

REVIEW ESSAYS

Nations, Empires, and Wars

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Waves of War is destined to be influential—and controversial. This book examines nationalism, ethnic strife, and war. These topics are not only prominent in the headlines but also of interest to comparative historical sociology, political sociology, and macrosociology more generally. While the topics are familiar, this is a challenging book. Andreas Wimmer presents a novel theory, creates new and powerful databases, and pursues demanding quantitative analyses spanning several centuries and the entire globe. He reframes several important debates, and the reader must understand foundational concepts in novel ways and come to terms with complex and demanding empirical analyses. For those persuaded by Wimmer's arguments and evidence, the lessons for sociology and sibling disciplines are valuable and far-reaching. Even for those who disagree—especially for those who disagree—coming to terms with *Waves of War* will be important because this book will be setting the agenda and framing debates for years to come.

Wimmer asserts that his understanding of nationalism puts legitimacy on center stage: "The idea of the nation as an extended family of political loyalty and shared identity provided the ideological framework that reflected and justified this new compact. It meant that elites and masses should identify with each other and that rulers and ruled should hail from the same people" (p. 4). This social compact "made the first nation-states . . . militarily and politically more powerful than dynastic kingdoms or land-based empires. . . ." (p. 4). Through conquest, absorption, emulation, and diffusion, states around the world have followed suit. Nationalism has been, is, and will be (for the foreseeable future) the most effective and pervasive basis of political legitimacy.

Waves of War: Nationalism, State Formation, and Ethnic Exclusion in the Modern World, by **Andreas Wimmer**. New York: Cambridge University Press, 2013. 328 pp. \$34.99 paper. ISBN: 9781107673243.

To explain where and why nation-states first emerged, Wimmer contrasts France to the Ottoman Empire over a millennium. He relies on "a power-cum-legitimacy approach" (p. 6), an institutionalized balance of power and stable exchange relationships. As summarized in the following table, his modeling rests on four sets of actors:

Elites		Masses
Dominant	Dominant elites	Dominant masses
Subordinate	Subordinate elites	Subordinate masses

In the wake of a successful nation-building project, the post-Revolutionary French "nation" spanned and incorporated religious divisions, regional rivalries, linguistic communities, and ethnic groups. In contrast, the Ottoman Empire fragmented. Wimmer adopts a game-theoretic framework and simulations in Chapter Two to explain this divergence. In France, a strong central state coupled with a civil society marked by dense and effective voluntary organizations provided fortuitous conditions for a nation-building project. In this context, "the idea and institutionalized practice of solidarity among all elite and non-elite sections of the population" were nurtured by ongoing exchanges. This inclusive alliance system drew "boundaries of belonging

against non-national others rather than against a particular segment of the domestic population" (p. 46). It also yielded valuable resources for waging war and set the stage for institutionalized bargaining that allowed non-elites to secure expanded civil, political and social rights in the twentieth century. By contrast, the nineteenth-century Ottoman Empire did not have a strong central state, nor were dense voluntary organizations found there. Instead, subordinate (defined in ethnic and religious terms) elites and masses maintained durable exchange relationships that competed with and impeded efforts to forge an overarching Ottoman nation.

Because his theory could not be tested with extant databases, Wimmer (with several collaborators) undertook ambitious and demanding data-collection efforts. For example, when examining the spread of nation-states (Chapter Three), Wimmer's dataset includes most of the world from 1816 to the present. His cases are the territories recognized as states in 2001. Each of these territories in the dataset was at risk of becoming a nation-state (i.e., rule established in the name of the people) (p. 86). Using an event history approach, he finds that transitioning to a nation-state was significantly more likely as empires waned. Nation-state transitions were associated with each of the following: transitions to nation-state elsewhere in the empire, the number of years since an initial nation-state transition within an empire, and the number of wars fought in the empire.

While these positive findings are noteworthy, the non-findings may be of greater importance. Wimmer challenges claims that economic modernization (Ernest Gellner) contributes to a transition to the nation-state. Likewise, because literacy is not associated with these transitions, Wimmer calls into question the emphasis that Benedict Anderson places on "imagined communities." Arguments focused on the state's capacity to rule (Michael Hechter and Charles Tilly) are also contradicted. Nor do Wimmer's findings support world polity's expectation that the nation-state template diffused globally. Instead, diffusion effects are restricted to a territory's immediate neighborhood and across empires. These findings are

provocative; and because they are based on a dataset that spans centuries and the entire globe, they must be taken seriously, even and especially by skeptics.

In turn, Wimmer examines the links between nation-state formation and war over 200 years. By examining trends over two centuries, he finds that war is more likely at a time of institutional change like absorption into an empire or seceding from an empire to create a nation-state. When Wimmer turns his attention to ethnic politics since World War II, he moves beyond a focus on minorities at risk of persecution: he (and associates) gathered data on "all politically relevant ethnic groups and their degree of access to executive-level state power—from total control of the government to overt political discrimination and exclusion" (p. 144). *Waves of War* also distinguishes among the types of conflict: rebellion (fights over the boundaries of inclusion), infighting (ethnic elites fighting among themselves), and secession (struggles to change the polity's territorial boundary). Wimmer reports that ethnic exclusion predicts rebellions (fights over the boundaries of inclusion) but does not influence infighting and secession. The duration of imperial rule makes conflict over secession more likely but does not exert significant influence over rebellions and infighting. He also finds that the number of power-sharing partners (in the executive offices of the state) makes a significant contribution to infighting among ethnic elites but was unrelated to rebellions and secessions.

Wimmer stresses the value of variable-oriented research and points to the limitations of case-oriented historical accounts. He is especially critical of accounts that take the nation-state as a given (and the unit of analysis) and accounts that make sweeping theoretical claims on the basis of a handful of cases. He acknowledges the value of case-oriented historical research and characterizes statistical analyses as an attempt to "identify recurring patterns in the tapestry woven by hundreds of such specific historical threads" (p. 7). The several appendices provide a thoughtful discussion of data sources and the challenges of coding important variables. Further, Wimmer believes that nuanced review of historical narratives is necessary to ensure that the

regular patterns that surface in statistical analyses are not spurious. Still, skeptics of cliometrics will find plenty to debate in the overall approach and offer sharp criticisms of specific implementation choices.

The core empirical chapters were published as articles (in the *American Sociological Review* and *American Journal of Sociology*) and received multiple awards from sections of German and American sociological associations. Even with Wimmer's efforts to integrate these chapters, their origins as separate articles is evident. Each chapter presents a separate database and analytic technique. Further, there appear to be important differences in the meaning of key concepts and subtle, but consequential, shifts in measurement (see below). Notwithstanding, there was a synergy among these separate articles (now chapters), and bringing them together in *Waves of War* makes this synergy obvious and provides Wimmer the opportunity to advance an innovative and far-reaching agenda for comparative historical research and theorizing.

One of the strengths of Wimmer's work is his dogged commitment to building databases that cover the entire world over several centuries. But Wimmer is more interested in variables than cases. Wimmer's coding seems to shift across these chapters—especially for the world's most powerful countries. Although this might be an artifact of incorporating several articles published independently, I think it is a symptom of larger and less tractable problems.

According to Wimmer (and associates), for a polity to be considered a nation-state, two conditions must be met: the "sovereign right to rule" is in the name of the "people" and "foreign rule of all sorts" is overcome (p. 86, see also p. 256). In Chapter Three, for purposes of studying the spread of the nation-state (pp. 238–39), Wimmer asserts that Japan and Germany became nation-states in the middle of the nineteenth century (1868 and 1871, respectively) and have been nation-states since that time. This would, of course, come as a surprise to the Emperor of Japan and the German Kaiser, both of whom were under the impression they ruled over an empire. For that matter, the Nazis who ruled Germany from 1933 to 1945 claimed sovereignty over an empire-like Reich, not a nation-state. After World

War II, the United States and its allies occupied these countries and forced a new constitution on each of them. These constitutions—in marked contrast to the Japanese Empire and German Empire/Reich—do emphasize the hallmarks of a nation-state. In the first half of the twentieth century, did Japan and Germany act as nation-states or as empires? In Chapter Four, given his reference to "the attempt by Nazi Germany to establish an imperial policy in Eastern Europe" (p. 127), Wimmer apparently thinks that the Japanese Emperor, German Kaiser, and Nazi high command were correct in thinking they ruled over empires. Wimmer provides evidence that imperial expansion and transition to nation-statehood (with the wars of secession that come with it) are important causes of inter-state wars. This is an important finding, but its implications are obscured by the confusion over the polities waging imperial wars. While this might be a legacy of separate articles (and datasets) included in a single book, it raises important conceptual questions. Can nation-states wage imperial wars? Or, do wars of imperial expansion imply an empire?

According to Wimmer, Russia transitioned to a nation-state in 1905 and has been a nation-state since that time (Tsar Nicholas II's likely objection notwithstanding). He believes that the Soviet Union's absorption of countries in the Baltic and Caucasus regions ended their nation-statehood. When the Soviet Union collapsed in the early 1990s, these countries underwent a second transition to nation-statehood (pp. 237–410). But Russia became a nation state in 1905, and it remained one throughout the Soviet era (at least in Chapter Three). Elsewhere, especially when he turns his attention to nation-state transitions and war (Chapter Four), Wimmer refers to the "Soviet empire" and makes no mention of the Russian nation-state. How did the Russian nation-state survive Soviet rule when the other Soviet republics lost theirs? Or, is Wimmer treating the "Soviet empire" (Wimmer's terminology) as synonymous with the Russian nation-state? Stalin (a Georgian, not a Russian) would likely disagree with this coding decision.

Nor does *Waves of War* comment on the fact that China (in Wimmer's database, the

territory controlled by China in 2001) corresponds with the territory controlled by the Chinese empire over millennia. Similar tensions are evident in the handling of Great Britain and France. For Wimmer, these are the quintessential nation-states. They transitioned early and changed the rules of the geopolitical game. But over this same period, they were the world's great imperial powers. In some passages (especially in Chapters Two and Three), Wimmer emphasizes the revolutionary social bargain that a nation-state entails. Elsewhere (Chapter Four), he talks about the waxing and waning of the British and French empires. Can France and Great Britain be both a nation-state and an empire? Or are empires qualitatively different from nation-states?

Wimmer is quite emphatic that the United States cannot be considered a nation-state with a constitution that embraces slavery and prohibits citizenship for slaves (p. 87, n. 13). For this reason, the United States only becomes a nation-state in 1868 with the passage of the Fourteenth Amendment (p. 241). Abraham Lincoln did not think the Civil War was being waged to create a nation-state. Rather, it was waged to make sure that a nation so conceived did not "perish from the earth" (Lincoln 1863). A U.S. President cannot be the arbiter in such a dispute. But in his introductory chapter written for this book, Wimmer includes the United States (along with Great Britain and France) as one of the first nation-states, the nation-states that changed the rules of geopolitics (p. 4). Was the United States among the first nation-states as Lincoln and Wimmer assert (at least in the introduction)? Or, was the United States something of a laggard as Wimmer and associates claim in Chapter Three, only becoming a nation-state in 1868?

Wimmer's knowledge of world history is remarkable, as are the breadth and depth of historical works he has consulted. Moreover, his thoughtful and open discussion of coding conventions and challenges to implementing them is laudatory. This book includes 200 pages of text, and it also devotes 100 pages to appendices that provide rich detail on sources and coding decisions. I am confident in Wimmer's ability to address these inconsistencies—his

thoroughness is evident throughout the book, and so are his considerable abilities.

The question is not *if* Wimmer can impose consistency; the more important question is whether he *should*. Wimmer insists that each case (or more specifically, the variables that describe its attributes) be given equal weight. This drives Wimmer and associates to select one year to capture the often contradictory, confusing, and complex transition to nation-statehood. By sampling territories claimed by nation-states in 2001, he placed equal analytic weight on the world's smallest and least powerful states as he did on the largest, most powerful and most populous countries. The contradictions may well flow from these foundational assumptions.

Ragin (1987) identifies limitations to variable-oriented research in general, and these are magnified when global macro-societal comparisons are attempted. Regardless of the unit selected, the researcher will be forced to wrestle with sharply dissimilar cases (*ibid.*, p. 9), between "satisfying the demands of statistical techniques" and "the theoretical, substantive, and political concerns" that motivated the research in the first place (*ibid.*, p. ix). The implausible and inconsistent coding decisions for the world's most powerful countries likely flow from Wimmer's quixotic efforts to impose uniform coding rules. The world's most powerful countries are at the same time nation-states and empires (including, according to Mann [2012], the United States). But acknowledging that some nation-states are at the same time empires would create havoc for Wimmer's parsimonious coding scheme.

Some nation-states also have empires. If a reasonably strict definition of empires with global horizons is imposed (Mann 2012), the total number of cases over the past 200 years would not exceed 10. These nation-states with empires wield disproportionate influence over the existence and attributes of the territories (as of 2001) that Wimmer samples. He places on a single continuum territories of continental proportions and home to states and economies that set the terms of global economics and geopolitics (e.g., Great Britain, France, Germany, Japan, Russia, the United States, and China) and much smaller territories with modest

economic and geopolitical impacts (many of them occupied and controlled by the nation-states with empires).

The problems go beyond quibbles over measurement, and they cast doubt on Wimmer's central conclusions. He argues that the nation-state social compact was at the same time a very compelling basis for legitimacy (the rule of like over like). Moreover, he contrasts this to empires and argues that the nation-state enjoys decisive advantages in the realm of warmaking. His account, however, ignores the powerful influence wielded by countries that were at the same time nation-states and empires. This sweeping power was on display during and after World War II. The victors occupied and rewrote the constitutions of the losers and created enduring international organizations. Each of the permanent members of the U.N. Security Council was at the time of World War II a nation-state and an empire: the United States, the United Kingdom, France, China, and the Soviet Union (now its successor state, Russia, holds a permanent seat).

Waves of War devotes considerable attention to the demise of the Ottoman Empire. Given its historic importance, this is well-justified. But Wimmer says little about the empires that survived into the twentieth century. With few exceptions, the decolonizations that occurred after World War II were instances in which a nation-state survived (e.g., Great Britain, France, Holland, and Portugal) and overseas colonies become new nation-states. When a territory seceded from a land-based empire (e.g., the Ottoman Empire or Soviet Union) and established a nation-state, this newly created polity stood in sharp contrast to the form of rule at the center of the empire. However, in the case of post-World War II decolonization, establishing a nation-state was not only acceptable to the mother country, it was actively supported. In fact, most newly independent nation-states borrowed heavily from the constitutional provisions upon which the mother country was founded. The strong, direct, and obvious links between the mother country and colonies are invisible in *Waves of War*. Instead, Wimmer emphasizes horizontal diffusion

among the territories within the empire: the likelihood of a nation-state transition increases if other newly independent countries within the empire transitioned to a nation-state. Did the nation-state form diffuse horizontally as polities seceded from empires, as Wimmer claims? Or, at least in the post-World War II era, was the nation-state imposed from above by the mother country and reinforced by international structures created by the nation-states with empires that prevailed in World War II? *Waves of War* neither asks nor answers these questions.

In the introductory and concluding chapters, Wimmer explains how these databases and analyses, together, reframe debates over nationalism, war, and ethnic conflict. He shows that the institutional transitions are of central importance and that data must be structured and collected with this in mind. The empirical analyses at the heart of this book drive home the centrality of ethnic politics in contemporary conflict and state transitions. The synergy among the several empirical chapters and the extensive work that Wimmer did to develop, extend, and refine his theoretical argument make this book informative, provocative, and valuable.

I strongly recommend *Waves of War* for a wide range of graduate seminars: seminars on nationalism, state formation, and the like would be well-served by using this book, as would surveys of the political-sociological and comparative-historical literatures. Further, because Wimmer offers a refreshing openness and thoughtfulness concerning data, theory, and method, this book might prove useful in seminars addressing these broad topics.

In criticizing the book, I identified implausible and inconsistent coding decisions to call into question the conclusions presented in *Waves of War*. Even if these criticisms are valid, this challenging book will and should frame debates. Wimmer has a great deal to say, often surprising, about the concept and history of nationalism. By compiling and analyzing truly global datasets that span several centuries, Wimmer can and does give equal weight to all regions, nations, and peoples. While his defense of

quantitative and variable-oriented research is manifest, Wimmer presents this as extending and complementing case-oriented historical research (see above). To decide if the findings flowing from his quantitative analyses are spurious, he encourages a continued dialogue with case-oriented research. Macrohistorical and comparative sociology would be well served if others, critics and supporters alike, accept Wimmer's invitation to use this book as a focal point of debate.

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Middle Class Inequality: The Market Hits Home

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The affluent soar upward to ever-greater riches, the poor dig deeper to survive, and the middle class inches forward, stuck on the ground. This broad summary of economic trends in the United States over the past forty years also describes many other prosperous countries. The theme varies some, but the up-down-stuck theme tells us a great deal about globalization and economic inequality since 1975. Corporations, markets, and, increasingly, workers span national boundaries. As they do, the institutions that once made one nation different from another have less leverage over factors that make nations more alike while pushing individuals within each nation farther apart.

An interdisciplinary team of social scientists led by Janet Gornick and Markus Jäntti have, nonetheless, uncovered enough variation among nations to assemble a fascinating but demanding volume of empirical research. *Income Inequality: Economic Disparities and the Middle Class in Affluent Countries* fascinates because it focuses the inequality discussion on those who are stuck in the middle of the income distribution. The soaring rich and burrowing poor get the bulk of academic and journalistic attention—and for good reason. Social and economic disparities are rooted in the behavior of the rich and affect the poor more than others. But the middle is the biggest segment (metaphors like "hollowed out"

Income Inequality: Economic Disparities and the Middle Class in Affluent Countries, edited by **Janet C. Gornick** and **Markus Jäntti**. Stanford, CA: Stanford University Press, 2013. 515 pp. \$65.00 cloth. ISBN: 9780804778244.

notwithstanding) and politically pivotal. Thus, the attention in these papers on the middle class is welcome.

Income Inequality is demanding because so many details matter. For example, data show more inequality in both wealth and wages than in disposable income. That means that somehow people form families and allocate labor market time in ways that counter, however incompletely, the centrifugal forces of global capitalism. Governments variously encourage or discourage postsecondary education, saving, home-ownership, and family formation; the incentives they give people and corporations affect how much inequality their citizens live with. The scholars who contribute to this volume worked hard to master the complexity; they leave us with few quick and easy generalizations, but many details worthy of further research.

The revolution in women's education and employment during the middle of the twentieth century is the key factor in middle-class

life. In almost every chapter, women's contributions to the income, care-giving, and composition of the family weigh heavily. Complications and contradictions pile up. The gender gap in hours worked actually decreases inequality; the gender gap in wages might be increasing it. Much depends on the correlation between men's and women's wages within households. Susan Harkness shows that women's hours at work reduced inequality relative to what it would have been if no women worked or if all women worked the same number of hours. It is far from a foregone conclusion. Simple theories of labor supply do not imply this result, but Harkness finds the pattern in all 17 countries in her dataset. Women's hours at work reduced inequality mainly because the correlation between the man's and the woman's wages in two-sex, two-earner households was modest in all those countries. If all women work full time in the future, this counterbalance will go away; inequality would rise as a consequence. Now the female partners of low-earning men work more hours than other women, raising their households above some of those with high-earning men; equalize women's hours and the inequality of family incomes aligns more with that of men's wages.

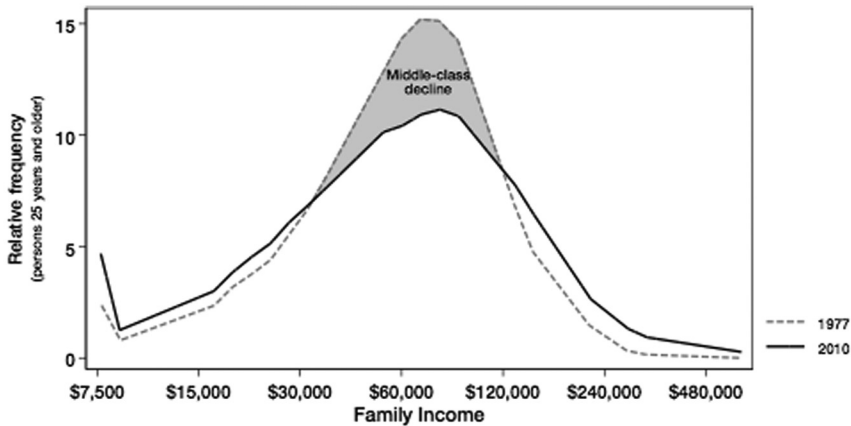
Women with paychecks become visible in the macroeconomic data. Their contributions to well-being through unpaid work at home are the invisible economy. Nancy Folbre, Janet Gornick, Helen Connolly, and Teresa Munzi recalibrate the statistics by bringing the unpaid work done at home into the account. If social science could put a fair value on at-home production we would learn that rich countries were richer in the past than we thought and, because we know from time-use surveys that women do less work at home now than before, countries are not quite as rich now as they might be if more work was done at home. As societies segue from home-based childcare and food production to paid work for those goods and services, they conclude, "As women reallocate their time from unpaid to paid work, household inequality is likely to increase, both because the hours of paid work are distributed more unequally than hours of unpaid work and because the imputed hourly value of unpaid work . . .

varies less than market wages" (p. 256). They make a number of calculations based on their specific imputation of the hourly value of unpaid work, but their conclusion holds for any reasonable imputation. They do not say that inequality would go away if half of us stayed home half the time; you cannot pay the mortgage with imputed dollars. They do take seriously the proposition that unpaid work contributes to well-being. In doing so they show that today's division of labor makes the men and women within these households somewhat more equal while adding to inequality among households.

Wealth inequality for the middle class starts at home. For families in the middle of the income distribution, the dividing line between positive and negative net worth is home ownership. Among those who are not yet in a home of their own, affordability matters most, as Eva Sierminska, Timothy Smeeding, and Serge Allegrezza make clear in their chapter on assets and debt. Home owners, though, depend on rising home values; stagnant or falling house prices turn their most valuable asset into a burden. In all countries, single-parent households are more likely to rent than own, even after adjusting for family income. It is clear why that should be at low incomes, but it persists, contrary to expectation, in the top deciles. The authors call for dynamic data; they cannot sort out causes and effects without information on the sequence of events.

Jäntti, Sierminska, and Philippe Van Kerm compare incomes and wealth. They begin with the commonplace observation that nations line up the same way on both — those most unequal on incomes are most unequal on wealth — but discover that households and individuals do not. Young professionals have high incomes but below-average wealth; some seniors have little income but live off dividends and interest from their accumulated wealth. Everything that predicts income predicts wealth, but the predictors do a far better job accounting for income than wealth. Perhaps inheritance makes the difference. Again, the closer we look the more we learn and the more questions we ask; the more questions we ask, the more data we need.

Debt rose to the fore as subprime lending, derivative bundling, and loan insurance



Note: The area under each curve is standardized to 100 so the height above any dollar amount reflects the relative frequency of that income. Incomes adjusted for inflation (2010 prices) and then transformed to a ratio scale to make the display clearer.
Source: Author's calculations from the Current Population Surveys, Persons 25 years old and over, March 1978 and March 2011.

nearly brought down the global financial system. People borrow for big items like their homes, cars, and college educations. But some people, especially young people, pile up consumer debt with daily expenses. Consumption inequality is even less than income inequality. This volume has less to say about these trends and regularities than it might have.

Several authors, especially Arthur Alderson and Kevin Doran in the first empirical chapter, describe the income distribution as “hollowing out.” It is the wrong metaphor, and its repeated use in this volume and elsewhere can distort both analyses and conclusions. The image derives from plots like Figure 1.2 (p. 60) of Alderson and Doran’s chapter. They carefully discuss in four panels how the typical income histogram for the countries in this book gets lower with fatter tails. The share of the population in the lowest and highest deciles rises, while the share in the middle falls. It is the distribution of changes that takes on the U-curve with a hollow middle; the distribution of income itself is still, in every country in this book, higher in the middle than at the ends. The middle class is much smaller than it used to be; call it squeezed, squashed, or dispersed. Yet, after a major change, the middle class is still far larger than either the poor or the rich. As Anthony Atkinson and Andrea Brandolini show, social scientists work with several definitions of the middle class, but it remains the biggest economic segment

by any reasonable definition. The above figure illustrates by comparing the U.S. income distributions of 1977 and 2010. The horizontal axis goes from the lowest to highest incomes; the height of each curve is proportional to the share of the population with that much income. In 1977, 69 percent of American households had an income between \$33,000 and \$120,000 (the amounts where the density lines cross); by 2010, that was down to 56 percent. We all know the American middle class is smaller than it used to be, but we also have to keep in mind that it is still the biggest social, economic, and political segment.

We know these facts and much, much more about how income and wealth are distributed among households in almost forty nations because of the LIS (the letters once stood for “Luxembourg Income Study;” now they are just three letters). Beginning in 1983, economist Tim Smeeding and psychologist Gaston Schaber along with the sociologist Lee Rainwater integrated and standardized measures of income and poverty from high-quality, nationally representative datasets for a handful of countries. Over time the project grew to include more nations, more time periods, and more measures, most notably wealth. Political scientists joined. Altogether the interdisciplinary collaboration is very impressive. The seventeen papers of this volume share that trait; nine authors are economists, eight are sociologists, four are political scientists (one of

whom has a joint appointment), and nine more have appointments in interdisciplinary professional schools and research institutes. The chapters mesh. The editors and authors both deserve credit for this model of collaboration and eclecticism.

To summarize, *Income Inequality* highlights why the middle class is sociologically, politically, and economically interesting. Figuring out why the average working family in rich countries is not keeping pace with economic growth turns out to be more intellectually challenging than it might first appear to be. The right trusts the rich to advance all of society; the left would redistribute

incomes to raise the poor. The center-left garners votes with middle-oriented rhetoric but has yet to change the trend lines. The papers in this volume suggest that supporting women's opportunities and wages, promoting middle-class wealth accumulation, and regulating debt can all promote greater equality in living standards. The social science blends gender, family, politics, and economics. The patterns defy easy summary, suggesting that the complexity may be part of why problems persist. But Gornick, Jäntti, and their collaborators are leading us in the right direction.

Reading *The Great Transformation*

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Karl Polanyi's book *The Great Transformation* is a classic. First published in 1944, it has come to be recognized as a founding charter for economic sociology. It anticipated major accomplishments of late-twentieth-century social science (including, among others, Ben Bernanke's studies of the Great Depression and Amartya Sen's work on famine). Its core problems—how do societies respond to globalization? how do they address the risks of market failure?—are central to contemporary macrosociology. It is probably time to recognize the canonical status of this book and put it on the classical theory syllabus alongside Marx, Weber, and Durkheim.

But *The Great Transformation* is also—can we admit this about our classics?—a mess. It is conceptually sloppy. Some key terms (such as “market society” and “social dislocation”) are never explicitly defined. Others (such as “a ‘movement’”) are defined with pedantic care, and then used willy-nilly, as if Polanyi forgot what he said the words meant. Important steps in the argument assume what is to be proven. Inconsistencies abound. The treatment of historical sources is casual. To round it all off, the book concludes with a breathtakingly wrong prediction: namely, that the era of market liberalism is over for good—as of 1944. To salvage

The Great Transformation: The Political and Economic Origins of Our Time, by **Karl Polanyi**. Boston, MA: Beacon Press, 2001. 317 pp. \$25.00 paper. ISBN: 9780807056431.

The Power of Market Fundamentalism: Karl Polanyi's Critique, by **Fred Block** and **Margaret R. Somers**. Cambridge, MA: Harvard University Press, 2014. 296 pp. \$49.95 cloth. ISBN: 9780674050716.

a theory from all this, let alone one that we can apply to societies in the present day, surely requires a heroic effort of interpretation.

Fred Block and Margaret R. Somers undertake the salvage effort in *The Power of Market Fundamentalism: Karl Polanyi's Critique*. Block and Somers have done more than anyone to ensure that Polanyi's text gets the recognition it deserves: their book collects and revises several critical and exegetical essays that they have written over three decades. The result is an important, interesting, and idiosyncratic reading of *The Great Transformation*.

It is grounded in a serious intellectual history of Polanyi's early milieu. Block and

Somers argue, based on archival evidence, that Polanyi was a Hegelian Marxist at the time he began work on the manuscript for *The Great Transformation*, but broke with Marxism and developed his own theoretical system through the process of writing the book. The task of the interpreter is therefore to distinguish the mature, coherent system from the youthful, Hegelian elements that are still present in the text and that represent an earlier stage in the author's thought. What Block and Somers claim to be doing for Karl Polanyi, in other words, is almost exactly what Louis Althusser and Étienne Balibar claimed to be doing for Karl Marx in *Reading Capital*; as with Althusser and Balibar's reading of *Capital*, the result is not always persuasive as a gloss on the original text, but it is an interesting theoretical contribution in its own right.

The outline of Polanyi's critique of market society is well known. A self-regulating market economy of the sort contemplated in general equilibrium theory requires that the factors of production—land, labor, and capital—be commodities. But land, labor, and money are not produced for sale and their supply is not price-elastic in the short run. To attempt to set up a self-regulating market system on the assumption that these three factors of production are commodities is therefore to court disaster. (The precise nature of the disaster is a point on which the text is inconsistent, but famine is a plausible example.)

To this, Polanyi adds a historical argument: in the latter half of the nineteenth century, some people in England seemed hell-bent on setting up just such a self-regulating market system on a world scale—and courting that very disaster. The only reason human society survived was that the expansion of the market was checked by a protectionist countermovement. This countermovement took very diverse forms: Polanyi lumps together under this heading utopian communes, fascist militias, trade associations lobbying for tariffs, and paternalistic welfare programs. What these collective efforts had in common was that they aimed to sequester resources from the market and allocate them according to some other principle. Some such protectionist countermovement, Polanyi argues, is inevitably to be

expected when the market principle is applied in ways that threaten social disaster.

Many of the problems with this argument are also well known. Here is one: Why are social protectionist movements forthcoming just when they are needed? In Polanyi's account, the answer is that society needs them: "For if market economy was a threat to the human and natural components of the social fabric, as we insisted, what else would one expect than an urge on the part of a great variety of people to press for some sort of protection?" (p. 156). That does not so much answer the question as restate it. He later adds that the intervening causal mechanism is the action of classes or sections—the task of defending society ultimately "fell to one section of the population in preference to another" (p. 169)—but the question is why it should have fallen to anyone instead of just falling through the cracks. Even the tasks that are necessary to save a society sometimes go undone. (Not all societies last forever.)

A second vexing problem in *The Great Transformation* is why anyone ever believed that a self-regulating market was possible. Polanyi emphasizes that national and global markets could only be instituted through the efforts of zealots who shared an almost millennial belief in the self-regulating market economy (p. 139). But he also argues that such a market system is a "stark utopia" (p. 3)—not only impossible, but obviously impossible. True believers in a general market equilibrium have to assume that prices and quantities of labor, land, and money all adjust as if these are commodities produced for exchange; but, Polanyi asserts, "labor, land, and money are obviously *not* commodities" (p. 75). So how was anyone snookered into thinking that they were?

Block and Somers address these problems head on. They are unsparing critics of Polanyi's text, and in order to keep his theory afloat they are willing to throw a lot of things overboard. Some of what they throw out is baggage that few readers will miss—such as Polanyi's quasi-Hegelian assumption that a society is a unified subject that knows, has interests, and acts to achieve its ends. Other things that they discard, such as the concept of capitalism (p. 78), may surprise readers. Much of Polanyi's historical

narrative goes overboard, too. Block and Somers make a particularly strong case for jettisoning his account of “the Speenhamland system.” This was a late eighteenth-century system of poor relief that, on Polanyi’s account, kept people from starving during the transition to labor markets in rural England, at the price of cultural degradation. His account of Speenhamland was, to an earlier and more conservative generation of interpreters, the point of the book. Block and Somers show definitively that it is wrong and argue that it is unnecessary to his purposes in any case. Out it goes.

What we are left with is a spare “conceptual armature” (p. 8) that bridges some of the most troublesome gaps in Polanyi’s book. The foundation of the reconstructed theory is “holism,” the premise that the action of institutions and social classes can be understood only in relation to each other (p. 58). The relatively stable relations among institutions, in turn, establish “opportunity structures” (p. 69) that shape action. In place of the image of society as a coherent, unified subject with mysterious powers of self-defense, their reconstructed Polanyi gives us a vision of society as an articulated set of arrangements for living—or as a congeries of “multiple social institutions and dense networks of social relationships” (p. 226). In order to explain why protectionist countermovements emerge, in this version of Polanyi’s theory, we need not assume that anyone actually knows what is good for the whole society, nor that anyone is able, or even willing, to act effectively to secure the universal good. Instead, we need merely assume that people may act to defend their vested interests in particular relationships and institutional arrangements that are threatened by disruptive competition.

More provocatively, Block and Somers solve the epistemic problem—why did anyone believe in the market utopia?—by arguing that certain ideas enjoy “epistemic privilege” (p. 156). Epistemically privileged ideas have an inherent persuasive power not only despite their empirical implausibility, but because of it. Their example is “market fundamentalism,” or the belief in “a sacred imperative to organize all dimensions of social life according to market principles” (p. 150). Market fundamentalism is

persuasive because it is often accompanied by three other, interdependent ideas—a claim that its conclusions can be deduced from real but unobservable causal mechanisms that underlie empirical regularities (“theoretical realism”); a claim that these real mechanisms are “natural” in the sense that they are pure givens, neither plastic nor amenable to human design (“social naturalism”); and a self-serving story about how the true believers came to know the truth behind appearances (the “conversion narrative”) (p. 158). A doctrine that combines these three ideas is immune to refutation, because it is unfalsifiable. Block and Somers argue that *any* such doctrine will tend to outcompete a doctrine that does not—it has an intrinsic “comparative advantage” (p. 156) in the marketplace of ideas—and, if it is a political or social doctrine, its persuasive power may even give it the character of a self-fulfilling prophecy. An epistemically privileged idea can make itself true by persuading people to reorder the world in accord with its premises (pp. 107, 156).

All this talk of opportunity structures and self-fulfilling prophecies may sound more like Robert Merton than Karl Polanyi. Nevertheless, Block and Somers assert that this theory, or something like it, is implicit in *The Great Transformation*. They further argue that this reconstructed Polanyian theory permits us to explain events that took place after the publication of *The Great Transformation* and that might otherwise seem to call Polanyi’s argument into question.

Probably the greatest such anomaly is the development of popular support for the self-regulating market. *The Great Transformation* explicitly describes a tension between market economy and democracy. In Polanyi’s view, popular suffrage was incompatible with a free market in labor, because enfranchised laborers would vote themselves social protection and use state power to take wages out of competition. Events of recent decades would seem to challenge this view: particularly in the United States, political sociologists have watched voters, including many working-class voters, elect candidates who espouse relatively extreme versions of market liberalism.

Block and Somers argue that a properly reconstructed Polanyian theory can account

for this apparent anomaly. They argue that international economic arrangements, and in particular, the post-Bretton Woods liberalization of trade and finance, created new competitive threats for workers in the United States, while constraining the policy options available to respond to those threats. Many working-class people, especially in the regions most exposed to competition, therefore sought social protection in other ways—by turning to churches, for example, or embracing nationalist candidates who promised to exclude non-citizens from competing for a share of the federal budget. Block and Somers push this interpretation of the American populist right very far, and some of their examples seem to me far-fetched—the anxiety for the fetus evident in the rhetoric of the pro-life movement, for example, is said to be a displacement of anxieties about “market forces” (p. 204)—but in very general terms, I think they are on to something. Much of what appears to be free market sentiment on the populist right is, in effect, disguised social protectionism: many far-right voters seem to oppose welfare benefits for immigrants, for example, in part because they have a vague idea that the expense of such benefits might jeopardize the fiscal sustainability of their own government benefits. They are not acting in defense of society as a whole. They are, however, defending particular social arrangements against the threat of competition produced by the internationalization of labor markets, and a chastened Polanyian theory may be helpful in making sense of this defensive mobilization.

A second knotty problem for would-be Polanyians today is to explain how the belief in economic liberalism survived the great transformation that Polanyi thought had killed it. Block and Somers’ answer to this question—signaled by the title of their book—is that the reason for the revival is inherent in the market fundamentalist idea itself. Epistemically privileged ideas have more staying power, regardless of their truth or falsehood. (As evidence for this staying power, they offer a long list of parallels

between the early nineteenth-century arguments of Thomas Malthus and the late twentieth-century arguments of various conservative Republican critics of welfare for poor single mothers.)

There is much more to *The Power of Market Fundamentalism*, including a chapter on Polanyi’s anti-utopian rhetoric that is particularly illuminating. Because it is a book about the power of ideas, however, it may be appropriate to conclude this essay by speculating about the impact that this book itself is likely to have. Like *The Great Transformation*, it is more than a work of historical interpretation and theory-building; it is also a polemic against economic liberalism, and a manifesto for a sociological alternative. But without Polanyi’s faith that some group will inevitably act to defend the interests of society, it is a strangely pessimistic manifesto. Block and Somers offer no theoretical blueprint for the proper form of a mixed economy. They reject the call for a grand economic theory of everything. Instead, they call for democratic experiments rooted in local communities and workplaces. They oppose market fundamentalism in the name of a “new public philosophy” (p. 224) that is antinaturalistic, empiricist, and pragmatic: we will find our way by a kind of collective, democratic tinkering. This public philosophy is appealing, but if Block and Somers are correct, it is also likely to be ineffective. It calls for a knowledge that is too falsifiable, too little sure of itself; the comparative advantage will go to the fundamentalists, market- and otherwise, because they have the holy trinity of rhetorical resources (naturalism, realism, the conversion narrative) that confers epistemic privilege. *The Power of Market Fundamentalism* summons the uncertain pragmatists to the barricades so that they can do battle against market fundamentalism—and lose.

I am glad that Block and Somers wrote this book, but I hope they are wrong about the power of ideas, so that their book can have the impact it deserves. I also hope, as they do, that it sends more of us back to re-read *The Great Transformation* for ourselves.

Labor and Inequality: American Society After the Decline of Unions

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Sociologists of labor are fond of extolling the pros and cons of what trade unions do or have done in the past. But the *consequences* of labor's near-disappearance are rarely mentioned. This is the main question in Jake Rosenfeld's eye-opening book, *What Unions No Longer Do*. Rosenfeld assesses the outcomes of an American society in which its workers no longer benefit from high levels of union membership. In so doing, he has told a new story about a central feature of contemporary American life—inequality—and has tied it to what is typically regarded as nothing more than a footnote of U.S. history—the decline of the labor movement. In the United States, union membership declined at the same time that economic inequality increased. What is the connection?

Labor's Rise and Fall

The post-World War II context proved especially fertile for labor organization, and by the mid-1950s, approximately one-third of all non-agricultural workers had a union card in their wallet. During these prosperous—and anomalous—decades after the war, Rosenfeld argues that a “tripartite arrangement” consisting of labor, government, and business, helped keep wages high. Unions acted as “pay-setting institutions.” Moreover, high levels of union membership helped elect political elites who favored, or at least had to deal with, unions and workers, providing the context of broad socio-cultural support for unions. In this context, workers' institutions were not bit actors, as many historians have argued. Rather, the labor movement grew into “the core equalizing institution” (p. 2) of post-war capitalism, the backbone of a rising multiethnic middle class (emphasis in original).

However, union membership declined precipitously throughout the seventies and eighties. By 2009, union membership in the private sector, where it has historically had

What Unions No Longer Do, by **Jake Rosenfeld**. Cambridge, MA: Harvard University Press, 2014. 279 pp. \$39.95 cloth. ISBN: 9780674725119.

the largest impact on workers' livelihoods, clocked in at 7 percent, the same level as during the first year of the Great Depression.

A few explanations for this trend have risen to prominence in sociological circles. The globalization of industrial production and the rise of an economy dominated by services and information technology have been seen as a context antithetical to unionization, as well as a prime way that the high union densities within basic industries were either eliminated, shipped overseas, or erased by frightened workers voting out the union and settling for lower wage premiums. Rosenfeld deals less with this explanation, however, in favor of two others.

First, many scholars today see unions themselves as their own gravediggers because their highly bureaucratized organizations that solidified after World War II were not equipped to organize new workers. Second, while Rosenfeld allows that declining approval rates for unions are a reality, he insists they be considered in the context of contemporary politics. In other words, behind both of these trends are political changes that favored the growth of viciously anti-union employers and lawmakers who have played a leading role in labor's fortunes. In making this claim, he adds a political dimension to the crisis of union decline, a central concern for labor sociologists.

Having established a more comprehensive understanding of labor's fate, Rosenfeld turns to his main act: explaining why it matters so much. Through rigorous statistical analysis and a keen eye for history, Rosenfeld demonstrates that the ramifications of labor's historic decline are much broader than we previously thought. His work

suggests that what is of central concern for labor scholars should be on the radar of economic and historical sociologists as well as, in fact, anyone interested in inequality.

Unions and Inequality

Unions are pathways to social equality in a number of ways. First, unions raise the wages of the lowest-paid workers more, relative to better-paid workers in the same workplace, thereby contributing to wage parity. Second, unions often try to bargain for equal pay for those whose skills and experience levels are more or less the same, eliminating racial, gender, age, and other biases in pay and treatment. Third, high union densities structure the economic and cultural climate for all workers, union or not, through a spillover effect. For example, when union levels within a given region or industry are high enough, even nonunion employers tend to increase benefits and wage packages to deincestivize unionization. Finally, unions provide working people with organizational ties to political issues that impact their lives. When unions are strong, they help to elect sympathetic politicians who place constraints on the power of business. Given this, the story of the rise and fall of unionism has much to tell us about our current predicament.

Rosenfeld argues that one traditional union strategem—striking—is rarely done anymore. Large strikes, now nearly eliminated (he says there were almost 500 in the early 1950s, and the count collapsed to five in 2009), are one weapon that unions have given up or traded in. Falling union density rates combined with the hard lessons of the 1980s and 1990s chastened even militant unions into filing grievances instead of taking to the picket lines. However, contrary to some labor scholars who see the resumption of the strike as necessary to rebuild labor's former glory, Rosenfeld shows that today wage gains have been decoupled from strikes. The numbers are simply too miniscule to make an impact on the economic landscape.

The across-the-board rise in inequality that stems from labor's decline also hides deeper racial and gender dimensions that Rosenfeld draws out. The disproportionate

representation of blacks in the union movement meant that deunionization hit this population especially hard, while also, obviously, increasing racial inequality. By the 1970s black male private sector union membership had hit nearly 40 percent, and black female union rates were double those of white women. Because union workers make more money and enjoy on-the-job protections and benefits packages, these statistics help illustrate how wage gaps between blacks and whites, especially between women, were mitigated during the times of higher union enrollments. Rosenfeld estimates that had unions retained their peak strength, wage differentials between black and white women in the private sector would be 10–30 percent lower.

As wage-setting institutions and political engines for worker empowerment, unions once exerted significant clout in American life. This book's analysis reveals the collapse of organized labor as a key determinant of our high levels of economic inequality today. However, Rosenfeld's singular focus on economic inequity unfortunately narrows the broad scope of his title, as unions *no longer do* many things aside from leveling the wage scale. Workers have always joined unions for reasons other than higher pay—to gain greater control over the labor process, to neutralize a bad boss, and to enjoy greater socio-cultural freedoms, all of which have something to do with social inequality, broadly conceived. Similarly, he makes only one passing reference to the Occupy Wall Street movements, which can be largely credited with raising the issue of inequality in a way that had not happened in decades. The on-the-ground conflicts between labor unions and those movements, the most recent and vocal contemporary proponents of economic redistribution, would have usefully complicated the otherwise simple equation of "more unions, more equality."

Furthermore, readers with an eye for history may object to his characterization of a "tripartite arrangement," a labor, business, and government nexus during the midcentury decades. This period is central to his argument because it is the high-water mark of wage compression and equality delivered through substantial union membership and a sympathetic state, and it highlights the

degree to which politics *matter* for union success. Although this is the common reading, there is significant debate on this issue that goes unassessed in his book. Was there really a tacit agreement that led to better working conditions? Or were unions simply better at fighting? Even during the exceptional post-war decades, Peter Evans (2010) suggests that effective labor organization deserves more credit than a friendly state for labor's success. C. Wright Mills' *The New Men of Power* (1948), to take a classic example, highlighted the deep skepticism trade union leaders held for corporate elites. Nelson Lichtenstein's more recent work forcefully argues against the existence of an alleged labor-management accord (see Lichtenstein 2002).

Government is not the answer?

But the most contentious point of Rosenfeld's book is not his analysis of what is wrong, but rather his critique of the only thing organized labor has going for it today—the public sector. Today over half of all union members are in the public sector, and they maintained their power during the period of private sector union decline. This has understandably given the outside impression of government unions as a protected oasis of middle-class and educated white-collar bureaucrats.

Ultimately, he concludes that “government is not the answer.” The primary basis for his objection is a concern with inequality. Because public sector workers are generally more highly educated, for example, unions become the guarantors of those with a college education, not blue-collar workers, which widens the gap between the advanced degrees and everyone else. Also, because public sector workers out-earn their private sector counterparts even without a union, unions lose their historic connection with the least well-off. Finally, the wage premium for joining a union in the public sector is very small, and so labor's historic mission of raising wages is thereby muted. In other words, he writes, “as unions concentrate in the public sector, their historical role representing those with comparatively low education and income levels is reduced” (p. 66).

Rosenfeld argues that the political impact of organizing within the public sector is

limited too, as new public-sector union members only increase their voter turnout rates by very small degrees, compared to private-sector union members, who start from a lower place of political engagement. The value added, in other words, is comparatively low for public-sector workers compared to those at private companies. Moreover, he says, the public-sector workforce has likely reached its capacity, with little room to grow. So while private-sector unions are almost gone, government employee unions have nowhere to go.

But we can employ Rosenfeld's own counterfactual methodology to see the importance of public-sector unions, especially as they concern the issues that matter most to his analysis. For example, while public-sector unions today may represent women at high rates, blacks, immigrants, and other ethnic minorities are vastly underrepresented, even though black workers are more likely to be employed in public jobs. A decline in public-sector unionism therefore would disproportionately impact black workers, exacerbating wage inequality by race. And though union rates are relatively high for public workers—about one-third of public employees are unionized—there are still great gains to be made there.

The political dimension is even more crucial. Public-sector workers are concentrated in highly contentious industries like health-care and education. Because they outnumber private-sector workers by about nine to one, it is unsurprising that the last two decades have seen union-busting efforts increasingly pointed toward public employees. Fiscal crises (real, imagined, or manufactured) have provided an excuse for local governments to sell off public infrastructure, cut wages and salaries for workers, decrease police and fire staff, and so on. Public-sector unions are often the first to fight these policies that impact all of society. The most recent attacks began in 2011 when workers, students, and other community members occupied the Wisconsin state capital to protest the shredding of labor rights for government employees. But the antiunion forces outlasted the events in Madison and spurred a broad coalition of conservative forces to introduce similar legislation in other states, with varying degrees of success.

What Unions No Longer Do was published before the most recent and devastating of these political fights was over: the Supreme Court's ruling in the *Harris vs. Quinn* case, perhaps the most anti-union piece of legislation from the bench in recent history. The specific question was whether or not publicly-funded home healthcare workers, who are covered through union collective bargaining contracts, can be compelled to pay union dues. Samuel Alito's decision, representing the cabal of conservative justices, declared the creation of a new "partial public employee," a worker in a state of legal limbo somewhere between the private and public sphere. Though the New York Times declared it a "partial victory" for labor, most people within the movement see it as far more dangerous: by essentially allowing the free-rider problem of right-to-work states to expand into the public sphere, the continued power of public-sector unionism hangs in question. Which brings us back to why public-sector unions are so important, in contrast to Rosenfeld's claims. The public sector is far from the comfortable, stable, and secure place he seems to suggest, and its unions are more necessary than ever. While public-sector unions are not *the* answer, no other single strategy alone is either.

Concluding Remarks

Rosenfeld's book appears at a time of intense debate about inequality in America, and his original analysis suggests that something

important has been left out—workers and their organizations. *What Unions No Longer Do* concludes without a principled call for unions to be more like social movements and without urging the working class toward increased militancy. Rosenfeld knows far too much about the problem for that bit of optimism. In fact, there is little in the book that is prescriptive at all. One thing unions once did that they no longer do, and that goes unmentioned in the book, was to inspire confidence in a grand alternative—to the power of business, to the ubiquity of the market—for a new kind of political regime and a better life. The construction of the economy and political society that Rosenfeld seems to pine for requires this belief. But his book is less concerned with the future. Instead, it offers a powerful counterfactual that should be of general interest in the social sciences and for anyone interested in how American society got where it is today. In that sense, it is at once a work of history, a tale of today, and a story of what might have been.

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