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REVIEW ESSAYS

Who Needs a General Theory of Social Reality?

STEVE G. HOFFMAN
Department of Sociology
University at Buffalo, SUNY
sgh@buffalo.edu

Theoretical Principles of Sociology, Volume 1: Macrodynamics, by **Jonathan H. Turner.** New York, NY: Springer, 2010. 364pp. $169.00 cloth. ISBN: 9781441962270.

Theoretical Principles of Sociology, Volume 2: Microdynamics, by **Jonathan H. Turner.** New York, NY: Springer, 2010. 348pp. $169.00 cloth. ISBN: 9781441962249.

The headline story of the mock tabloid, *Science World Weekly*, recently announced, "Stephen Hawking Shocker: Supernovas Suggest Universe Has Small Cosmological Constant!" In this imaginary society, the publication of a general theory of social reality might land a feature article. Alas, this is not the world most of us live in. Here, academic scholarship circulates within small networks of specialists. It is good to keep this in mind when sociologists propose that we increase our impact by modeling the natural and physical sciences. We fight over the crumbs of public attention.

Academic scholarship can get especially niche at its highest levels of abstraction, as with a synthetic, formal, and universalistic theory of all social reality. As Hirsch, Michaels, and Friedman (1987) point out, synthetic theory is "the specialty of a minority of theorists. . . and at their best provide fodder for graduate courses and other grand theorists, but in practice are ignored by most sociologists." Whatever one thinks of this state of affairs, it is undoubtedly true. So who needs a general theory of social reality? Jonathan H. Turner believes we all do and has spent much of his career building it. He suggests that the main challenge is a shortsighted antipathy toward theoretical unification that has three main sources. First, Parsonsian functionalism's combination of conceptual mountains and explanatory failure tainted future efforts at "grand theory." Second, a "new age of specialization and middle-range theorizing" has advanced knowledge piecemeal, accumulating many timeless truths but providing no overall framework. Finally, there is the "smug cynicism" of the anti-positivists who gained a bullhorn during the cultural turn in social theory (the miscreants go nameless and are summarily dismissed). These factors have coalesced into an intellectual climate forbidding to explanatory theory with universalistic ambition. Turner warns that the cost is a fragmentation that erodes the discipline's place in the scientific pecking order. But fret not, for Turner has taken up the good fight.

The publication of the first two volumes of *Theoretical Principles of Sociology* offers an opportunity to pose a few questions related to the prospects of grand sociological theorizing. If we assume, for the sake of argument, that there has been and will continue to be a place for some grandness, what might such a theory look like? If a unified theory is to hold widespread sway (in full disclosure, I would not bet on one), it will require considerably more than just explanatory coverage. It will also require more than predictive reliability. It will require excited and energized followers and lots of them. Who will read, engage, and proselytize for a twenty-first century grand theory? I will speculate on this before moving to a secondary issue, which is whether or not Turner delivers the goods.

The Much Discussed But Greatly Exaggerated Death of Grand Theory

The term "grand theory" was always meant as a diss. At the apex of American general sociological theorizing, C. Wright Mills (1959) coined the pejorative phrase in his critique of Talcott Parsons. His main complaint, largely underappreciated for its reach, was that Parsons' universalism had borne a bloated conceptual edifice that lacked adequate tools for explaining historical change. Mills memorably pointed out that on the rare occasion when Parsons offers an empirical explanation, he shifts to a Marxist conceptual vocabulary. Structural functionalism lacked a convincing theory of power, and as such it had rendered sociology largely irrelevant to the post-war West's need to understand the heady events of its recent global history.

Over a few decades and after innumerable pile-on critiques, few graduate students were reading *The Social System* and countless undergraduate majors turned a caricature of structural functionalism into a convenient whipping post. This was a stunning rebuke, although it is not as if Parsons was ever the only game in town. He was just particularly, and peculiarly, prominent. Out of his systematic dismissal bloomed a highly prolific assortment of theoretical projects (see Camic and Gross 1998). The dominant trend in America has long been middle-range theorizing within increasingly specialized scholarly silos. Hovering above that trend have come several ambitious synthetic frameworks however, with the most influential on American sociology exhibiting a careful delineation of scope conditions. Consider for example, Pierre Bourdieu's unification of structural inequality with interpretive sociology, Randall Collins' merger of functionalism with conflict theory and the microsociology of emotions with macro history, or Anthony Giddens' application of his structuration theory to the globalization of risk.

At least two traditions, broadly construed, have vied to become broadly accepted and genuinely grand theories. First, a wide variety of neo-Marxist frameworks sought to adapt historical materialism to global capitalism, with the most successfully generative being world systems theory. Second, postmodernist theorists, despite their bluster to the contrary, have only rarely been able to resist totalizing categories for the "present moment."

Postmodern theorists' clever use of irony and pastiche, their abuse of quotation marks, their tendency to convert verbs into nouns, and their quirky punctuation do little to hide a universalistic ambition that minimizes difference across peoples, social structures, and history. Postmodern theorists of gender and queer identity are the partial exceptions here, although few offer the sweep of a grand theory.

While there is without doubt some knee-jerk pessimism toward grand theorizing and its defenders are asked to, well, defend, the reports of its death have been greatly exaggerated. On the one hand, no single theory, theorist, or even loose framework has been able to replicate the centrality that Parsons enjoyed (that said, his centrality was, by any measure, an historical anomaly). On the other hand, quite a few theorists have proposed synthetic frameworks (see Ritzer 1991). A few of these have even been quite grand in the Millsian sense (Luhmann 1995, Wallace 1983). There has been, and will likely continue to be, some space reserved for synthesizers. If we can assume that general theory has and will continue to persist, what might a grand one need to do to get some lift?

For starters, the theory will need more than just epistemic plausibility (it is hard to imagine a more empirically implausible work than *Simulation and Simulacra*, yet Baudrillard managed to amass influence within our discipline). For a grand theory to become grand in the institutional sense, it must become more than just "fodder for graduate courses and other grand theorists." It will need followers, lots and lots of followers. Our grand theory would likely get a boost if a prominent Western European male with significant high profile training in metaphysics wrote it, or perhaps someone with Goffman's playful wit but a bolder vision for how to move between levels of analysis.

Bourdieu has come the closest to a universally adored theorist of the last few decades, and the diffusion of his work provides a helpful model. Bourdieu was able to enroll a large number of smart, productive, and highly capable American scholars from high prestige institutions to spread his ideas. Next, he very self-consciously crafted his own reception on American soil (Bourdieu and Wacquant 1992). He managed this just when the discipline was hungry for a broad synthesis that neither reduced social practice to mechanistic response nor assumed individuals possess boundless creativity. In addition to this social and cultural capital, Bourdieu's widespread appeal owes to several stylistic and substantive issues, such as an impressive facility for shifting between dense theoretical abstraction and revealing empirical specificity (a quality that only Goffman can rival), an impatience with overly-formalized conceptual definition (which has the drawback of vagueness but the advantage of enabling followers to be creative), a decided preference for relational rather than context-free concepts (which helps keep the postmod's and interpretivist's attention), and an anthropologist's skepticism toward the reification of empirical representations of everyday life.

While any one of these factors is neither sufficient nor necessary, in concert they offer a reasonable prediction for what a grand theory for the twenty-first century would need to accomplish. My main point is that for a theory to become truly grand, in the sense of a commonly-accepted framework that seeks to capture all or at least most of social reality, it will need to do more than just explain. It will need to be loved. A loose recipe for this trick, then, might look something like the following: exhibit the epistemological subtlety of *Distinction*, the insight of *Presentation of Self in Everyday Life*, approximate the networks forged by Bourdieu and the Center for European Sociology, possess the broad sweep of, say, Shmuel Eisenstadt, be unafraid to move across units of analysis like Randall Collins (or Jonathan Turner), surprise and delight readers with both theoretically portability and empirical precision—and in doing all this capture the imagination of influential and energized followers capable of elaborating concepts across historical time and space. No small trick.

The Banality of Invariant Principles

Do Turner's three volumes deliver the goods? The odds are greatly stacked against him. At the least, his effort deserves to be pondered over, debated, and ultimately, synthesized by the grand theorists of our future. However, Turner seems unwilling to do much of the hard work it would take to get readers excited about his project. The central problem is that these volumes overcharge the reader for what amounts to a modest payoff.

Turner seeks a single framework that can seamlessly pull together three fundamental levels of human social organization—macro, meso, and micro. Inspired by Herbert Spencer, he offers invariant principles covering the fundamental forces that govern all social reality. Volume One focuses on the how macro societal structures are the product of five such forces: population (i.e., size, rate of growth, compositional patterns, etc.), production, distribution (i.e., inter- and intrasocietal systems for exchanging commodities and services), regulation (i.e., power, coordination, and control), and reproduction (i.e., the replenishing of members and culture). Volume Two considers ecology/demography, status, roles, culture, motives, and emotions as the forces of the micro realm. Volume Three has yet to be published at the time of this writing. Unlike One and Two, the last volume will not introduce more fundamental forces because the dynamics of the meso-level (i.e., organizations) are "derivative of pressures emanating from macro and micro forces."

Each chapter culminates with a set of "elementary principles" for how each force operates. These include a long list of subfactors and recursive reference to the previously posed principles. These formulations rely on vague causal descriptors (additive, multiplicative, positive, negative) for the directionality of relationships, but no specific weighting of measures. Volume One contributes twenty-three elementary principles of macrodynamics and Volume Two includes another twenty-nine. Despite Turner's desire for parsimony, the pile-on of concepts and terminology bogs down the explanatory potential of his framework.

Rarely are we presented empirical justification for the theoretical assertions (Volume One is particularly abstract while Volume Two offers mostly personal anecdotes), and demonstration of the explanatory utility of the framework is nowhere to be found.

The claim to universalism is grounded in the level of abstraction of the principles (Turner claims the "operative dynamics" of his forces can be found in any society in any historical era and welcomes the empirical refinements of others) and two empirical assumptions. The first assumption is that a *longue durée* trend toward greater structural complexity began with a "Big Bang" in the evolution of human collectivities—when human groupings stopped being nomadic and settled into permanent communities. This created the population pressures that have since propelled new socio-cultural formations that either deal effectively with problems of scale or perish. The second empirical assumption is that the boundary conditions of human adaptation to the physical and social environment is in large part, hardwired by our evolution from early hominids and the disproportionate time our species evolved within hunting and gathering groupings. The social patterns of great apes are then referenced as proof positive for several base-level behavioral tendencies of human beings, such as a fundamental desire to achieve a sense of justice, a propensity for ephemeral small group participation, and a highly evolved sense of emotional arousal (his anthropology of ape colonies is very thin, although I found the discussion of emotion in human interaction fascinating). Armed with these universalizing assumptions, Turner never bothers to gather the kind of comparative, historical, or ethnographic data one might hope for when a sociologist makes claims across time, space, and place.

Overall, the first two volumes only deliver a fraction of the attributes that will be required of a twenty-first century grand theory. On the plus side, the work is sufficiently broad in scope and the analytic focus moves across units and levels of analysis. The goal is theoretical explanation amenable to empirical refinement. The highlight, for me, was the discussion of the embeddedness of encounters within Volume Two. Here Turner's synthetic scheme does some very nice analytic work. Also in Volume Two, Turner attempts a useful rehabilitation of role theory that, although not totally convincing, was stimulating and deserves serious consideration among theorists of interaction.

On the negative side, the principles are a chore to read through. The volumes plow forward in a tightly organized but ponderous way. It does not help that grammatical and editing errors distract every few pages, sometimes multiple times in a single paragraph, which is both embarrassing and, more worrisome, raises concerns about the editing standards of financially beleaguered book publishers. The principles are long, repetitive, sometimes sloppy, too self-referential, and riddled with problems of tautology. Many of the central concepts are grossly overtaxed, particularly in Volume One, where "selection pressures" and "logistical loads" refer to practically any macro-level dilemma and all manner of institutional adaptation. Turner's elementary principles typically refer to obvious or quite banal causal relationships. Mills' formula for Parsons works here as well: 50 percent verbiage, 40 percent well-known textbook sociology, and 10 percent is left open to the empirical investigation of someone else. The work is carried forward in such spare and quite frankly, boring prose, that only the most invested of readers are likely to show enough commitment to assess his substantive contributions.

Analytic Induction and Grand Theorizing

Armchair theorizing often suffers for a lack of analytic induction, especially where historical and empirical examples can push theory toward revealing and surprising insight. This however, requires a constant tacking back and forth between empirical particularity and theoretical generality, not some lazy utopian division of labor between empiricists and theorists. One could, for example, usefully deploy the notion of selection pressures to explain how internet technology developed in response to the threat of nuclear annihilation. However, pairing this concept with "secondary logistical loads" does very little to explain how online trading, Facebook, the overthrow of several Arab dictatorships, or an app society has emerged from this adaptation. The banality of invariant principles provides precious little purchase on the peculiar twists and turns of lived history. The next generation of grand theorists will need to be very good empiricists indeed.

Turner gets most fired up when he discusses the dire need for a grand theory that can supplant the "anti-scientific" forces aligned against it. Unfortunately, he never seriously engages the cultural turn in sociological theorizing, opting instead for broadsword dismissal of what Max Weber so astutely referred to as the "overreaching tendency of a formal-juristic outlook" (1949: 82). Bourdieu, likewise, formulated significant and important critiques of positivism that are far from "anti-scientific." No social scientist worth their credential would dispute the idea that a universalistic science is *one* way to explain social reality. Rightly or wrongly, and I think Turner is simply wrong beyond an extreme fringe, this dismissal puts him at odds with most of the interpretive wing of our discipline. This is no way to get us excited about a moment of grandeur.

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Why Some Nations Succeed

TOBY E. HUFF
Harvard University
thuff@fas.harvard.edu

Why Nations Fail: The Origins of Power, Prosperity, and Poverty, by **Daron Acemoglu** and **James A. Robinson.** London, UK: Profile Books, 2012. 529pp. $30.00 cloth. ISBN: 9780307 719218.

The challenge of this book is to explain why some nations are more prosperous than others and why the pattern of the poorest nations has been unchanged for hundreds of years. The authors consider alternative explanations based on geography, culture, and simple ignorance, but they believe the answer lies in institutional arrangements. They insist that it is the existence of "extractive institutions" which benefit a narrow elite that cause nations to fail. They see a "virtuous cycle" whereby "inclusive institutions" supporting economic growth and prosperity feed off each other, yet there are many more cases of the "vicious cycle" in which extractive institutions remain, supporting only the interests of a narrow self-serving elite. And since Daron Acemoglu and James Robinson (hereafter A&R) suggest that the countries at the bottom of the global income scale have been the same for the last 150 years, the virtuous cycle has not taken over. So it appears that we have no general remedy for escaping the hold of self-serving extractive institutions once they are in place.

There is merit in the suggestion that pluralism and the wider participation of "the masses" has a salutary effect on economic development. Beyond that, the authors purport to look at the historical background that led England and the Western world to economic success, though their analysis is surprisingly truncated. In order to give readers a dramatic contrast of the effects of institutions on economic behavior, the authors compare Nogales, Arizona where incomes average $30,000 with Nogales, Mexico where the average income is one third of the other Nogales. It is easy to agree, superficially at least, that a large part of the differences here are the result of institutional arrangements that are traceable to founding commitments of the two countries. In the United States, the founding fathers created a constitutional democracy and supportive economic institutions, whereas in Mexico, the foundations for constitutional democracy and open competition were not laid. But having ruled out any influence of "culture," the authors fail to note that all the English colonies up and down the east coast of the United States drew on the history of English (and Continental) legal theory whereby charters were granted to groups of citizens which then led to written constitutions; conversely, none of the founders of the Latin South American states in the nineteenth century attempted to (or succeeded in establishing) constitutional democracies on the basis of such principles. Is this a culture difference?

Where others might suggest India (the world's largest democracy) as the test of the efficacy of the British "cultural legacy," these authors claim Sierra Leone and Nigeria as failed benefactors of that heritage. This gives the reader a sense of how much the book is a product of spin and speculation.

At the heart of the difficulty is the authors' narrow conception of institutions. It leads them to focus on minor differences while overlooking major historical inflection points that had vast impacts on political, economic, and intellectual development that were specifically European. Whereas economists tend to look at the small structures of *rule* formation as *institutions*, sociologists and other social scientists view institutions as large structures that solve major societal problems, such as the family, the legal system, the polity, the military, or perhaps the stock market. From an economist's point of view, the narrower focus on *rules* and their changes makes perfect sense: any small change in the rules of economic competition will result in new distributions of money, wealth, and poverty.

Furthermore, this fits well with the preference of economists for *methodological individualism*: according to this view, whenever the economist wants to know what will happen with a rule change, they have only to place themselves in the position of the actor and conjure up the most "rational," that is utility-enhancing, outcome the individual would "naturally" chose. This works on that level in some contexts, but when the structures of a society get to be very large and less controllable by individual decisions, the outcome of mass profit-seeking may not be what individuals wanted or foresaw, as when markets or financial systems collapse. In addition, economists have been forced to recognize inexplicable "preferences" so that strict utility may not apply. In any case, once created, large institutional structures may have perdurable effects that far outlast the short-term business or investment cycles that concern economists. Furthermore, legists and other political actors may not be thinking primarily about profit-seeking behavior when they design the institutions that govern human interaction, including economic action.

Nevertheless, the question is, just *how* and *when* did more inclusive political and economic *institutions* arise in England, Europe more broadly, and the United States? The analysis that A&R provide is surprisingly speculative, especially regarding legal history and the origins of Western political and economic structures. Nor do they draw much on previous research by economists on the emergence of *efficient institutions*, the evidence of their existence and early origins.[1] Because of this narrow focus on rule changes (institutions), economists often fail

[1] See Jan Luiten van Zanden, *The Long Road to the Industrial Revolution.* Leiden, NLD: Brill, 2009.

to notice the big changes. That is what A&R have done. It is perfectly true that ''institutions differ and play a critical role in explaining economic growth throughout the ages'' (p. 124). Likewise, it is perfectly logical to say that ''inclusive institutions (as opposed to ''extractive institutions'') allow and encourage participation by the great masses of people in economic activities that make the best use of their talent and skills'' (p. 74).

But having said that, we have to ask where these institutions, especially the inclusive ones, come from. In Chapter Three the authors use the example of North versus South Korea in the twentieth century. That is an odd example to use if we are trying to understand the longer term historical processes that led to the emergence of inclusive institutions in the first place, especially as it overlooks the radical remaking of South Korean's political and legal institutions after the two wars (W.W. II and Korean) under the influence of Western assumptions. Using that twentieth century context puts the cart before the horse. There is a pre-history of some significance before South Korea was transformed into a modern state, and one would need to explore that history in order to understand how and why South Korea emerged with progressive institutions while North Korea did not.

Suddenly in this same discussion, the authors tell us, ''Inclusive economic institutions require secure property rights and economic opportunities not just for the elite but for a broad cross-section of society'' (p. 75). The next paragraph is followed by a further qualification: ''Secure property rights, the law, public services, and the freedom to contract and exchange all rely on the state, the institution with the coercive capacity to impose order, prevent theft and fraud, and enforce contracts between private parties'' (pp.75–76). All of this seems right; but instead of developing their argument along historical lines that start with the actual historical processes whereby these new inclusive institutions evolved, the authors jump here and there giving the impression that these liberating pluralistic and inclusive institutions could be conjured out of whole cloth in any time or place. If that were true, then surely Asia and other parts of the world would not have lagged economically behind Europe for so long since the industrial revolution.

This procedure of jumping here and there is rather glaring in the case of Egypt, for Egyptians threw off the yoke of British colonialism in 1919, but reveled in the authoritarian power of Gamal Abdel Nasser after his military coup in 1952, followed by the even more tyrannical Hosni Mubarak from 1981 to 2011. Egyptians may well know that their country has been dominated by corrupt leaders and extractive institutions, and even believe as the authors do, that ''the roots'' of poverty and underdevelopment are ''political.'' That is easy to say but not so easy to change.

Unfortunately, both the conceptual and historical analysis of institutional structures provided by A&R are inadequate and highly misleading. There is, for example, a very large literature on the emergence of the whole range of modern legal rights, including ''secure property rights'' and many others that powered the Western world to scientific, technological, and economic success. Instead of focusing on that literature, A&R take a very different route.

In Chapter Four they focus on ''small differences'' and discuss some consequences of the Bubonic plague of 1347. The authors believe that this cataclysmic event produced significant changes in wages and labor relations, apparently helpful in the long run. The small differences that the authors consider are between ''East'' and ''West'' in Europe, England being the West where better outcomes were achieved than in the ''East,'' the area east of Western Europe.

But the big question is, where and when did the larger structures of law and secure property rights come from that are essential for the authors' thesis? In Chapter Eleven the authors focus on ''the virtuous circle'' and we see how out of kilter their account is. It is surely true that the *rule of law* is central to a well-functioning economy as well as polity, as Max Weber insisted. Without law there can be no political stability and hence no economic stability. But A&R seem to imply that the rule of law occurred only suddenly after the Glorious Revolution of 1688 and after the Black Act in 1723. Yet in their account of the legal proceedings that led to a new day for the rule of law, they acknowledge that *jury trials* (a medieval invention) took place, which hardly could have happened had not the rule of law already been in place. A&R have in this manner entirely overlooked the uniqueness of Western legal and political development.

By focusing on the rather minor shifts in labor relations after the Bubonic plague, the authors miss the legal revolution of the twelfth and thirteenth centuries in Europe that had lasting impacts all the way to the present. That revolution created a large stock of *new legal entities* and a new *bundle of rights*. It established a conception of *legally autonomous spheres* of political and economic action that is uniquely Western. The medieval era invented urban charters of legal autonomy and granted that status to a broad range of *corporate* groups that are the historical precursors of the charters (and later constitutions) used by the American colonists. These new corporate devices—legally autonomous entities (*universitates*)—could be treated as self-willed agents capable of *making their own rules and regulations*, and hence of self-governing. This included the idea of making decisions according to the principle of "what touches all should be considered and decided by all," or by the greater and sounder part, which is part of our conception of representative democracy and "due process of law."[2] These new rights also included the right to sue and be sued, as well as to buy and sell property. Among the legally autonomous entities possessed of the new rights were collective actors such as cities and towns, charitable organizations, worker and merchant guilds, town councils, universities and parliaments. There is no hint of the revolutionary nature of these new corporate entities and legal devices nor of their economic and political significance for Western development in *Why Nations Fail*.

Moreover, it has been known for some time that legally autonomous entities did not exist in Islamic law (or Chinese law for that matter).[3] Recently this deficit in Islamic law has been spelled out in considerable detail.[4] For economists the point is that partnerships in Islamic law have rather short lives because, if one of the partners dies, then the partnership automatically dissolves. At the same time, economic actors (Muslims in the Middle East, merchants in China) were hardly in a position to invent and impose these historically new institutions in the medieval or early modern period. In the case of the Islamic Middle East, these handicaps persisted all the way to the late nineteenth century when Islamic law was almost wholly replaced by European-designed international legal structures. Today China is even more problematic.

In a word, there were rather large legal deficits in all non-European societies right up to modern times. It is a chimera to imagine that economic intervention, the empowerment of the dispossessed, and the eradication of poverty is no further away than a tweak of institutional arrangements. In their sympathy for economic actors in other parts of the world, A&R have inadvertently lifted them out of the very social, legal, and political conditions that have in the past prevented their progress. Pre-Columbian Peru, according to A&R, would be as good a candidate for developing modern

2 Among others see Harold Berman, *Law and Revolution: The Formation of the Western Legal Tradition* (Cambridge, MA: Harvard University Press, 1983); Kenneth Pennington, "Due Process, Community, and the Prince in the Evolution of the *Ordo iudiciarius*," *Rivista internazionale di diritto Commune* 9 (1998): 9-47; R.H Helmholtz, *The Spirit of Classical Canon Law* (Athens, GA: University of Georgia Press, 1996); and James A. Brundage, *The Medieval Origins of the Legal Profession* (Chicago: IL: University of Chicago, 2008).

3 Toby Huff, *The Rise of Early Modern Science: Islam, China and the West* (Cambridge, UK: Cambridge University Press, 1993; 2nd ed 2003), Chapters Four and Six.

4 Timur Kuran, *The Long Divergence*: *How Islamic Law Held Back the Middle East* (Princeton, NJ: Princeton University Press, 2010), though Kuran's thesis was published long ago and the deficit was pointed out in Huff, *The Rise of Early Modern Science* (pp. 134–41). Cf. T. Kuran, "The Absence of the Corporation in Islamic Law: Origins and Persistence," *The American Journal of Comparative Law* 53, #4 (2005): 785–834; and idem. "Institutional Causes of Economic Underdevelopment in the Middle East," in *Institutional Change and Economic Behavior*, ed. János Kornai László Mátyás, and Gérard Roland (New York, NY: Palgrave Macmillan Press, 2008), pp.64–76.

political and legal institutions as another place on the planet (p. 433).

In the end, the authors want it all ways: there are no cultural inhibitions, all peoples are free to create whatever institutions they need and want. Institutions hold the key and the authors claim to have an explanation of how the good institutions emerged, and hence how things can be changed for the better. But, ''you can't engineer prosperity'' (p. 446ff) and so they proceed to criticize outside agencies for attempting to intervene in developing countries. Attempts to change institutions fail, they now say, ''because they do not take place in the context of an explanation of why bad policies and institutions are there in the first place. . .'' (p. 447). This appears to claim that institutional change may encounter some underlying *cultural* resistance—but this is what the authors have labored so hard to eliminate from their world.

This is an interesting book, but it relies far too much on anecdotes detached from legal and institutional histories, and from an inadequate grasp of European, Islamic, and Asian legal history to give readers the insight and analysis that is needed.

Are We Consuming Beyond Our Means? The Debate Over, and Resistance To, Excessive Consumption and Debt

George Ritzer
University of Maryland
gritzer@umd.edu

Beyond Our Means: Why America Spends While the World Saves, by **Sheldon Garon.** Princeton, NJ: Princeton University Press, 2012. 475pp. $29.95 cloth. ISBN: 9780691135991.

Buying Power: A History of Consumer Activism in America, by **Lawrence B. Glickman.** Chicago, IL: University of Chicago Press, 2009. 403pp. $45.00 cloth. ISBN: 9780226298655.

Against Thrift: Why Consumer Culture is Good for the Economy, the Environment, and Your Soul, by **James Livingston.** New York, NY: Basic Books, 2011. $27.50 cloth. 257pp. ISBN: 97804650 21864.

These three books on consumption would seem to indicate far greater interest in the topic by American historians than American sociologists who, in spite of the creation in 2012 of an American Sociological Association section on Consumers and Consumption, continue to focus far more on issues of production than consumption. Yet history seems to have much the same problem as sociology, in the sense of not according consumption as much attention and significance as it deserves (see, for example, Lawrence Glickman, p. 155). Nonetheless, these books, often with diametrically opposed themes, demonstrate the great importance of consumption not only to various historical developments but also to the contemporary world, especially to the Great Recession and its ongoing after effects. In the terms of these books, the latter could be interpreted as a result, at least in part, of too little consumption (James Livingston), too much consumption and debt (Sheldon Garon), or insufficient or misdirected consumer activism (Glickman).

James Livingston, as an historian, feels empowered to analyze the economy because ''the economists blew it, and we all know they did'' (p. xvi). A paradigm shift is needed and he argues that it cannot come from economics, it must come from knowledge of history and the hybrid fields that he represents and draws upon in his book. He cuts across all of these fields and their attendant literatures to write a scathing critique of Americans' obsession with saving and investment *and* an impassioned plea for Americans to consume more (shockingly, he even supports advertising by viewing it as utopian, liberating, and heavily influenced by the counterculture).

He offers a striking perspective that seems to contradict the dominant way of thinking about consumption for more than a century (Veblen, Riesman, Marcuse, as well as in Garon) and everything we ever learned and believed about it. Livingston also contradicts our belief in the redeeming powers of production—he prefers to call it the ''pathos of production''—or ''an almost Puritan belief in the redeeming value of producing as against consuming, saving as against spending, working as against whatever comes after'' (p. 166). He traces the roots of both the Great Recession and the Great Depression to an excessive focus on production, savings, surplus profits and capital. This resulted in a ''global savings glut'' that led not to increased investment (in any case, net investment has been declining in the United States since 1919 and in his view does not translate into economic growth), but into the speculative investments that caused the economic bubble at the root of those economic crises and many other problems in American society. To deal with this problem, Livingston argues for a redistribution of the wealth away from the rich and profits and toward consumers in general so that everyone can afford to consume more. Decisions on resource allocation and investment should be ''socialized'' and shifted away from wealthy investors (and their focus on exchange values) and toward consumers and their preference for use values. This would lead to more balanced growth rather than speculative bubbles. He also rejects the conventional wisdom about tax cuts and other incentives for the rich in order to get them to invest more. The resulting funds would not go to increased investment in now unneeded expansion of industrial capacity, but rather to a still further bloating of our surplus capital and therefore to more speculation and economic bubbles. Such changes require enormous psychological and moral changes away from feeling guilty about consumption and deifying abstemiousness in order to increase the amount of money available for production. In Livingston's view, our ''soul'' is not to be found in work, but rather the ''soulcraft'' associated with leisure time and consumption. He is in the end drawn to the work of Battaille and his case for ''excess, sacrifice, expenditure, and profitless consumption'' (p. 200) and against pointless production. In any case, he argues that consumption ''beats working.''

There is much that is appealing in this book, but I have several reservations. First, while it draws on many academic sources, it is a trade book with the result that much of that scholarly work is dealt with superficially.

Second, and more importantly, it (and indeed all the volumes reviewed here) buys into a modern binary separating production and consumption and comes down on the side of production. However, as I have tried to show in a number of recent works (Ritzer and Jurgenson 2010; Ritzer, Dean, and Jurgenson 2012), it is ''prosumption'' that is the more basic and primordial process and concept. As a result, the solution to problems posed for individuals by production are not solved by switching to consumption. Rather, the solution lies in the fact that all production and consumption involves consumption and therefore what is needed is a process of prosumption that better integrates and balances the production and consumption ends of the prosumption continuum.

Third, and most importantly, this book is badly hurt by its near-exclusive focus on the United States and its lack of attention to, and familiarity with, globalization. While Livingston *may* be right that investment has not made much difference in the United States, it has certainly made a huge difference in other parts of the world, especially China which is growing rich as a result of its multifaceted investments. Indeed, it could be argued that it is growing rich because its share of GDP attributable to consumption is less than half that of the United States. However, it is true that the Chinese now seem to be focusing more attention on consumption because they are a bit more concerned about enriching peoples' lives and a bit less about having the nation grow wealthier.

The lack of attention to globalization (Garon is very good on this) creates another problem for Livingston. The gains from a huge increase in consumption are not going to go mainly to the United States, but rather to places like China where the products desired by American consumers are likely to be manufactured. Livingston's program would lead to more "stuff" for many Americans (many of whom do not need much of it), but *not* to the enrichment of the U.S. economy as a whole. Consumption in and of itself cannot make the United States wealthier. In fact, by distracting attention from production (really prosumption), it could make the United States poorer.

Finally, I am not at all convinced that consumption is the satisfying process that Livingston makes it out to be. If I was going to make such a case for consumption, it would be in the context of the prosumption that also accords a key role to production. In fact, in a discussion of the work of food scholar Michael Pollan, Livingston makes that point. He quotes Pollan as saying that "I realize I've gotten at least as much pleasure from working together to create these meals as I have from eating them" (p. 184). If I was to make a case for a revolutionary change, I would not put my money on a one-sided process of consumption, but rather on a more balanced process of prosumption.

In *Beyond Our Means: Why America Spends While the World Saves*, Sheldon Garon takes a position diametrically opposed to Livingston's main argument. To Garon, the big problem is the excessive debt incurred by Americans as a result of excessive consumption. While Livingston urges Americans to consume and to spend more, Garon argues that Americans need to consume less and save more. Many other societies have had a better balance of savings and consumption, but the United States has been greatly imbalanced in the direction of consumption.

In making this argument, Garon challenges the now-accepted grand narrative that the late twentieth and early twenty-first centuries marked a major shift in American society in general, and the American economy in particular, from production to consumption (that false binary again). "Historians commonly write about the twentieth century in terms of the rise of 'the consumer' and 'consumer society.' But the experiences of the two world wars should give us pause" (p. 170). Those experiences involved a reduction in consumption and a belief that extravagance and waste, especially in wartime, were problematic, even treasonous. In fact, the grand narrative in many other societies (e.g., Belgium, France, Great Britain, Germany, Japan, South Korea, and China) was increasing institutional emphasis on savings and thrift.

In this, as in many other things, America was exceptional. In this case, American exceptionalism involved much less of an emphasis on savings (especially small savings) and thrift, less effort to protect citizens from overindebtedness, and most importantly, much less institutional support for those behaviors. That is, in comparison to many other countries, the United States did not do as much to put into place, or emphasize, such institutions as savings banks, postal savings systems and school savings programs. Whatever national efforts were made to increase savings were much less likely to be successful without this institutional support. America's comparatively meager efforts of this type were also more likely to be countered by campaigns orchestrated by the business community in favor of maintaining, if not increasing, consumption. The pattern was somewhat different during WWII when the United States succeeded in saving more, but at the same time Americans also consumed more than other countries. After WWII, consumption in the United States began to accelerate greatly while there was less and less emphasis on, and institutional support for, savings. In the decades after WWII, "it became harder and harder to save, for that would mean resisting messages to borrow and spend that seemingly came from everywhere: from advertisers, bankers, business writers, economists, national leaders, and of course neighbors. By the turn of the twenty-first century, the decision to live beyond one's means appeared not reckless, but the mark of a good American" (p. 355).

All of this is related to the high level of consumption, low level of savings and high level of debt associated with the Great Recession that began in the United States in late 2007. Of great importance in this context was the boom in the consumption of housing and the huge amount of debt incurred in order to afford those houses, as well as the home equity loans taken out on those homes in order to have access to funds that would allow for other types of consumption.

While Livingston wants Americans to consume more and save less, Garon takes the opposite position of the need to save more and consume less. Who's right? My sympathies lie with Garon since it seems clear that Americans have created an unsustainable economy based on "hyperconsumption" and "hyperdebt." Garon also offers a much more serious comparative-historical analysis while Livington's work is more of a popular polemic.

There is one possible point of agreement between Livingston and Garon. That is, both might well be in favor of a Keynesian position in the current economic predicament and argue that more spending, especially by the government, is needed rather than more savings and greater austerity.

Lawrence B. Glickman is the author of *Buying Power: A History of Consumer Activism in America*. Most of the activism analyzed in this book has been in opposition to consumption in general as well as to specific forms of consumption. Its focus then, is much more in tune with the arguments being made by Garon than those of Livingston. Glickman accepts the idea that there are both general and specific problems associated with consumption that need to be addressed by activists.

While Livingston offers a grand narrative of increasing consumption, and Garon offers one of increasing thrift (except in the United States), Glickman offers a grand narrative of continuing American activism as it relates to consumption. He takes on those who see a decline in consumer activism and more importantly those who see discontinuity in that history and focus on its high points in 1900–1920, the 1930s and the 1960s. In contrast, Glickman argues that "a great deal happened in the "off" decades of the twentieth century, to say nothing of the eighteenth and nineteenth centuries" (p. 259). Glickman also details more recent consumer activism associated, for example, with the many earlier failed efforts to create what is now called the federal Consumer Financial Protection Bureau. He concludes: "Throughout American history, consumer activism has waxed and waned but never disappeared" (p. 305).

Glickman sees great hope in consumer activism in light of assertions of the weakening of American citizenship, blamed at least in part, on increasing commercialization. He suggests an alternate way of looking at the relationship between commerce and citizenship: "The fact that so many Americans are not only ardent consumers but avid consumer activists... suggests that they see consumption not only as a private pleasure but also as a public good. At a time when cynicism about the political process is high—not least because *it* has become increasingly commercial—the enduring appeal of consumer activism is that it promises citizens, in their capacity as shoppers, a kind of power and responsibility that seems largely unavailable through conventional politics" (p. 310).

Overall, these are three serious books on consumption that challenge a variety of conventional views on the topic (and each other). In that, they should serve as useful models for sociologists interested in consumption and related topics.

References

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