One issue to look out for when addressing the history of the social sciences — and intellectual history more generally — is that scholars are apt to see themselves as in dialogue with the events about which they are writing. As with scientists writing about their own disciplinary past, there is a felt need either to credit the past as prologue, or to distance oneself from the folly of one’s predecessors. Such, of course, are the roots of whig history.

The implicit aim of a new whig history, which shapes much intellectual and social science historiography is, in broad strokes, to explain how anthropologists and their intellectual allies bested academic competitors, and can now lead society away from a myopic modernism toward a more harmonious, genuinely cosmopolitan future.

This narrative is fairly similar to the original Whig narrative diagnosed by Herbert Butterfield, which took history to progress away from authoritarianism to political, economic, and religious liberalism. However, the whiggishness of the present narrative can be difficult to acknowledge, because the phenomenon of whig history is actually incorporated within the narrative as an intellectual pathology arising from the same teleological modernism being cast as outdated. It is counterintuitive that the narrative could be whiggish, because whiggism is a declared enemy of the narrative.

An important feature of whiggism is that it is more of a narrative form than a concrete synthesis of events. Even the basic periodization of the narrative can be extraordinarily variable (what I have called “mobile periodization”). One of the most popular posts on this blog is Chris Donohue’s post...
on “environmental determinism” circa 1900. Chris argues that anthropologists have assembled a

canonical history of their discipline to show

how human geography emerged from the shadows of racial prejudice and theoretical
reductionism, much like the modern inquiry of social anthropology. [Ellsworth] Huntington as
well as Ellen Semple are critical components of the theodicy of human geography’s emergence as
a scientific discipline, underscored through the consistent invocation of [Franz] Boas’ disapproval
of Huntington’s methodology.

(Interestingly, although Chris’s post usually gets several hits a day from search engines, there are
peaks around the beginning of academic terms — we’re experiencing one right now. My guess is
that this happens when professors introduce new anthropology students to the bad old ideas, the
moral lessons of which are no doubt reinforced with a quiz or short essay assignment.)

Consistent with mobile periodization, the Cold War also marks an important point in this new whig
history of ideas, establishing a high-water mark of modernist ideology. Several features characterize
the ideas linked to this ideology: 1) they were scientistic or rationalistic, quixotically aspiring to the
explanatory (and perhaps even predictive) precision of the physical sciences; 2) accordingly, they
asserted to be fully objective and value-neutral, and thus normatively authoritative; 3) in fact (per
Mannheim’s paradox), they were reductionistic, borrowing the terms of their analysis from the
values of mid-century America; and 4) as such, and because they were close to institutions of power,
they were deeply implicated in failures of American policy, particularly (but not limited to) the evils
of the arms race, the Vietnam War, and intervention in the politics of “Third World” countries.

These last two points, at least, replicate the reductionism and Eurocentrism that present-day
anthropologists emphasize in the imperialism-friendly geography of Huntington and Semple. The
failures of Cold War policies, like imperialism before them, led to the discrediting of these sciences,
paving the way for the possible ascendancy of the alternative intellectual programs of our own time.

Ted Porter’s foreword to Cold War Social Sciences establishes the key points of a narrative in which
the “Cold War” can function as a useful analytical rubric. His version of this narrative begins with
the foundation of disciplianary social science in the 1880s and ‘90s, as studied particularly by
Dorothy Ross. According to Porter, late-nineteenth-century social science “was a project of the
Progressives, a movement of reform and rationalization with a wide popular audience” (x). What
distinguishes the Progressive-era social sciences from the Cold War social sciences is not that they
were political, but that they wore their politics on their sleeves. Although they took inspiration from “evolutionary thinkers such as Herbert Spencer and Charles Darwin” and from “philosophers such as John Stuart Mill and William James” (x), Porter is clear in his claim: “Few then aspired to a kind of knowledge that transcended human institutions. Many concerned themselves with the history of law or with the character and functioning of institutions, or with economic relationships of agriculture or labor” (xi).

According to Porter, in the mid-twentieth-century, both the immediate and general environment surrounding the social sciences changed markedly. Social scientists were offered sponsorship by “state agencies as well as large private patrons” with all the opportunities and compromises thereby entailed, and were also occasionally subjected to “anti-Communist witch hunts”. These forces did not “politicize the academy in a direct way,” but “tended rather to depoliticize it, adding one more incentive for scholars and university administrators to emphasize technical tools of science and to insist on its independence and detachment. Yet,” — here we have a classic post-Marxist invocation of Mannheim’s paradox — “this preoccupation with neutral objectivity can itself be seen as a form of politicization by virtue of its very claim to stand outside the value-laden character of the processes and interests that shaped the production and uses of social knowledge” (ix). Crucially, social scientists of this era disavowed their dependence on their political and cultural surroundings: “Those with professorial positions, in particular, rarely acknowledged what their methods and theories may have owed to the circumstances of their work” (xiii).

Consistent with my analysis of historians’ treatment of “category (1)” research directly supporting Cold War initiatives, and “category (2)” research, which was more academic but also received dedicated funding, Porter emphasizes the historical interdependency and practical conflation of the generality or fundamentality of knowledge claims and the intellectual authority that supported the instrumental uses of social scientific research (xii):

Quite a lot of social science, as we learn from the papers in this collection, really was supported for specific military purposes, but academic authors preferred not to believe that they were merely performing contract work. Indeed they were not, for what is most interesting here is precisely the wide overlap of what they called fundamental social research with more applied investigations. The leaders of social science had an explanation for this blurring of boundaries. It was, for them, almost axiomatic that the more abstract, theoretically or quantitatively rigorous form of science is inherently the more powerful. Academic research was properly placed atop the hierarchy of knowledge, they insisted, because it provided the theories and methods required to make practical investigations fruitful. This was a key feature of what the natural science ideal, or ‘physics envy,’ meant to them. They drew satisfaction from the thought that by liberating defense projects from too narrow a focus, they could advance the goals of foreign policy more effectively than the agencies could have done by focusing on specific pragmatic goals.

Important to Porter’s narrative, this mentality was a dominant, but fortunately also an ephemeral pathology, which erased self-consciousness from social scientific work. Latent heroes questioned it at the time, but it was only later that a new generation of scholars could claim to have rendered its
invisible prejudices visible, and so allowed its authority to be questioned, thus permitting the abandonment of its intellectual strictures and moral compromises (continuing on xii):

Yet in retrospect we can see that the shaping was reciprocal. Postwar social science itself was formed in important ways by its bureaucratic alliances with this military superpower in a world divided ideologically and diplomatically. Even in the late 1940s and 1950s, some social scientists dissented from military-related goals, but these people typically became disaffected with their disciplines as well.

By now, of course, this mentality has been totally eclipsed, at least in the humanities and humanities-like social sciences: “Few interpreters now are inclined to claim that everything fits together seamlessly, as in the structural-functionalism that the institution-builder and grand theorist Talcott Parsons and his allies found so satisfying” (xiv).

In Pt. 2 of this post, we will look in more detail at the critical role played by “modernization theory” in establishing the crucial narrative characteristics of Cold War social science, and possibly also Howard Brick’s much more detailed account of the escape of anthropology from the modernist trap in his essay in this volume, “Neo-Evolutionist Anthropology, the Cold War, and the Beginnings of the World Turn in U. S. Scholarship”.

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Comments

1. Patrick McCray - September 24, 2012
Will-
This gives me a lot to think about; I just got this book and haven’t yet had time to tuck into it. One of the points that comes to mind though, and maybe this isn’t exactly germane to your post, is the need to avoid a “Cold War determinism” when thinking about how the CW affected
research in the social sciences (and conversely). Not terribly brilliant in terms of an observation but something that always comes up with my students.
P.

**Reply**

**Will Thomas - September 29, 2012**

Hi Patrick — not only should one not be deterministic in one’s treatment of the Cold War’s impact on natural and social science (even proponents of the “Cold War” rubric like Solovey and Porter are careful to make this point), but Chris and I would urge that it is dangerous to apply the label “determinist” or “reductionist” or “scientistic” to past thought. It’s a little like (and no doubt descended from) labels like “mechanist” or “atheist” to describe strands of early modern thought. It’s more a label historical actors applied to abuse others’ thought than one that accurately describes the thought itself.

Interestingly, historians tend to be eager to undo the relegation of genres of the deeper past with which scientists are often uncomfortable (magic, alchemy, theology…) to categories of failed thought. The situation seems to be oddly the reverse with more recent thought, perhaps because such recovery still evokes an uncomfortable sympathy with things like the Cold War militarism or hard-core economic liberalism with which that thought is (sometimes fairly, sometimes unfairly) associated. The instinct seems to be to portray the thought as addled by hidden assumptions and ideologies, to “Otherize” it by portraying it as obsessively intent on normalizing a certain point of view, and “Otherizing” alternatives (an irony or paradox, related to the point about whiggishness above). Alex Wellerstein and I debated this issue a bit in this post; see also his post at Restricted Data.

There is, though, an increasing, if diffuse effort to recover an alternative picture of the goals and logic of past inquiry. The trick, as ever, will be to develop a synthetic picture. Interestingly, I just found out yesterday that Richard Staley is now working on the relations between physics and anthropology — Boas and Malinowski both trained in physics, and Mach is a key player — which he claims will revise our understanding of not only anthropology circa 1900, but physics as well. (He discusses this a bit on his faculty page).

**Reply**
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