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The comparative historical sociology of W. E. B. DuBois

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At a time when comparative historical sociologists are discussing the relationship of our scholarship to projects of social reform (“can comparative historical sociology save the world?”), we can learn a great deal from one of our discipline’s founders, who was both a comparative historical sociologist of breathtaking erudition and a deeply engaged scholar whose work was always oriented towards social transformation. I mean W. E. B. DuBois. Thanks to Aldon Morris’s *The Scholar Denied* (2015) and maybe also thanks to Black Lives Matter, many American sociologists are discovering, or rediscovering, the sociological contributions of DuBois. Most of us probably already knew he was a pioneering theorist of race and an early urban sociologist. Morris makes a stronger case: not just that DuBois got there first, but that he influenced his contemporaries Max Weber and Robert Park, and thereby all of the rest of us who read Weber and Park. In a provocative review of this book, Julian Go (2016) has argued for restoring DuBois to a place of honor on our graduate syllabi.

I would like to make a case for putting some of his works on the comparative historical sociology syllabus in particular.

Putting some works by DuBois on the syllabus means reading them, of course, and if your graduate education was typical of graduate education in our discipline, that may mean reading them for the first time. The prospect is daunting: DuBois lived a long time and wrote a lot. His first book was published just three years after Durkheim’s *Division of Labor in Society* and his last book was published just three years before Tilly’s *Vendée*. For some years I have been slowly and with pleasure reading my way through this *oeuvre*, and I am only part way through. But I will briefly describe two of his works that, I have found, repay re-reading.[1] More than a decade ago, Zine Magubane (2005) argued for canonizing DuBois as a comparative historical sociologist; my purpose in this essay is to second that nomination, belatedly but enthusiastically, and to pile on even more reasons why his work ought to interest comparative historical sociologists today.

My exhibit A is *The Suppression of the African Slave-Trade to the United States of America, 1638-1870* (1986 [1896]). Is there such a thing as progress in moral standards? What does that progress have to do with economic development? Émile Durkheim posed these questions in his 1893 doctoral dissertation on *The Division of Labor in Society*, and three years later, W. E. B. DuBois answered them in his own dissertation on *The Suppression of the African Slave Trade*. DuBois didn’t address Durkheim’s work directly and probably hadn’t read it. But he described his dissertation as “a chapter of history which is of particular interest to the sociologist” (1986 [1896]: 193) and he used historical comparative methods to answer questions about which Durkheim merely speculated. DuBois used systematic comparison across regions and time periods to test the competing hypotheses that moral suasion, political pressure (including legislation and the threat of rebellion), or economic interests were responsible for the ultimate abolition of the transatlantic slave trade. He concludes that it was only the coincidence of all three of these forces, none more primary than the others, that led to the abolition of the trade. The book is among other things a pioneering study of social movement outcomes, and the argument is notable for its sophisticated treatment of conjunctural causation, for its recognition of contingency, and for its use of explicit counterfactual reasoning. (DuBois argued there was a missed chance to abolish the slave trade at the time of the American Revolution, and supported this argument with a comparison to the Civil War.)

How well does this book stand up? The sources and methods available to historians of the slave trade have improved dramatically in the 120 years since DuBois finished his dissertation, and we now know better on many points. Historical demographers will note that he misjudged the extent of smuggling in the mid-19th century because he overestimated deaths and underestimated births in the enslaved population. (This point is the subject of a truly great footnote in David Levering Lewis's biography of DuBois.) Still, as a work of scholarship, this book holds up rather better than Émile Durkheim's dissertation, which your colleagues still teach. Consider assigning this one as comparative historical sociology's answer to that one.

My exhibit B is *Black Reconstruction in America, 1860-1880* (1995 [1935]). This book uses comparative reasoning to argue that Reconstruction was a missed opportunity to achieve social democracy in America. The book also illustrates the power of unexpected historical analogy to force a rethinking of abstract concepts. DuBois argues that the Civil War was decided by a "general strike" of enslaved laborers who collectively walked off the job and thereby deprived the Confederacy of resources. He also argues that the Reconstruction governments amounted to a "dictatorship of the proletariat." These are deliberate conceptual provocations that draw our attention to unexpected similarities across time and space. The use of surprising—and debatable—analogy is one of the characteristic intellectual moves of comparative historical sociology, executed here to great effect.

Black Reconstruction also seems methodologically innovative in its use of biographical detail to support a sociological argument. DuBois devoted hundreds of pages to the lives and achievements of Black legislators in the South during Reconstruction. It is easy to misread this part of the book as nothing more than a vindication of a few great Black men. DuBois certainly was an elitist. But in this text, his is best understood as *methodological* elitism, in service of a sociological point: he takes the elite to be interesting because following people of unusual ability allows him to reveal the social limits on human achievement, in much the same way that glass ceilings only become visible when you climb high enough to bump into them. The book's use of personal biography to reveal both contingency (what if Lincoln hadn't been killed?) and structure (what stopped a talented politician like Hiram Revels from becoming another Lincoln?) exemplifies DuBois's distinctive approach to sociology as the scientific search for "the limits of chance in human conduct" (DuBois 2000 [1905]).

This book stands up surprisingly well. Maybe for this reason it is one of very few of DuBois's books still under copyright that is still in print in an inexpensive paperback edition. If you could only read one book about the history of Reconstruction, you would probably not want it to be *Black Reconstruction*, but instead Eric Foner's *Reconstruction: America's Unfinished Revolution, 1863-1877* (1988)—only because that book is, basically, an update of *Black Reconstruction*. But if you want a classic comparative-historical text that uses

Reconstruction to exemplify a sociological approach to war, revolution, the creation of labor markets, and the question of why there is no social democracy in America, you will want to choose DuBois.

Why don't more of us teach DuBois already? Sociology, like other fields of academic production, is organized around a distinction between pure and applied scholarship that is also a distinction between high-status and low-status positions. Twentieth-century comparative historical sociologists positioned themselves on the pure end of the spectrum. DuBois was impure. Morris (2015) argues that it was not only the racism of academic gatekeepers, but also their aversion to DuBois's activist orientation, that shut him out of the best libraries, grants, and graduate teaching opportunities. His long career of scholarship produced no dissertation advisees employed in departments of sociology. No surprise, then, that when a handful of early-career sociologists convened in 1979 to discuss the comparative historical scholarship of their own teachers—in the conference that led to the canon-making *Vision and Method in Historical Sociology* (1984)—they left out any mention of DuBois and his legacy. They weren't his students.

[DuBois] never claimed that comparative historical sociology could save the world, but he certainly thought that the world needed saving, and hoped his historical sociology could contribute something to that project. He did not save the world, but he changed it.

But we can still claim him as a teacher. Although the philosophical interpretation of DuBois is vigorously debated, I read him as a pragmatist, and the pragmatist case for putting DuBois on the syllabus does not rest on the historical significance of his works. Life is too short to read every book of historical significance. Instead, it rests on the *usefulness* of reading his works for our own collective projects of understanding and changing the world. He never claimed that comparative historical sociology could save the world, but he certainly thought that the world needed saving, and hoped his historical sociology could contribute something to that project. He did not save the world, but he changed it.

Indeed I sometimes think that we do not read DuBois because he was so effective at changing the world that his works now seem dated. He spent an extraordinary amount of scholarly effort marshalling comparative historical evidence to refute racist theories that are now long discredited. Some of the hundreds of pages he spent arguing with racists are, as they say, of merely historical interest. You can skip those parts, of course, just as you may have skipped a few pages of Marx's screeds against this or that forgettable left Hegelian. But as you skip these pages, reflect that it was DuBois who discredited those discredited theories. It would be ironic if he went unread just because he won the most important argument in the history of American sociology.

Endnotes

1. Not all of his works fall into this category: he spent much of his long career without the security of an academic appointment, he wrote quickly under conditions that were not always ideal for careful scholarship, he sometimes lacked access to the best or most recent sources, and he often deliberately mixed genres in ways that make his writings hard to classify or evaluate. The same was true of many of the greats, of course, and we preserve their reputations for greatness in part by reading them selectively and ignoring their weaker efforts. (Who now remembers *Herr Vogt*?) I propose that we extend the same respect to DuBois.

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How to do policy-oriented social science

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Sociologists and other social scientists can improve human well-being by asking useful questions and figuring out the correct answers. Some of these questions are "policy-oriented"; they are about the impact of policies and institutions. For example: Do social policies reduce poverty? What kind of healthcare system yields better health? Will reforming schools improve education? Do gun control regulations reduce crime? Do high taxes impede economic growth? How can government boost happiness?

Not too long ago, it was fairly common among sociologists to see policy-oriented research as best left to other disciplines such as economics, public policy, and political science. For sociology, in this view, policies and institutions ought to be dependent variables, the things we seek to explain.

My dissertation, written in the early 1990s, was a cross-country comparative analysis of the effects of economic cooperation, economic constraints, income inequality, government size, and union strength on outcomes such as economic growth, unemployment, and inflation. One prominent faculty member in my department, when I approached him about being on my PhD committee, responded that while the dissertation sounded interesting, because of the topic he would be able to write a job market letter for me only to policy schools, not to sociology departments.

This reflected a needlessly narrow notion of what sociologists ought to do. Thankfully, this conception has, for the most part, gone by the wayside. Policy-oriented analysis is now commonplace in our discipline, and as best I can tell, hardly anyone objects.

How should we do policy-oriented research? I favor embracing a multitude of analytical strategies. We can generate theories (hypotheses) or test them. We can analyze assorted units —individuals, groups, texts, rules, beliefs, countries, the world system, to name just a few. We can identify correlations or trace causal paths. We can gather and analyze data that are quantitative or qualitative. We can interview, observe as participants, mine historical archives, run experiments, crunch numbers, and more. Anything that enhances our understanding is, to my mind, a step forward.

Not everyone shares this view, however. Many comparative-historical sociologists work with "macro" data. Our units of analysis are countries, or other large geopolitical units such as regions or states. (For ease of exposition, I'll refer to countries from here on.) We engage in a type of analysis I call "macrocomparative" — we compare across countries and/or over time