries to explain such diversity entirely with reference to environmental factors. And it is this reality that draws thoughtful scholars again and again back to the traditions of the "old" institutionalism, of which Hodgson is an exponent, as well as to the efforts of traditional sociology and anthropology. The strength of the old institutionalism is that it acknowledges this reality: institutional/cultural/social structural variation influences human behavior in ways beyond what can be swept back to environmental differences.

I distinguish here between the old institutionalism, which focused on exploring the consequences of institutional variation, and the "new institutionalism," which, at least initially, tried to endogenize such variation. The drive to endogenize had both rational choice and Marxian variants, but in both instances the result has been to minimize the degree to which cultural or institutional differences could influence behavior in ways independent of the effect of variations in the nonhuman environment.

The characteristics (including beliefs and behavioral inclinations) of those other humans with whom one interacts, can of course also be considered part of one's environment, broadly considered It is also true that such non-human factors as climate and the current level of technological availabilities can and have been influenced by past human activity. But it is conventional, and I believe useful, to distinguish cultural from environmental explanations of behavior in this way: cultural explanations are those that attribute behavioral variations to variations in the aggregated characteristics (beliefs and behavioral predispositions) of the group in which an actor is embedded.

There are many definitions of institutions, but I understand them here to be formal rules governing human interactions.[5] Institutions are reflective of and part of the broader category of culture. Some aspects of culture reside or are embodied in artifacts and

[5] The distinction between formal and informal rules is admittedly somewhat arbitrary. If I go to coffee with you for the first time at 3 PM today, it is certainly not an institution. If I do it tomorrow, is it? There is no hard and fast line where such a regularity of behavior, reinforced by mutually realized expectations, "becomes" an institution.

written materials. But the central core of what we mean by culture are the beliefs and behavioral inclinations encoded in human brains as the result of teaching, imitation, and observation. Culture is neither more nor less than the aggregation of these encoded associations and beliefs, supplemented by external information storage devices such as stone tablets, books, or hard drives, or other artifacts including tools or structures.

The limitations of both institutional economics and the more orthodox traditions it frequently opposes are the result of continuing to act out a larger drama within the social and behavioral sciences whose basic themes are no different from what they were thirty or even one hundred years ago. On the one hand we have those who combine the basic principle of rational choice theory (maximization of utility functions reflecting stable and transitive preferences) with the additional restriction that these preferences are selfish in an attempt to construct an all embracing theory of human behavior. On the other hand we have institutionalists who, along with traditional sociologists and anthropologists, emphasize an independent causal impact for such concepts as culture, norms, social structure or, in this case, institutions.[6] The organizing principle of the former tradition has been methodological individualism, whereas for the latter it has been opposition to attempts to reduce or trace back the content of norms and social structure to characteristics of the individuals whose activity they influence or organize.

This play has been performed again and again, and both sets of actors have some good lines. The economic/rational choice defense of the principle of methodological individualism has merit, although this is not true of all versions of it. A sophisticated defense cannot claim that the current behavior or the past evolution of a unit can always be understood with reference only to the properties of the individual unit itself. It can claim, however that the behavior of the members of a group is ultimately explicable with reference to the non-human environment along with the aggregated characteristics of its individual members. What else, after all, can there be?

The institutionalist tradition, on the other hand, correctly stresses the large number of empirical and historical phenomenon that cannot be accounted for by rational choice models if they are coupled, as they so often are, with the assumption of selfishness in all spheres (people act so as efficiently to advance their material welfare). But the defense of such concepts as culture and social structure as supraindividual does not follow.

Rational choice models assuming stable, transistive, and narrowly selfish preferences cannot explain a broad range of important phenomenon, from voting to the sacrifices parents make for their children, to our ability to overcome free rider problems and initiate cooperate activity in small groups. The problem with the institutionalist critique, however, is that it has bought into a long tradition with firm roots in sociology and anthropology that really doesn't take issue with the assumptions about innate human psychology typically made by those operating within the rational choice traditions. *It*

[6] Independent in the sense that they are predetermined variables, and moveover that this predetermination does not result from a simple mapping from features of the non-human environment or available technologies. At some level of remove, of course, there is a path dependent history that has led the culture of a group or region to be what it is.

simply assumes that culture and civilization restrain these baser impulses, thus explaining all the phenomenon that the rational choice tradition is unable to account for. Having made this point, institutionalists then repeatedly criticize rational choice theorists for failing to acknowledge it. But for many, the critique is like water off the back of a duck, because the claims and the explanatory framework seem so egregiously to violate the principles of methodological individualism.

We cannot treat the "culture" and norms that enabled small groups of non-kin to form initially as a deus ex machina. "Culture" could not always have existed to restrain the otherwise disastrous outcomes that our innate psychological predispositions would apparently have lead us to. The persistent emphasis on the primacy of culture is to me reminiscent of the story of the person who interrupts the distinguished lecturer to explain that he is clearly unaware of the fact that the earth is a large plate sitting atop a giant turtle. When the surprised scientist asks what the turtle sits atop, the member of the audience harrumphs, clearly irritated at the ignorance of this supposed expert, and replies that, of course, it's turtles all the way down.

The solution to this origin problem is to acknowledge that our innate psychology is not as unremittingly nasty and brutish as Hobbes would have it. Our innate psychology enables, and initially enabled cooperation in small groups of 30 to 100 beyond what can be explained as the consequence of kin selection. The predispositions that make up our innate psychology, moreover, are the result in part of evolutionary processes that involved selection at multiple levels, including levels above the individual organism. None of this is inconsistent with the gene level perspective that has now, appropriately, come to dominate evolutionary theory.

Tocqueville saw small groups as natural, and in this he was correct: they form because we are inclined to form them. We are inclined to refrain from harm even when the logic of the one shot Prisoner's Dilemma is indisputable. And we are inclined to retaliate when wronged, or when a group norm is violated, even when it may not be in our material interest to do so. These traits could not have been favored initially by individual organism level selection: but groups that had more individuals so inclined tended to grow faster, and under the right demographic conditions, although such traits were shrinking in frequency at all times in every group within the population, they increased in the population at large.

Institutional economics must let go of the comfortable critique of rational choice theorists as "reductionist." There is nothing wrong with criticizing rational choice models when they explain or predict poorly. But it is too easy simply to damn analysis as reductionist. Good science requires that we try and break down a phenomenon into its constituent parts. Where we confront limitations due to deficiencies in our current level of scientific knowledge there is justification for conducting analysis at a higher level. But absent such limitations, we should proceed. The problem is not that the rational choice tradition champions methodological individualism. The problem is that it so often, explicitly or implicitly, couples the assumption of stable and transitive preferences with that of selfishness. These type of models don't simply say that people act in satisfaction of their

desires. They don't simply say that preferences are stable, transitive, and perhaps monotonic. By assuming narrowly selfish preferences they predict that people act in all instances so as to advance their material well being. Although true in many contexts, it is not so in all, as an important range of experimental and observational data makes clear.

Standard microeconomics is the study of the operation of foraging algorithms in an environment never anticipated by the forces of natural selection that refined them. Constrained maximization tools work relatively well in modeling the operation of such algorithms. But our evolutionary history has endowed us with other algorithms that also facilitate our survival and are particularly important in conditioning political behavior. These include a willingness some of the time to play a strictly dominated strategy in a one shot PD, such that cooperative and potentially repeated interactions among non-kin can begin, a willingness, once continuing interaction has begun, to devote inordinate amounts of energy to detecting rule violators, an obsession that can distort our ability to think logically (Cosmides and Tooby, 1992), and a willingness to engage in costly punishment of those identified as defectors (Fehr and Gächter, 2000). Rational choice theory puts itself in a box if it assumes that our behavior is driven exclusively by our foraging algorithms.

It is our biologically mediated behavioral inclinations that, in the first instance, enable reciprocity and cooperation among non-kin in small groups, not the "invention" of culture. Once we move beyond small groups, the norms, expectations, institutions and rules that emerge or that we construct are essential in allowing the operation of larger organizations and states. But these cultural or institutional innovations are built upon a biological substrate.

Until this argument is fully engaged the traditions of institutional economics and its cousins in disciplines outside of economics will continue to participate in a pointless and increasingly stale *methodenstreit* that will not advance our understanding of human behavior.

Endogenous Institutions

There is a long tradition running from technologically determinist passages in Karl Marx to early new institutional history writings of Douglass North that suggests that institutions and, perforce, culture, are ultimately epiphenomenal: reflective of more fundamental givens such as resource endowments and technologies. Thus, in a perhaps bowdlerized version of Marx, the handmill gives you the feudal landlord, the steam mill the industrial capitalist. In the early Northian version, "efficient" institutions arise because they offer a free lunch in comparison to the alternatives: someone can be made better off without making anyone else worse off, so an old rule is discarded, and a new one brought in. The triggers are assumed to be changes in land-labor ratios or available technologies. In both of these frameworks institutional variation is a surface phenomenon, to be studied as confirmation of the operation of more fundamental forces, but without much independent influence itself.

The more recent writings of North represent an important about-face on this issue. First of all, he now acknowledges that simply because a different institution might by Kaldor-Hicks efficient, in the sense that winners could in principle compensate losers and still be better off, there might not always be mechanisms that would automatically (and costlessly) arise for effectuating that transfer. Thus inefficient institutions might persist. If one adds to this the possibility that there could be multiple efficient institutions for a society with given technologies and preferences depending on how endowments were initially distributed, it begins to be clear that there is a potentially broad range of rules that might characterize a region with given resources and access to a particular set of technologies.

Moreover, North now acknowledges the influence on behavior of ideology and norms. What this represents is recognition that individuals may not be driven solely by wealth maximization, and that deviations from this assumption may go beyond simply variation in preferences for leisure vs. material goods. The net effect of these alterations is to reinforce the conclusion that institutions or rules may differ across space and time where resources and technologies are similar, but may also be similar across space and time when resources and technologies differ or change (Field, 1991). Rule variation, therefore, can be *consequential*, which was, of course, the central organizing principle of the old institutionalism.

This intellectual move away from the more extravagant claims of the early new institutionalism is mirrored within literatures attempting to understand biological influences on behavior. In the 1970s, sociobiologists moved aggressively to explain all human behavior as the consequence of inclinations favored evolutionarily by fitness maximization (Wilson, 1978). Sociobiological explanation is not in principle the same as rational choice theory assuming stable, transitive and narrowly selfish preferences. In the latter framework individual organisms act so as to maximize their own material welfare. In the former natural selection favors genes that predispose to behavior by the organisms containing them that leads to increases in the frequency of such genes in future generations. But if gene interest is assumed more or less coextensive with organism interest, and if selection is assumed to operate at levels no higher than that of the organism, then the conclusions reached about human behavior are very similar – thus apparently providing such rational choice models with a biological underpinning.

There is now, however, greater acknowledgement that much human behavior in our current environment is simply maladaptive. Evolutionary biologists such as Cosmides and Tooby view our inclinations as having been honed in a two million year Pleistocene; in some cases the behavioral inclinations selected may not be fitness enhancing in the current environment because of the relatively slow pace of genetic evolution in humans. In other instances cultural and technological changes may have enabled and reinforced maladaptive behavior, such as fertility control. The acceptance of both of these propositions represents a step forward from 1970s sociobiology, but also represents, as in the case of the new institutionalism, some dulling of its apparently revolutionary implications. What still remains very controversial is the proposition that some

genetically influenced behaviors that continue in large measure to be adaptive were originally selected for at levels above the level of the organism.

Among mainstream economists today (2003) it is much more widely accepted than was true fifteen years ago that institutional, cultural, and other normative features of economies can vary in ways that have profound influences on economic outcomes, that these rules are human creations, sometimes planned and thus the subject of political choice, that it makes a great deal of difference whether we get them "right" and that this won't automatically happen. The examples of the different growth trajectories and environmental records of East and West Germany before reunification, or North and South Korea are obvious examples, but economic history provides many more, from the economic consequences of slavery in the American South, to the possible influence of different legal traditions on the structure of financial systems in common law and civil law countries (LaPorta et al., 1998). Thus, with the partial retreat of the "new" institutionalism, the fundamental insight of the old institutionalism has been reaffirmed.

Institutional economists about whom Hodgson writes viewed it as important to describe the rules – legal and otherwise -- within which economic activity took place. The key institutional issue in economics thirty years ago was whether, in thinking about static general equilibrium analysis, there were three categories of predetermined variables (tastes, technologies, and endowments), or whether there was also a fourth (rules) (see Field, 1979, 1981, 1984, 1991). Mainstream economists and game theorists did not speak with one voice on this issue, some readily granting the influence of institutional variation on outcomes, others, in a number of traditions, claiming that they were epiphenomenal and could be swept back in an explanatory sense to the more basic triad of tastes, technology and endowments.

The waters were muddied by the proclamations of the new institutional economics. The new institutional economics promised that it would give institutions their due, but not in the fuddy – duddy tradition of Commons, Mitchell, or the German Historical school. On the one hand, there was a drive ultimately to eliminate the role of rules as an independent influence on outcomes. On the other hand there was an intent to work out the implications of the Coase theorem writ large. The Coase theorem stated that in the absence of transactions costs rule structures would not affect the sectoral allocation of inputs; in that sense they would not matter. In the presence of transactions costs, however, some rules might be more efficient that others; the new institutional economics would use these principles to explain why we have the rules that we do.

By the early 1990s the new institutional economics had run into some explanatory brick walls that caused its leading practitioners to back away from some of its early objectives. In particular, as noted, scholars such as North began talking about the role of ideology and the possibility of the persistence of inefficient rules. But once one allows for this, it is clear that rules can have independent influences. Moreover, it is no longer obvious what are the distinguishing feature of the new institutional economics. Currently, it seems to be the use of the analytical tools and vocabulary of game theory to analyze or

interpret historical institutional arrangements. This is a considerably more modest enterprise, and it is much less clear that the propositions advanced are falsifiable.

Hodgson and I are in broad agreement that history matters and that, because this leads to institutional variation that at any moment of time is not epiphenomenal, institutions matter. Where we may differ is on where we should go from here. Suppose we consider the issue of static allocation that dominated much of economic theory at least through the 1970s. I have argued in a series of papers that if we wish to construct general equilibrium models, it is important to acknowledge a fourth category of predetermined variable beyond technologies, endowments, and preferences. This category can be called culture, norms, or institutions. Whatever we call it, it consists of beliefs, not just about how the non-human world works (we can think of this as technology), but how other humans will act or react, and to some degree, what are the appropriate ends of human activity (thus these beliefs shade over into preferences).

If we grant this perspective, however, is it helpful to say we need a different *theory* to analyze static allocation in North as opposed to South Korea, or are we rather talking about the same general theory with different parameter values? We need to be precise, and Hodgson is looser than I would prefer in his use of the term theory, sometimes speaking of it as simply a set of concepts or categories (for example in discussing why empirical observation must be "theory laden"), while at other times suggesting that the term necessarily implies some positing of causal relationships among these categories.

While a number of economists have recently tried to build rational choice models incorporating the influences of social norms (e.g, Akerlof, 1980; Kevane and Wydick, 2001), this leaves open the question of their analytical status. Can norms and institutions truly be understood as forces external to the individual, like the weather? If they can be viewed as external to an individual, can they also be viewed as external to the *individuals* whose behavior they influence?

Economists have differed with respect to the analytical status of norms. Both Hodgson and I have reacted to the limitations of economic or rational choice models as they in fact confront us in much economic discourse. As an institutionalist, Hodgson has some sympathy for the sociological, anthropological tradition of Durkheim and Malinowski, with its emphasis on institutions and culture as superorganic, with dynamics of their own, "independent" of the individual humans whose activity they organize. Hodgson continues, I think, to embrace this distinctive tradition. My argument here is that we now need to move beyond the correct claim that culture, norms, or institutions represent an influence on behavior beyond those of technologies, endowments, and preferences, and locate these norms and institutions in the aggregated characteristics of those individuals whose behavior they influence, augmented to some degree by the physical environment they create.

Rational choice models come in at least three canonical flavors, and it is important, in a specific instance, to be aware of which versions we are dealing with. It is also important to keep in mind that these models involve assumptions about cognition as well as goals

and objectives; the term rationality can mean different things in the two spheres. My emphasis in the trichotomy below focuses principally on behavior, as opposed to cognitive aspects of rationality:

Rational choice models, Level 1: People act in satisfaction of their desires.

It is not worth spending a great deal of time on this variant, because there is no conceivable empirical data that could refute it. Models based on this premise can't be rejected with observed behavior and it is pointless debating their scientific merits with proponents.

Rational choice models, Level 2. People have preferences that are stable and transitive, and act on the basis of these preferences.

Stability is based on the proposition that the basic goals of humans, like other living organisms, display some time consistency. Transitivity is typically justified by appeal to a money pump argument. The appeal of assuming that if an individual prefers A to B and B to C, she will not prefer C to A is ultimately supported by an evolutionary claim: individuals whose choices did not adhere to this rule would be subject to extinction because one could suck all their wealth from them using a money pump. Suppose an individual has revealed that she prefers A to B, B to C , and C to A. Start by giving her C. She should then be willing to pay something to swap C for B, and something more to swap B for A. But now we can ask her to pay a third time to swap A for C, having induced her to make three successive payments to obtain what we originally gave her for free! Presumably individuals with such preferences would be evolutionarily disfavored

A variant of Level 2 rational choice model building, attributable to Samuelson's influence, insists that preference structures be derived from observed behavior. If we add to stability and transitivity the assumptions of convexity, monotonicity, reflexiveness and completeness, then level 2 specifications are subject to empirical refutation through demonstrated violations of the weak and strong axioms of revealed preference. For example, a violation of the weak axiom of revealed preference occurs when an individual chooses commodity bundle A when they could have chosen B, revealing a preference for A over B but then, faced with a different budget constraint, chooses B when they could have chosen A, apparently revealing a preference for B over A. The strong axiom is violated if an individual indirectly reveals a preference for A over C (by first preferring A to B and then B to C) and then reveals a preference for C over A.

Ideally, one could imagine rational choice methodology as a means for developing predictive models of human behavior: extracting from the revealed preferences displayed in observed behavior concise mathematical specifications of individual utility functions that, when combined with the maintained hypothesis of maximization, enabled reliable out of sample predictions. In practice, however, very little actual work has ever been done along these lines. The most common use of the revealed preference methodology has been to evaluate the welfare consequences of proposed policy interventions in an ordinal sense (they will make people better off or worse off). I don't mean to trivialize

these conclusions which are sometimes not at all intuitive, but it would be a great stretch of the imagination to claim that the principle of revealed preference has led to the development of a robust methodology for predicting levels of human behavior.

In rational choice models, level 3, people are assumed to have stable, transitive and narrowly selfish preferences which lead to behavior efficiently advancing the material interest of the actor.

Level 3 models often use the assumption of wealth maximization as a practical means of operationalizing selfishness. The distinction between level 2 and level 3 is the insistence in the latter on the importance of assuming human selfishness and denial in the former that this assumption has any necessary relation to the method (Gintis, 2003). In practice the distinction is not quite so clear, because of the common assumption of monotonicity in revealed preference exercises. If money is viewed as the generalized good, monotonicity requires, everything else being equal, that more money is preferred to less. Thus, experimental evidence indicating that under the right circumstances people will persistently leave money on the table, while clearly an empirical refutation of level 3, also poses some challenges to level 2.

So far I have said little about possible limitations on human cognition. Rational choice models often dispose of the problem by assuming perfect information. Cognition is costless, errorless, unbiased. The assumption of rational expectations is somewhat less restrictive: it requires that people use all available information processed according to the best available logical and statistical algorithms in forming their beliefs about the world. The assumption is a bit more realistic because it acknowledges that some information might not be available, and implicitly suggests another optimization problem in the sense that more information could presumably be made available through the expenditure of additional effort or resources.

If we take some version of level 2 as part of what is meant by traditional microeconomics, do the deviations we need to accept in order to account for political behavior lie only in the realm of cognition? That is, does human bounded rationality, to use the term popularized by Herbert Simon, suffice to explain why humans behave differently from what canonical versions of the theory predict? While it is of course true that human cognition is imperfect in many ways, the answer to this question is negative. This is my objection to the otherwise estimable work of scholars such as Elinor Ostrom: she is too willing to attribute the behavioral deviation to the Simon tradition of bounded rationality (Ostrom, 1998).

The human predisposition to play cooperate in a one shot PD, or make voluntary contributions to public goods, or engage in costly punishment of norm violators, cannot be swept back to some imperfection in cognitive capabilities. Before you can have the parametric prices that confront choosers in intermediate theory textbooks you need trade, and before trade you need relations of reciprocity among non-kin, and to get that you need a human ability to overcome one shot PDs that must lie somewhere else than in our predispositions for self preservation. I have described elsewhere how genes predisposing

to such behavior can be favored by natural selection. Any understanding of who we are and how our societies got to where we are needs to confront this argument, or else engage in (invisible?) hand waving. The dream of deducing social organization from some posited original state as the result of behavior by rationally choosing narrowly self interested agents is a chimera. The basis of the appeal of the sociological and anthropological tradition, with which the old institutional economics has many affinities, has been that norms and institutions, externally posited and imposed, have seemed to provide a resolution to a gaping lacuna in rational choice theory at least of the level 3 variety. But the development of culture presupposes continuing interaction among non-kin adults, leaving unanswered the question of how such interaction originates. It can't be turtles all the way down.

Practitioners of rational choice methodology, particularly economists, have been ambivalent about cultural explanations of behavior and, perforce, those relying on institutions. My proposal for a rapprochement is this: 1) those in the institutionalist - traditional sociological/anthropological tradition accept a sophisticated version of methodological individualism. 2) Those who find level 3 rational choice models appealing abandon the insistence on strictly selfish preferences and the belief that natural selection necessarily provides an underpinning for them. I have no illusions that negotiating this Peace Plan will be any easier than solving the Israeli – Palestinian conflict, but I would like to lay out the logic for it.

The vision I have for understanding both the universal and variable features of human organization is explicitly biological and evolutionary, and not just evolutionary in a metaphorical sense. Take as our starting point what we know about language. Two salient features are worth noting. First, vocabularies very greatly across the world and these differences are clearly learned cultural phenomena. Yet all of the roughly 5,000 known human languages obey a set of basic rules of grammatical structure that reflect design features of human cognition. This may seem marvelous or difficult to accept at first glance, but it is upon reflection no more surprising than that our genetic material also includes detailed assembly functions for such differentiated organs as our liver or our spleen. The design features means that with respect to learning of grammar we are differentially predisposed at certain developmental stages to form certain associations.

If we grant that the vocabularies of language are cultural features, are we correct in arguing that they are emergent properties, in the sense that they cannot be understood by referring to the aggregated properties of those individuals whose communication they organize? I think upon reflection, the answer must be negative. Language is based on a set of reciprocal expectations of how others will interpret certain sounds I may make. I would claim that language can be understood *entirely* with respect to properties of the individuals whose intercourse it organizes: in particular learned and shared vocabularies embedded within a universal and genetically based grammatical template.

Whereas it is not possible to comprehend a language by looking only at properties of one particular individual whose communication it organizes, it is possible to say that language is neither more nor less than the sum total within a group of shared expectations

about the meaning of sounds that have been acquired through a process of association and statistical learning. The particularities of a language (not the universal structure that is shares with all others) are simply the set of those expectations among the members of the collectivity whose behavior it facilitates.

We need to think about institutions and norms in the same way: the differences among them reflect particular patterns of shared expectations about how others in the collectivity will respond (positively and negatively) to different states of the world and in particular specific individual actions. These are acquired through the standard techniques of association and statistical learning stressed by behaviorists, although this acquisition is attained within a framework of hard wired predispositions to learn in certain directions.

Language represents solution to a coordination problem: the selection of one from a number of multiple possible equilibria. The achievement of a cooperative outcome in what might be a one shot PD is a much greater challenge, because the only Nash equilibrium in that game is inefficient. Political organization requires that people, literally or figuratively, leave money on the table. They must be willing, initially, to play a strictly dominated strategy.

The assumption of wealth maximization is a useful assumption in many contexts. What is not reasonable, however, is the assumption that it prevails in all. History, experience, and experimental evidence have documented that humans, even when they fully understand the nature of the experiment or the situation the find themselves in, will sometimes play strictly dominated strategies. This is true, for example, in one shot PDs, in ultimatum games, or in multiplayer PD games involving the collective provision of public goods and the opportunity for costly punishment of those who don't contribute. It is simply not possible to provide a rationale based on level 3 modeling for the play of a strictly dominated strategy.

Sophisticated rational choice modelers, when called upon to defend rational choice methodology, will almost invariably defend a version of Level 2, but in practice it is the unrestrained application of level 3 methodology in areas where the evidence simply won't support it that has given rational choice modeling a bad name. Those who have openly defended Level 3 methodology have often appealed to evolutionary arguments in support of the assumptions of human selfishness. Humans are selfish because we have inherited genes that so predispose us, and if our great grandparents had not had these genes, we would not be here. The specter of Charles Darwin has always seemed to lurk in the background for those who would challenge Level 3 models. On the other hand, those who want to force Level 3 into every nook and cranny immediately run into difficulties dealing with parental sacrifice for children. Most economists and biologists now agree that the Hamilton kin selection mechanism adequately accounts for parental sacrifice. But the logic of that mechanism requires acknowledgement that natural selection doesn't necessarily make humans individually selfish in all venues.

Natural selection will favor genes that predispose in favor of organism behavior that results in higher frequencies of such genes in future generations. For humans, this often

means predisposing in the direction of behaviors that do in fact efficiently advance the material well being of the actor. But not always, as the example of parental sacrifice indicates. The logic of multilevel selection models, necessary to account for the predispositions that enable us to solve one shot PDs and the multiperson PDs implicit in voluntary provision of public goods problems, necessitates special demographic assumptions about population fragmentation and regrouping over time. But it relies on the same principle: genes that influence the vessels containing them to act in ways that foster higher rates of increase of these genes will be favored, whether or not this influence is always in the best interest of the vessel.

Conclusion

The founding fathers of institutionalism were a disparate group with disparate interests and perspectives, but they appear to have been motivated by two general goals. The first, more activist and normative, was to provide policy recommendations as well as an intellectual foundation for government intervention as a means of improving the operation of a capitalist economy. The second was to use the positive insights of institutionalism as a foundation for a more general theory of human behavior.

Part IV of Hodgson's book is concerned with this second agenda, which has also formed the focus of my essay. But parts I – III are about the history of ideas, which is reflective of the emphasis on intellectual history which dominates institutional economics today. These sections would serve as a good starting point for someone interested in an intellectual and political history of institutionalism, both in Europe and the United States. The scholarship here is generally solid, although some of the summary statements, sound good until one deconstructs them. For example (p. 200), Hodgson describes how Lionel Robbins and Parsons "carved up the social sciences: sociology was to be concerned with the social determination of goals; economics with the study of the rational choice of means to pursue these ends." If they ever had an agreement, I don't think Parsons got it in writing, because I doubt Robbins was prepared to cede to Parsons the idea that human goals were socially determined, nor have generations of subsequent economists, who have had little truck with sociology, Parsonian or otherwise.

Another example: Hodgson says of Veblen that he "avoided the three reductionisms of methodological collectivism, methodological individualism, and biological reductionism" (p. 140). Relative to other work in this genre, Hodgson is restrained in the tendency to reify concepts. But he does succumb now and then. If one is going to talk in these terms, one needs very careful definition and analysis of what is meant – which is lacking here.

The claim that we need different theories of human behavior for different cultures, Hodgson's main theme throughout all four sections of the book, arguably turns on what we mean by different. Since all human groups are remarkably similar from a biological perspective, I question the desirability of rejecting the pursuit of a unified theory of human behavior, and have outlined in this essay some of the directions in which we might proceed in order to construct it. Perhaps institutionalism's fate can be attributed in part to a series of intellectual accidents, such as Marshall's replacement by Pigou at Cambridge. But certainly the power of institutionalist ideas, and the ability actually to

make progress on the normative and positive agendas identified above have had something to do with it.

The approach of the old institutionalism has not been wrong, but it has been limited, because it hasn't successfully explored the origins of institutions beyond making the correct point that these variations in them can't be entirely swept back to variations in the non-human environment. Historical economics is the time series version of cultural economics. Hodgson could equally well have asked how economics forgot culture, and written an almost identical book on the problem of cultural specificity in social science. The problems of and challenges facing institutionalism are cut from the same cloth as those affecting traditional sociology and anthropology.

Many voices have been raised calling for the introduction of more institutionalist ideas in the training of economists. A critic might ask why "the institutionalist propaganda that has been prominent in the economic discussion of the past 15 years in this country, has not been more productive of concrete works and changes in the curriculum of our graduate schools."

The last sentence might have been written recently, but in fact was penned in 1931 (Burns, 1931, p. 80). Almost three quarters of a century have now passed. The central truths of institutional economics have not been lost. But those who call themselves institutionalists have defined the field largely as backward looking intellectual history, ceding the important policy arena to mechanism designers, those who write tax law, and Jeff Sachs. These scholars, although not calling themselves institutionalists, are proving to be the truer heirs of John Commons. As far as Veblen, well, he is sui generis, but if institutionalism is to be a foundation for a broader unification of the social sciences, a more systematic and critical treatment of biological and evolutionary theory along with experimental evidence is needed than is here provided.

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