Introduction

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The basic tenet of rational choice theory is that both behavior and social institutions arise from the action of individuals, each of whom makes their own decisions.[*] The theory also focuses on the determinants of these individual choices. It holds that individuals have complete and transitive preferences over the options available to them.[1] These preferences allow the options to be ranked according to their relative desirability. Rational actors take account of the available information, the probabilities of attaining their preferred ends, and the potential costs and benefits of doing so. Ultimately, they act consistently to choose the course of action that they deem to be optimal for them.

Due to its emphasis on methodological individualism – the contention that social explanations ultimately must be derivable from facts about individuals – and its ostensible commitment to self-interested behavioral assumptions, sociology has long been blind, if not downright hostile, to the virtues of rational choice.[2] Since I am a sociologist, perhaps it is worth retracing my path to intellectual heresy and the dark side. This is a long story.

It begins, I suppose, in high school in Worcester, Massachusetts. My father, a biochemist, was a passionate believer in science. Science had enabled him to escape a dreadful childhood of poverty and parental irresponsibility in his Maxwell Street neighborhood – Chicago's answer to Manhattan's Lower East Side. Over dinner, he would regale us with descriptions of his research and tales of academic politics. Truth to tell, the intricacies of his research seemed beyond me; they were exceedingly technical, involving benzene rings, hydroxyl groups and a multitude of other molecules and interactions. Nevertheless, I tried to grasp the fundamental ideas lying behind the research.

Although biochemistry is a resoundingly empirical field, my father was also interested in general theoretical questions. During much of his career he sought to understand a very deep problem: namely, how it is that extremely small amounts of a hormone can have such massive effects on entire organisms. Rather than exclusively focusing on one or more specific hormones, like adrenaline or ACTH, he sought to identify the fundamental mechanisms behind all hormone action. In this quest, he was influenced by friendships with two celebrated scientists – Norbert Wiener of MIT, and Albert Szent-Györgyi of the Marine Biological Laboratories in Woods Hole. From Wiener, he learned about information theory.[3] From Szent-Györgyi he came to a deeper appreciation of the importance of theory in biological research.

I never met Wiener, the father of cybernetics, but we spent a good deal of time with Szent-Györgyi at his beautiful home, which seemed more like a small estate, on Penzance Point in Woods Hole. The first Hungarian ever to win the Nobel Prize (for his work on ascorbic acid), Szent-Györgyi was an immensely charismatic figure. In addition to his illustrious scientific career, he had played an important role in the Hungarian Resistance, causing Hitler to issue a warrant for his arrest. After the war, it was rumored that the Soviets had offered him the Hungarian presidency. Evidently, he turned it down.

Prof, as he was known in his circle, was also a legendary storyteller. I fondly remember the times he took me fishing in his small skiff. He joked that when he went fishing he always used a large hook, so that he wouldn't catch a large fish rather than not catch a small one. Of course, this was a scientific metaphor: an injunction to ask big questions rather than small ones.

Another one of his stories was memorialized by the Czech poet (and immunologist) Miroslav Holub (1977). It goes like this. A small group of Hungarian troops was camped in the Alps during the First World War. Their commander, a young lieutenant, decided to send out a small number of men on a scouting mission. Shortly after the scouting group left it began to snow, and it snowed steadily for two days. The scouting squad did not return, and the young officer felt guilty for having sent his men to their death. Suddenly, unexpectedly, on the third day the long-overdue scouting squad returned. There was great joy, huge relief in the camp, and the young commander questioned his men eagerly. 'Where were you?' he asked. 'How did you survive, how did you find your way back?' The sergeant who had led the scouts replied, 'We were lost in the snow and we had given up hope, had resigned ourselves to die. Then one of the men found a map in his pocket. With its help we knew we could find our way back. We made camp, waited for the snow to stop, and then as soon as we could travel we returned here.' The young commander asked to see this wonderful map. It was a map not of the Alps but of the Pyrenees.

This story likewise points to the importance of theory in scientific research. Research necessarily plunges investigators into a series of uncertainties. How do they know that their question has an answer? Or that they possess the requisite tools to dig the answer out? Or that the investment of time and resources will pay off and not be a waste of time? These uncertainties tend to generate anxiety. That anxiety can be allayed if there's a map – even an inaccurate one – purporting to tell investigators how to proceed, and that ultimately their goal is attainable.[4]

All this to explain that already in high school I was primed to believe in the signal importance of theory in scientific research. When my interests turned to social science, the most obvious theory on offer to someone of my generation was Marxism. Indeed, one of the reasons I chose to attend college at Columbia was to study with C. Wright Mills, then one of the most prominent Marxian intellectuals in the United States. Unfortunately, Mills died of a heart attack during my freshman year, and I never got to set eyes on him. My familiarity with Mills's books, however, did manage to impress a younger sociologist, Immanuel Wallerstein, whom I met at a reception to recruit sociology majors. He invited me to visit his office to chat whenever I felt like it. I took him up on the offer. With Wallerstein's considerable help, and that of the College department's chair, Daniel Bell, I completed my undergraduate degree.

I decided to stay on at Columbia for graduate school. It so happened that theory had an exalted place in the culture of the Columbia graduate department.[5] The king of the hill was Robert K. Merton, probably the most celebrated theorist in the entire discipline. Merton was an august and forbidding silver-haired figure who had the elegant bearing of a Church of England bishop, or perhaps (I imagined) even of the Archbishop of Canterbury. Only later did I learn that he was born Meyer Robert Schkolnick, and that his social origins were not that different from my father's.

Among his many other accomplishments, Merton was the founder of the sociology of science and a scholar's scholar.[6] He was also at pains to distinguish himself from his principal rival at Harvard, Talcott Parsons. Whereas Parsons was promoting what Mills (1959) had famously disparaged as 'grand theory', in response Merton advocated 'theories of the middle range'. Middle-range theory was all about the empirical implications of substantively delimited questions. Instead of writing nostrums about the sources of social order, as Parsons had done, Merton advocated theories of specific kinds of social phenomena like crime, bureaucracy and science. Merton's longstanding productive collaboration with his colleague, the brilliant methodologist Paul Lazarsfeld, had helped to establish a departmental consensus that theory always needed to be wedded to evidence.[7] Although this view now seems anodyne, it was hardly so in the 1960s. At Harvard, few of Parsons's theories had any clear empirical implications at all, and sociology at Chicago – the other major program at the time – was primarily descriptive, and placed relatively little emphasis on theory.

At the time, Merton's middle-range theory had little appeal to me and, like many other students, I found him to be intimidating. As a result, I continued working with Wallerstein, who was developing his structuralist world-system theory (Wallerstein 1974). My initial forays into theory likewise were structuralist. My first book, *Internal Colonialism* (1975) dealt with a lacuna in Marxian theory. If, as that theory contended, the engine of modern history was class conflict, then Marx had precious little to say about precisely how the proletariat acquired *Klasse an sich* – that is, class consciousness. Or, for that matter, any other kind of collective consciousness. *Internal Colonialism* was about a different type of collective consciousness – nationalism – one that seemed anomalous from strict Marxian premises. My argument in that book was influenced by Wallerstein's contention that development in the core regions of the world economy was at least partly dependent on their generally malign interventions in the peripheries they controlled.[8] Beyond that, however, it proposed a structural explanation of the salience of national identity – the cultural division of labor. This theory is the subject of Chapters 4 and 5 in this volume.

I accepted an assistant professorship at the University of Washington in 1970 knowing not a whit about the place. It was about as far from the Ivy League as one could get, at least in this country. As Wallerstein's research assistant, I had been exposed to a wide swath of literature on modern European economic and social history. For this reason, the only Washington faculty member I had ever heard of was the economic historian, Douglass North. After settling in (and completing my dissertation), I contacted North, who was most welcoming. Sometime thereafter, we organized a faculty seminar on long-term secular change. North alerted me to relevant talks in the Economics department and often invited me to his home for dinner. Several years later, we decided to jointly teach an undergraduate class on social change. North had a lot of energy and a lively sense of humor, but what most impressed me about him was his penchant for asking fundamental questions: he liked to fish with big hooks. North suggested that we begin the class with a week on economic theory, which he would cover, followed by a week on sociological theory, which would be my responsibility. I sat in the first row in his lectures on economic theory. North covered some very basic concepts, like the law of demand, marginal utility, and property rights. By the end of the week, I had a disturbing revelation: there was no comparable body of theory in sociology. Thus, I resolved to learn something about economics, hoping that it might contribute to my research.[9]

As chair of the Economics department, North had hired two young economists from the University of Chicago. Yoram Barzel had received his PhD there, and Steven Cheung was an assistant professor at Chicago. Both Barzel and Cheung were influenced by Coasean ideas about transaction costs and property rights. North, whose previous work had largely been cliometric, began applying these ideas to the long sweep of western European economic history – a path that ultimately led to his own Nobel Prize. I sat in on classes taught by Barzel and Cheung, and later became friends with Barzel.

In an era when American economics was besotted with high-level mathematics, a distinctive feature of these three Washington economists (one that they shared with Ronald Coase) was their commitment to verbal rather than mathematical argumentation.[10] Since I had a longstanding aversion to highly technical analysis (deriving, no doubt, from laziness – recall my reaction to the benzene rings), this influenced my own subsequent writing.[11]

Another experience piqued my interest in rational choice. During a visit to Ann Arbor, I had a drink with Jeff Paige, who was a member of the University of Michigan's Center for Research on Social Organization. This distinguished group of sociologists also included Charles Tilly, William Gamson and Meyer Zald. When I asked Paige what the group was talking about, he replied that they were all trying to come to grips with the implications of Mancur Olson's *The Logic of Collective Action* (1965). Olson directly challenged one of the principal tenets of Marxian and other structuralist theories – namely, that due to their mutual interests, members of the working class would invariably join in the class struggle. Olson argued, to the contrary, that the workers were much more likely to free ride than pursue their collective interests. This implied that most progressive collective action was problematic, to say the least. Since everyone in the Michigan group was committed to studying collective action, if not contributing to it, Olson's book presented them with a significant theoretical challenge.

Meantime, I discovered that I shared interests with several colleagues in my home department. Washington sociology had a long tradition of emphasizing both social psychology and statistical analysis; neither field had been well-represented during my time at Columbia.[12] It turned out that the rational choice theory I had been exposed to upstairs in the Economics department had much in common with sociological exchange theory, which Richard Emerson had helped to pioneer at the *micro* level. Emerson was a remarkable individual. Laid-back and judicious, he was famed in the department for having been part of the first American team to ascend Mt. Everest back in in 1963. Emerson's article on power-dependence relations was a revelation to me. At the same time, Herbert Costner was insistent about the importance of testing the empirical implications of social theories.

The first public airing of my work in rational choice macrosociology appeared in *The Microfoundations of Macrosociology* (Hechter 1983). Emanating from my graduate workshop in macrosociology, this volume caught the attention of Karl-Dieter Opp and Reinhard Wippler, two German sociologists who had been thinking along similar lines. Together with James Coleman, who had already engaged with rational choice a decade and a half earlier, we formed the nucleus of a small group of rational choice sociologists. Over time, membership in this group grew, and ultimately we helped establish the journal *Rationality and Society*, as well as eponymous sections of the American and International Sociological Associations (Research Committee #45).

Coleman, who had been a chemical engineering undergraduate at Purdue, was a student of Lazarsfeld's at Columbia in the 1950s.[13] A hefty Midwesterner whose looks – he resembled nothing so much as an American football lineman – belied his profound intellectual imagination and curiosity, Coleman had an outsize reputation in sociology. He was famous for the Coleman Report, which helped transform educational theory, reshaped national education policies, and influenced public and scholarly opinion about the role of schooling in the United States. His heart, however, lay in developing mathematical sociology. I suspect that this is what initially drew him to rational choice theory. Although Coleman's first paper in rational choice was published in 1966, it had been largely ignored by his fellow sociologists.

Our activity in the early 1980s revived Coleman's hope that rational choice theory could someday gain ground in sociology. Together with the eminent economist Gary Becker, who had produced provocative economic analyses of a number of subjects dear to sociologists' hearts, he started a rational choice track in the Chicago sociology department's PhD program. Since this track required all students to take Becker's mathematically oriented class on price theory, it mostly drew graduate students from Europe and Asia (American sociology students tend to be math averse). Most of these students returned to their homelands after receiving their degrees, and as a result they had virtually no impact on American sociology. Following Coleman's premature death in 1995, the Chicago department hastily abandoned its rational choice track. From that date onward, sociological rational choice had no institutional support anywhere in the United States. It is, however, on firmer footing in northern Europe and Japan.

One other experience, in particular, affected my view of these issues. As a Visiting Fellow at the Russell Sage Foundation in 1988–89, I was a peripheral member of a behavioral economics group led by the psychologist Richard Herrnstein.[14] Russell Sage had played the crucial role in funding the development of behavioral economics, and I was keen to see what these people were up to. At the time, I was at the University of Arizona, and had already been exposed there to work in Vernon Smith's laboratory on experimental economics. Whereas Smith was keen to validate rational choice theories in the lab, the Russell Sage group instead was on a mission to uncover the limits of these theories. Although I was impressed with the group's rigor, it seemed to me that the behavioral economists were mostly interested in explaining individual rather than social behavior. By the end of the year, I had the impression that behavioral economics had only limited utility for understanding macrosocial phenomena like norms, collective action and social order.

Let me say something about the title of this volume. To dispel any possible misunderstanding, it isn't meant to convey the idea that this volume presents the *definitive* take on rational choice sociology. In truth, there are several different versions, and my own, if anything, is probably more idiosyncratic than most.[15] Rather, the rationale for my title is quite different. Although the papers in this collection are substantively quite diverse – covering topics as varied as state formation in early modern Europe, social order in contemporary Japan and contemporary women's fertility choices – the thread uniting most of them is their common roots in rational choice logic.

So much for my long and winding journey to rational choice. The chapters that follow present some of the milestones along the way.

Before proceeding, however, I want to acknowledge the essential role played by several key collaborators. Among these, Debra Friedman was by far the most important. Debra's imagination, which far outstripped my own, inspired her to apply the theory to a variety of substantive areas, as well as to university administration. Before his subsequent rebirth as an evolutionary psychologist, Satoshi Kanazawa was also a significant collaborator. My recent work on collective action was jointly undertaken with Steven Pfaff. Two former students, Edgar Kiser and Christine Horne, have also been important collaborators. Kiser and I tried to shake up the field of comparative historical sociology, and succeeded in doing so, at least for a time (Kiser and Hechter 1991; 1998). And Horne and I put out a unique mechanisms-based reader in sociological theory (Hechter and Horne 2009).

Chapter 1 in this volume (with Satoshi Kanazawa) lays out my conception of sociological rational choice. The chapter points out that although the theory has made substantial inroads in other social science disciplines – especially in political science – it continued to face strong headwinds in sociology. Some of the sociologists' reservations about rational choice are simply due to misunderstandings. Others, however, derive from well-founded doubts about the empirical adequacy of rational choice explanations. The chapter reviews studies that provide empirical support for the theory in literatures on the family, gender and religion – three subfields often considered to be least amenable to rational choice analysis.

The remaining chapters in Part I apply rational choice logic to different substantive issues. Chapter 2 (with Debra Friedman and Satoshi Kanazawa) addresses a key question in demography: why do people in advanced industrial families continue to bear children when the pecuniary costs of doing so clearly outstrip the benefits? It proposes a non-standard utility assumption – uncertainty reduction – to propose a theory of the value of children. It contrasts this explanation with normative and standard rational choice explanations of shifts in fertility behavior, and explores the extent to which the theory is supported by the relevant empirical literature.

Chapter 3 (with Debra Friedman and Derek Kreager) explores one kind of intergenerational income transfer, and was inspired by a visit with my grandchildren. It offers a theory of the differential investment in grandchildren by their grandparents. As in the previous chapter, this theory relies on the assumption of uncertainty reduction to explain why grandparents in post-industrial societies find it rational to invest in at least one of their grandchildren. It advances several hypotheses about end-of-life uncertainty, as well as proportional and differential investments in grandchildren. It concludes by assessing how well the theory fits the empirical record.

Part II of the book addresses the problem of collective action. Chapters 4 and 5 attempt to explain reasons for the salience of different cultural identities that often undergird collective action in modern societies. Chapter 4 presents a structural theory of class and status-group formation. The theory suggests that differences in the solidarity of any kind of objectively defined group are independently determined by the stratification between these groups (hierarchy) and the interaction occurring within them (segmentation). These expectations are supported by an analysis of variation in the solidarity of seventeen American ethnic groups in 1970. Second, the theory suggests that the relative importance of class as against status group divisions in societies as a whole depends on the degree of hierarchy and segmentation of their respective cultural divisions of labor. This is supported by a brief consideration of class voting in Australian states.

Chapter 5 (with David Siroky) modifies and extends this analysis to offer a structural theory to account for the variable incidence of class as against ethnic conflict. These two types of conflict result from distinct principles of group solidarity. Since each individual is simultaneously a member of an ethnic group (or many such groups) and a particular class, these two principles vary in the degree to which they are mutually exclusive or cross-cutting. The degree of economic stratification between groups and economic segmentation within them shapes the relative salience of each principle of group solidarity in any society and is associated with a characteristic form of political mobilization. In places where between-group inequalities are high, and within-group inequalities low, ethnicity should be the dominant principle of group solidarity and serve as the primary basis of group conflict. By contrast, in countries where between-group inequalities are low, and within-group inequalities high, class is more likely to serve as the dominant principle of group solidarity, and conflicts along class lines are more likely. These conjectures are tested with data in over 100 countries on cross-cutting cleavages, ethnic war, and class conflict. The results are supportive of the theory, and provide evidence that group stratification and segmentation shape types of civil war.

Chapter 6 offers an explanation for the decline of class consciousness and the rise of cultural consciousness (or what has recently come to be known as 'identity politics'). It contends that class politics has receded in advanced capitalist societies during the last century while cultural politics has increased. Social and political institutions take precedence over occupational structure to explain this shift. Participation in solidary groups has consequences for the social bases of politics. The political salience of such groups is largely influenced by social institutions that are independent of the occupational structure. The first such institution is direct rule. Whereas indirect rule tends to promote class politics, direct rule favors cultural politics. Rapid expansion of direct rule since the 1960s has muted class politics and increased cultural politics. This relationship is not deterministic, however; other institutions, as well, can mitigate the effects of direct rule on the social bases of politics.

Chapters 7 (with Steven Pfaff and Patrick Underwood) and 8 (with Steven Pfaff and Katie Corcoran) are drawn from our recent study of mutiny in the Royal Navy during the Age of Sail (Pfaff and Hechter 2020). This is an unusual analysis of collective action, in that it avoids the abiding sin of selection bias by comparing ships that experienced mutiny with those that did not. Although mutinies in which crews seized control of their vessels were rare events, they occurred throughout the Age of Sail. To explain the occurrence of this form of high-risk collective action, Chapter 7 argues – in contrast to much of the current literature – that the principal cause of mutiny was shipboard grievances. Yet all grievances do not have the same effect on collective action. The structural grievances flowing from incumbency in a subordinate social position are different from the incidental grievances that incumbents have no expectation of suffering. Based on a case-control analysis of incidents of mutiny compared with controls drawn from a unique database of Royal Navy voyages from 1740 to 1820, in addition to a wealth of qualitative evidence, it turns out that mutiny was most likely to occur when structural grievances were combined with incidental ones. This finding has implications for understanding the causes of rebellion and the attainment of legitimate social order more generally.

Chapter 8 explores how insurgents who are engaged in high-risk collective action maintain solidarity when they are faced with increasing costs and dangers. It explains why insurgent solidarity varied among the ships participating in the mass mutiny that took place at the Nore, at the mouth of the Thames estuary, in 1797. The key problem that the organizers of the mutiny faced was maintaining solidarity in the face of government repression and inducements for crews to defect. Solidarity, measured as the duration of a ship's company's adherence to the mutiny, relied on techniques used by the mutiny leaders that increased dependence and imposed control over rank-and-file seamen. In particular, mutiny leaders monitored and sanctioned compliance and exploited informational asymmetries to persuade seamen to stand by the insurgency, even as prospects for its success faded.

The final section of the volume, Part III, focuses on the problem of social order. As Hobbes recognized long ago, the principal guarantor of social order is the state. To that end, Chapter 9 (with William Brustein) presents an endogenous explanation of the uneven pattern of 16th century state formation in Western Europe. It asserts that the geographical distribution of the first modern state structures was largely determined by preexisting regional differences of social and economic organization that can be traced back to the 12th century, if not earlier. Three distinct regional modes of production existed in 12th-century western Europe. These were the sedentary pastoral, petty commodity, and feudal modes of production. The chapter explains why the optimal preconditions for the initial formation of modern states were to be found only in regions that were dominated by feudalism.

States differ, however, in their capacity to provide social order. Chapter 10 (with Satoshi Kanazawa) tries to account for the relatively greater degree of social order in Japan as compared to its advanced Western counterparts. To do so, it distinguishes the attainment of local order in social groups from the global order in national societies. The chapter suggests that models of spontaneous, self-organizing order cannot account for global order. In contrast to the most popular normative explanation of Japanese social order, which holds that it derives from Confucian values, this chapter proposes a solidaristic theory built on rational choice premises (Hechter 1987). This theory views social order as a byproduct of dependence and control mechanisms within key social groups such as families, schools and firms. A wide range of comparative evidence reveals that the solidaristic theory provides a superior explanation of the high level of social order in Japan than the normative one.

Chapter 11 (with Sun-Ki Chai) develops a formal model of social order in line with the conclusions of the previous chapter. The problem of order is tackled by considering the crucial role that social groups play in mediating between individuals and the state. The chapter argues that existing individualistic models of spontaneous, self-organizing order cannot account for order in large societies, whereas state-centered models cannot explain the state's origins. The origin of the state can best be explained as the result of interactions both within and between highly solidary social groups. Accordingly, it discusses the bases of group solidarity, focusing on the costs of monitoring and sanctioning. It examines the relationship between the dependence of group members and solidarity, as well as the conditions for coercive solidarity. Highly solidary groups permit the creation and maintenance of social order by using their control institutions to enforce member compliance more efficiently than the state can. At the societal level of analysis, such groups can be considered as if they were individuals, or corporate actors. The attainment of group solidarity and the attainment of social order are isomorphic processes. Social order, therefore, is an outcome of multiple nested layers of group solidarity, with the optimal size and span of nested groups varying from society to society.

At least since the collaboration between Robert Axelrod and William Hamilton (1981; see also Wilson 1975), evolutionary biologists and social scientists came to appreciate that they share a common interest in understanding the evolution of cooperation. At the societal level, cooperation is just another way of referring to social order. Chapter 12 discusses the principal theories of social order in evolutionary biology and in the social sciences. As in previous chapters, it distinguishes between invisible-hand and institutional theories. Whereas invisible-hand theories are both elegant and parsimonious, they cannot account for the outcomes of collective action. To accomplish that task, what must be explained is the emergence of institutions.

At the dawn of this century, the editors of the *Annual Review of Sociology* asked for my thoughts on the agenda for sociology in the 21st century. This is what I wrote (Hechter 2000, pages 697–8):

> I have always taken the sociologist's principal task to be that of explaining variations in collective action, institutions, and formal organizations, among other social outcomes. I seek to learn why revolutions occur in some places and times rather than others, why certain societies have norms that foster development while the norms of others inhibit it, why some firms succeed when others fail. This is quite a different task from the explanation of cognition, perception, personality, and other individual-level outcomes. At first glance, explaining social outcomes would appear to be a straightforward mandate, but any such impression is misleading. Ultimately, social outcomes result from individuals' relations with one another and with aspects of their (nonsocial) environment. Although the environment – think of natural disasters like earthquakes, hurricanes, and droughts – often exerts a strong influence on social outcomes, it is the realm of social interaction that causes the greatest difficulties. While many of the necessary tools are now at hand for the analysis of existing social networks, institutions, and organizations, our capacity to predict their specific forms *ex ante* is modest. In part, social outcomes depend on the values, or motives, that lurk behind our actions (they also depend on other subjective elements, such as beliefs and attitudes toward risk). Among other things, the efficacy of the incentives that are used to channel our behavior – by lovers, friends, advertisers, social movements, employers, and states – depends wholly on people's values. These values vary widely – both within the same society (some people seem more interested in attaining wealth, while others just as doggedly pursue status) and cross-culturally (Americans now seem to be besotted by celebrity, whereas Evans-Pritchard tells us that the Nuer valued cattle and cattle products above all else). It stands to reason that the institutions and organizations that are devised by altruists will differ from those that are created by egoists.
>
> Yet our capacity to accurately assess these values is unimpressive…
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> Getting a better grip on individual values and other internal states would be an important contribution, but this information alone will not enable us to account for social outcomes. The institutions and organizations created by altruists may not be as resilient as those devised by egoists. The road to hell is paved with good intentions: at the end of the day, the factories built by that noble idealist Robert Owen failed, while automobile plants built by that wretched anti-Semite Henry Ford were a grand success. Much the same might be said of socialism's disappointing track record relative to capitalism's. These lessons alert us to the fact that many social outcomes, indeed perhaps most of them, do not emerge from the action of multiple individuals in any simple fashion. Whereas elections are determined by the mere aggregation of individual actions, this is an atypical case. There is no rule of 'one man, one vote' when it comes to the making of most social outcomes. Social interaction often leads to unintended consequences. Modern societies are repositories of great power disparities between individuals and between collective actors, like firms and trade unions, and these disparities affect social outcomes in a myriad of ways. Although much is known about the effect of existing power disparities on a variety of outcomes – after all, this was a field first plowed by the likes of Marx and Weber – a powerful general theory of the emergence of social structures continues to elude us.

Almost two decades have passed since these remarks were published. In the interim, psychologists and experimental economists have made great advances measuring internal states like preferences and values, and elucidating the determinants of individual decision-making. They have used a variety of experimental games and new techniques, like transcranial magnetic stimulation, to map out the causes and consequences of different motivations and behaviors (Glimcher and Fehr 2014: Part II). This has enabled previously neglected subjective elements to be deployed in new explanatory models. As a result, a number of social outcomes arising from the aggregation of individual behaviors have been illuminated (Thaler and Sunstein 2009; Thaler 2015; World Bank 2015). This research, deservedly, has gained a lot of attention and led to several recent Nobel Prizes. Explanations rooted in psychology find much favor in the media and the general public because they are personal and hence more relatable than social ones. We can all intrinsically *understand* psychological explanations. Not so social ones – they entail hard-to-grasp abstractions like norms, classes, networks, and social structures. Since social scientists themselves often disagree about the meaning of these terms, it's easy to appreciate why lay readers might be skeptical of them.

Despite this, if we want to understand the emergence of institutions and social order, psychology probably cannot serve as the best point of departure (see Chapter 12 in this volume). This leaves the terrain wide open for sociologists. Sociologists indeed can play a key role in producing such explanations, but only if they are willing to countenance insights afforded by rational choice theory.

Notes

* Thanks to Maureen Eger, Edgar Kiser, Steve Pfaff, and Judith Ward for their comments.

1. Preferences are *complete* when individuals can always state which of two alternatives they prefer, or whether they are indifferent to either. They are *transitive* when option A is preferred to B, and B to C, then A is preferred to C.
2. Emile Durkheim's (1982 [1895]) polemic is the most brazen statement of this position; however, it is belied by his more individualistic analysis in *Suicide* (Durkheim 1951 [1897]). Talcott Parsons's (1937) discussion of 'the Hobbesian dilemma' was another influential critique. Max Weber (1978 [1918–21]) is the lone classical sociological theorist who explicitly dissented from a wholesale rejection of methodological individualism. More recently, academic politics clearly has played a role in the demonization of rational choice. Starting in the 1950s a few economists, led by Gary Becker (1957; 1981), began making notable inroads in substantive areas that previously had been the exclusive preserve of sociologists. As the number of scholars applying economics to non-market phenomena grew, many sociologists began to complain about the advent of an 'economic imperialism' that, in their view, threatened the standing of their discipline. Similar concerns were raised by a much smaller cadre of political scientists in the so-called Perestroika movement (Monroe 2005).
3. Indeed, my father was the first to propose that information theory could be successfully applied to the problem of hormone action. This idea ultimately led to Nobel Prize-winning research on signal transduction (Rodbell 1994: 223).
4. For scientists, the psychological role of the belief in theories – even mistaken ones – may be akin to that played by placebos in clinical medicine. Actually, this story has been discussed in the social science literature. During a conference I attended on 'Knowledge and Institutional Change' at the University of Minnesota in 1987, the organizational psychologist Karl Weick presented this anecdote in his paper without providing any attribution. After the conference, I pointed out to him in private that he had failed to credit its source (for some further details, see https://andrewgelman.com/2012/04/23/any-old-map-will-do-meets-god-is-in-every-leaf-of-every-tree/, and Gelman and Basbøll 2014).
5. For reasons that I never understood, in these years there was a sharp distinction between the College (undergraduate) department and the graduate department. In my experience, undergraduate majors had no contact with the graduate department faculty. The first time I ever talked with Merton, he asked where I had gone to college. When I replied that I had attended Columbia College, he was visibly disappointed.
6. See especially his *On the Shoulders of Giants: A Shandian Postscript* (Merton 1965); a subsequent edition of this book features a forward by the notable Italian humanist Umberto Eco.
7. And apart from Merton's own writings, theory at Columbia meant *European* theory – Marx, Durkheim, Weber and so forth. When I presented my proposed reading list for the oral examinations to Merton, he remarked that it had no American writers (in particular, he mentioned the absence of anything by G.H. Mead). I replied that this neglect was intentional. Foolishly, I hadn't considered that the absence of any of Merton's own writings from the list might have rankled him. Nonetheless, he didn't argue with me.
8. This idea built on Karl Marx's (1992 [1897]: Vol. 1, Ch. 31) discussion of primitive accumulation in *Capital,* and was subsequently elaborated by dependency theorists like Hans Singer and Raul Prebisch.
9. In academia, foraging in another discipline is a very risky thing for an untenured professor to do. In every American university, one's fate is determined by individuals in one's own discipline, and usually little is to be gained by leaving these more-or-less friendly confines. Fortunately, I was free to explore outside the bounds of sociology because I had received tenure in my third year. Otherwise, the course of my career might have been quite different.
10. Although verbal theorists were a distinct minority in the rational choice world – game theorists held most of the high ground – there were several important ones, including Gordon Tullock, Brian Barry, Jon Elster, Russell Hardin and Elinor Ostrom. Not coincidentally, all but Ostrom had been associated at one time or another with the University of Chicago.

11. Richard Thaler claims that he once overheard his pal Daniel Kahneman describe him to a third party as 'lazy'. Although naturally he took umbrage at this portrayal, Kahneman assured Thaler that his laziness was one of his greatest strengths as a scholar. This is because it meant that Thaler would only work on questions intriguing enough to overcome his intrinsic tendency to avoid work (Thaler 2015: xvi). However comforting this characterization may be, I'm reluctant to regard my own laziness as a badge of honor.
12. By the time I entered the Columbia graduate program, Lazarsfeld had retired and departed for the University of Pittsburgh. The department had assigned his courses on statistical methods to an uninspired substitute. For its part, Columbia's outstanding programs in social psychology were located in Psychology and in Teacher's College; they had no influence on graduate students in the Sociology department.
13. As a graduate student, Coleman, too, had found Merton to be intimidating. Near the end of his life, however, he dedicated his *magnum opus*, *Foundations of Social Theory* (1990) to 'Robert K. Merton, my teacher.'
14. Herrnstein was incredibly sharp, and not at all what I would have expected of the future co-author of the notoriously controversial *The Bell Curve* (Herrnstein and Murray 1994). The other members of this group were George Lowenstein, Drazen Prelec, Ronald Heiner and Howard Rachlin.
15. See, for example, Kroneberg and Kalter (2012), Snijders et al. (2013) and Goldthorpe (2016).

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