

What most distinguishes Igo's study apart is her effort to understand how the public responded to the use of inferential statistics to project mental constructs of a society using inferential statistics. She assesses the public's reaction to the various studies by following coverage in magazines and newspapers with a mostly middle class readership. She also digs into the correspondence directed to Kinsey, the Lynds, Gallup and Roper from interested citizens. The public was initially skeptical of the ability of survey research methods to gauge public sentiment on an array of topics. There was plenty to be skeptical about. Igo points to numerous shortcomings in the methodologies of all three studies. Pollsters discovered that interviewees had a disconcerting habit of responding differently to different interviewers. No one was attempting anything that approximated a random sample, a concept even the pollsters initially dismissed as a fad. The social scientists did not invite close inspection of their methodology and portrayed their enterprise as strictly empirical, value free, scientific inquiry. They disavowed any hidden agenda or political proclivities that might cloud their professional judgment; their task—like that of any election official—was to render an honest count. Gallup claimed that he did not even vote. To judge from the correspondence, the public's suspicions of the statistical analysis was not based on any sophisticated understanding of the pitfalls of sampling or questionnaire design; more commonly skeptics grounded their doubts on the simple fact that they were not surveyed. Nevertheless, Igo concludes, postwar Americans became resigned to the new authority invested in social scientists as national oracles giving voice to the people's will.

Cultural historians of the modern era and social scientists of many stripes will find much to admire in this insightful volume. Igo reminds us how deeply steeped social scientific inquiries are in contemporary social conventions and attitudes. She also outlines the overlooked role social scientists have played in shaping today's imagined communities, picking up where the census takers, map makers and newspaper publishers had left off during the century previous. In some particulars, I wonder if Igo's reach has exceeded her grasp. Gauging the public's reaction to these studies on the basis of correspondence directed to their authors obviously has its hazards. (Igo quotes from a number of letters the Kinsey Institute relegated to its "crank" file.) Yet, *The Averaged American* at least tells us what the public was told about these studies if not what it actually believed. If only we had a poll!

University of Texas, San Antonio

John F. Reynolds

Political Change and the Rise of Labour in Comparative Perspective. Britain and Sweden 1890–1920. By Mary Hilson (Lund, Sweden: Nordic Academic Press, 2006. 352 pp.).

Mary Hilson has written an in-depth study of the development of labor movements in two naval dockyard towns in Britain and Sweden during the late nineteenth and early twentieth centuries. Naval dockyard towns have long been

seen as particularly difficult territory for labor movements and have therefore been somewhat understudied by labor historians. Hilson hopes to remedy this lacunae somewhat by investigating how labor movements in Plymouth and Karlskrona understood, reacted to, and indeed ultimately overcame the challenges they faced in appealing to and organizing workers in these towns. Those interested in the peculiarities of dockyard towns, the intricacies of labor movement organizing, and the details of union and left-wing party development in Sweden and Britain will find much useful information in this volume.

But Hilson views her book as more than a study of two somewhat neglected towns in England and Sweden. *Political Change and the Rise of Labour in Comparative Perspective* also aims to answer broader questions in labor history as well as prove the value of comparative historical research. These two goals are linked as Hilson correctly notes that answering a cross-national question requires a cross-national approach.

Hilson's "broad question" is: "Why did social democratic parties emerge in all European countries in the late nineteenth century?" Regardless of levels of economic development, industrial profile, class structure, or political institutions, socialist parties appeared in all continental European nations at approximately the same time. Yet the tendency on the part of many labor historians is to treat countries as "exceptional." This is certainly true of the cases Hilson examines—Britain and Sweden. And of course, on some level they are (but what country isn't?). Britain was the earliest industrializer and possessed a political system of unusual stability and adaptability. That such factors shaped the development of its moderate, laborite labor movement seems beyond refute. Sweden, on the other hand, developed late, underwent greater and more tumultuous political changes, while remaining on the periphery of European political conflicts. That such factors shaped the development of its incredibly successful social democratic labor movement also seems beyond refute.

Yet if we focus merely on the dynamics of these (or any) particular cases, we will miss the forest for the trees. While the ideological coloring and success of European labor movements varied significantly, the fact remains that all arose at approximately the same time and under the influence of international socialism. If understanding labor movements' differences requires an examination of country and local level factors, understanding their broad similarities requires cross-national and structural analysis.

Hilson attempts to provide such a mixed study in her study of Plymouth and Karlskrona by focusing on many similar micro and macro level challenges labor movements in both places faced. (On the micro level the similarities came largely from both towns' dependence on shipbuilding—e.g. local industry's heavy dependence on state financing; somewhat isolated populations; particularly high resonance of imperialist rhetoric; and distinctive employment patterns—while on the macro level they came from international trends and events—e.g. political liberalization, economic development, and the First World War.) However, Hilson also pays attention to the important ways in which Britain and Sweden (and Plymouth and Karlskrona) diverged, and how these differences shaped the challenges and responses of labor movements in the two places. (Here, for example, there is much information about the distinctive role played by cooperative movements in Plymouth. On the other hand, I don't think

Hilson emphasizes enough the ways in which each country's political institutions shaped their labor movements. For example, in Britain the Labor party had to deal with fairly well-established liberal and conservative parties, while the Swedish SAP was the first modern, mass party in Sweden.)

That an explicit plea to combine cross-national and local research has to be made strikes a comparative political scientist as odd and troubling. While there is certainly a need for individual, country studies and local level analysis, there is simply no way a cross-national phenomenon like the rise and development of European labor movements can be understood without comparative, multi-level analysis. In the abstract, most labor historians recognize this, of course, but what Hilson seems to be calling for is more such work in practice. She also seems concerned that the trend toward ever more local analyses may have gone too far. While such studies have provided much more differentiated pictures of individual countries' political and social trajectories, this trend may also have contributed to an unnecessary narrowing of historians' focus. Hilson hopes *Political Change and the Rise of Labour in Comparative Perspective* will push the field back towards comparative, multi-level analysis and as a comparative political scientist interested in the history of the left I sincerely hope she succeeds.

Barnard College

Sheri Berman

War in Human Civilization. By Azar Gat (New York: Oxford University Press, 2006. xv plus 822 pp. \$35.00).

Azar Gat's newest book joins the recent scholarship attempting to explain the "riddle" of war. After writing books on military thought and the development of military strategy, Gat turns his eye toward the origins and fundamental nature of war itself. At the risk of boiling down a sophisticated argument too much, he agrees with Lawrence Keeley and others that war has been a fundamental aspect of human behavior almost since the appearance of humans. Gat contends that war has been a function of humanity's adaptive, evolutionary growth over time. In effect, humans have fought wars because winning wars has produced real, tangible gains (most notably access to food and sex for reproduction) for the victors that were worth the costs associated with it. The best summary of this thesis can be found in chapter five.

Gat begins his argument in pre-historic times and seems more comfortable when discussing the Greeks and Romans than when discussing the wars of the twentieth century. His chronology avoids the trap that some historians (including, I admit, myself) sometimes fall into of devoting disproportionate time to the more modern periods because of the much richer source bases. Thus halfway into the book, he has only just begun to discuss the development of sedentary civilization. This refreshing willingness to eschew the modern is a welcome facet of the book's structure.

When Gat moves on to the development of states, he argues that state formation did not increase war's frequency or its lethality. By contrast, states helped to