ECONOMICS AND THE IDEA OF TWO CULTURES IN THE POSTWAR AMERICAN HUMAN SCIENCES

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ABSTRACT

Current wisdom has it that the Cold War decades witnessed the emergence of two cultures within the American human sciences: one rooted in rational choice models (and hence mathematical and positivistic) and the other based on the key notion of interpretive understanding (and thus historicist and reflexive). Economics is generally seen as the linchpin of the first culture, whilst Wittgensteinian currents in philosophy and anthropology are said to define the second. In this paper, I seek to nuance this dichotomous perspective on the American human sciences. My first move is to reject the notion that the human science disciplines must be parsed according to epistemological categories. In the remainder of the essay, I articulate an alternative way of construing the similarities and differences between the various human sciences. My general argument is that an emphasis on subcultural practices—and, especially, on the ideologies involved in those practices—allows for new and fruitful contrasts to be made between economics and its disciplinary cousins.

INTRODUCTION

What sort of history can we write about American economics and its relations with cognate disciplines during the transformative middle decades of the twentieth century? According to what criteria should we periodize and categorize the Cold War human sciences? Any attempt to place economics within the broader history of the social and human sciences in the postwar United States must face these questions. This essay explores the general problem of how to frame an historical interpretation of the American human sciences. It offers some proposals regarding what we should look for when comparing the postwar careers of individual disciplines and research programs. In particular, I seek to carry out two related tasks. First, I call into question a common approach to the history of the American human sciences. This is my concern in section I. My second goal is to outline an alternative view. This I undertake in sections II, III, and IV.

The approach I want to cast into doubt revolves around the provision of epistemological "maps" of the human sciences. Let me explain my use of this cartographic metaphor. In his revisionist account of modern microphysics, *Image and Logic*, Peter Galison dissents from two influential periodizing models in the history of science. The first is the "entity map" in which historical narratives are organized around the shifting objects of physical science: the atom, the quark, and so on. The second strategy is to draw "theory maps." In these histories, it is the fortunes of the great theoretical traditions—gravitation theory, quantum theory, etc.—that determine the shape of the story. Galison's principle criticism of such maps is that they reduce the complex and multilayered practices of modern physics to a single index: objects of knowledge or

the changing structures of theory. The quotidian practices through which professional, capital-intensive, physics is actually carried on are left out of the picture.[1] I believe we encounter a similar problem in the history of the human sciences. But here it is not entity or theory maps that predominate but what I call epistemic maps: maps that relate the fortunes of the human sciences to their epistemological self-understandings and methodological procedures. Narratives of this kind assume that we grasp historical developments in economics, sociology, anthropology, etc., when we have identified the models of knowledge-making they seek to emulate. Science or humanism? Explanation or edification? Measurement or interpretation? The human sciences are then mapped according to which of these alternatives has been most central to its constituent disciplines.[2] If, as is often the case, a human science discipline is said to have sought the status of natural or mathematical science, our epistemic maps assume that "positivism," "naturalism," "scientism," and "formalism" are the best rubrics for our historical interpretations. However, given the enduring presence of the humanist model, epistemic maps often counterpose certain "epistemological others." Historicism, hermeneutics, interpretivism, and critical theory are frequently ranged against the varieties of positivism.[3]

[1] Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997), 10-11.
[2] See, for example, Wolf Lepenies, *Between Science and Literature: The Rise of Sociology*, trans. R. J. Hollingdale (Cambridge: Cambridge University Press, 1988); Richard J. Bernstein, *The Restructuring of Social and Political Theory* (Oxford: Blackwell, 1976); Martin Hollis, *Models of Man: Philosophical Thoughts on Social Action* (New York: Cambridge University Press, 1977); Bruce Mazlish, *The Uncertain Sciences* (New Haven, CT: Yale University Press, 1998). For further discussion and references, see section I below.
[3] For an historical articulation of these themes in American social thought, see James T. Kloppenberg, "Pragmatism: An Old Name for Some New Ways of Thinking?" *Journal of American History* 83 (June 1996): 100-38.

The resulting image is of a Great Epistemological Divide. Prevailing understandings of the postwar American human sciences are a case in point. Current wisdom has it that the Cold War decades witnessed the emergence of two cultures within these disciplines: one rooted in rational choice and systems-theoretic models (and hence mathematical and positivistic); the other based on the key notion of interpretive understanding (and thus historicist and reflexive). Economics is generally seen as the lynchpin of the first culture, whilst Wittgensteinian currents in philosophy and anthropology are said to underpin the second.[4] I take issue with this picture of a Great Divide. In particular, I reject the assumption that our maps of the human sciences should be drawn according to epistemological categories.

Epistemic maps peremptorily relate the fortunes of the human sciences to the models through which they seek knowledge. Not only does this tend to polarize the human sciences into the camps of "positivism" and "nonpositivism." It also obscures the vital if messy domain of experimental and theoretical practices. In what follows I argue that, if we want to understand how and why the postwar American human sciences evolved in the way that they did, we need to get away from the "science or humanism" framework. We need to look instead at what sorts of practices research programs in the human sciences have encouraged among their practitioners. In adopting this line I am following recent studies of theoretical and experimental "subcultures" in both the history of science and of the humanities. As I try to demonstrate in sections III and IV, the peculiar situation of the human sciences leads us to extend this subcultural perspective

[4] See Clifford Geertz, "Blurred Genres: The Refiguration of Social Thought," in idem, *Local Knowledge: Further Essays in Interpretive Anthropology*, new ed. (New York: Basic Books, 2000), 19-35. For an updated version, with postmodernism instead of Wittgenstein representing the antipositivist alternative, see Peter A. Hall, "The Dilemmas of Contemporary Social Science," 34 (Fall 2007): 121-41.

into a repertory of ideological strategies, or what I call "embedding techniques." The subcultural, practice-oriented approach nonetheless provides a richer and, I hope, more illuminating framework for narrativizing the postwar American human sciences.

I. THE PROBLEM WITH EPISTEMIC MAPS

Epistemology is a hard habit to kick. Alert as we are to the shibboleths of the "mirror of nature" metaphor of mind, historians and philosophers remain attached to epistemology-centred accounts of academic disciplines.[5] To be sure, practising scientists often have strong views about how their concepts and classifications relate to the phenomena they describe. Different understandings of what it is to be "objective" have played a central role in the natural and human sciences since the Enlightenment.[6] The problems come when the historian tries to develop epistemological and methodological standards against which to judge the evolution of the human sciences. It is precisely this endeavour that leads us into to the idea of a Great Divide.

To fix our ideas, I shall take up a recent example of this historiographical strategy. In *The Politics of Method in the Human Sciences*, an encyclopaedic volume on "the vicissitudes of positivism and its epistemological others in the contemporary human sciences," George Steinmetz states that the goal of the collection is "to provide a mapping of the…human sciences from their explicit and especially their *implicit* epistemologies, asking about the differences and similarities among and within these disciplines' epistemological cultures." "Only by making the epistemological stakes and disputes explicit," Steinmetz contends, "will it become possible to heed the call to 'open

[5] Richard Rorty, *Philosophy and the Mirror of Nature* (Oxford: Blackwell, 1980).
[6] Lorraine Daston and Peter Galison, *Objectivity* (Cambridge, MA: Zone Books, 2007).

the social sciences'." The epistemological stakes in question turn on the claims of positive science versus those of critical, reflexive knowledge.[7]

The remark about "implicit" epistemologies already flags up the problems that Steinmetz and his fellow contributors encounter. Steinmetz is aware that positivism does not always well describe the epistemological and methodological self-understandings of the human scientists he is discussing. "Positivism" has long been an invidious term, as Raymond Williams noted many years ago.[8] At the same time, however, Steinmetz is committed to drawing an epistemic map. So he posits a tradition or spectrum of approaches he calls "social scientific positivism." But because ideas about knowledge and method have ebbed and flowed during the course of the twentieth century, Steinmetz is forced to appeal to "implicit" forms of positivism to rescue his project. This hermeneutic liberty comes at a price. On the one hand, Steinmetz continually has to hedge against anachronism. Qualifications on this score come thick and fast. "There is," he tells us, "a great deal of variation across the disciplines and historical epochs in the forms of positivism and in the waxing and waning of positivist dominance." Positivism, he notes elsewhere, is "a core cluster of ideas that resurface periodically under different names and in varying guises." Perhaps it is a useful term, Steinmetz speculates, because it is a "bad other," a "folk category" among social scientists on "the epistemological left."[9]

On the other hand, Steinmetz seems aware that the concept of positivism will collapse into incoherence if it is so generally and negatively defined. He is compelled to

[7] George Steinmetz, "Introduction: Positivism and Its Others in the Social Sciences," in *The Politics of Method in the Human Sciences: Positivism and Its Epistemological Others*, ed. George Steinmetz (Durham, NC: Duke University Press, 2005), 1.
[8] Raymond Williams, *Keywords: A Vocabulary of Culture and Society* (London: Fontana, 1988), 239.
[9] Steinmetz, "Introduction," 3-4, 31, 30.

lay down a set of criteria for diagnosing cases of positivism in the human sciences. This induces from Steinmetz a series of definitions. At one stage, the presence of positivism is identified with "an emphasis on general, and usually empirical, laws," "doctrines of falsification or prediction," "a spontaneous preference for 'parsimonious' explanations…or for mathematical and statistical models," and, finally, "adherence to a caricatured view of the natural sciences as role model." That analytical net would seem to capture both too much (surely every research tradition would allow that its claims are refutable or falsifiable in some way) and too little (the identification of a "caricatured" view of science seems too pejorative and subjective to be a criterion of positivism). This free-ranging formulation is soon supplemented by a composite definition of positivism drawn from selective readings of Hume and Carnap. "The common denominators of all positivist positions," Steinmetz concludes, "are…*regularity determinism* [universal laws of causal invariance] and *system closure* [the ontological position that all possible states of affairs in the world are covered by causal laws]." Contemporary positivism is above all characterized by "depth-realism," according to which unobservable theoretical entities may nonetheless be the causes of observable events. Armed with this jumble of definitions, we can ask historical questions like "Which contemporary epistemological positions can usefully be designated as (neo, or neo-neo) positivist, and how do they resemble and differ from the positivism of previous eras?" Or: "Are alternative terms like 'objectivism', 'theoretical realism', 'deductivism', 'instrumentalism', or 'mitigated positivism' better or more precise descriptions of the social scientific practices under discussion?"[10]

[10] Ibid., 30-35.

I think this game is not worth the candle. If a historian finds herself asking whether the epistemic orientation of, say, William Riker is neo-neo-positivist or theoretical-realist, it is time she rethought her assumptions. For this is not a historical question, but a matter of labelling. Consider, indeed, just such a moment in Emily Hauptman's excellent study of postwar political science. Reflecting on the definitions of Steinmetz and other contributors, she notes:

> Although Lawson, Steinmetz, and Somers all believe rational choice theory (or its close relative, neoclassical economics) has some debt to positivism, they differ in their assessments of that debt. For Lawson, although mainstream economics is informed by Humean positivism, this influence is much less significant than the discipline's debt to deductivism and mathematical formalism. Somers agrees that a nonpositivist theoretical realism is primary in much of rational choice theory, but argues that some of these theorists affirm positivist claims that are glaringly inconsistent with their realist commitments. By contrast, Steinmetz develops his definition of "methodological positivism" in acknowledgement of the partial and often inconsistent ways that contemporary social scientists affirm positivist views. It is quite possible, then, that Lawson would not wish to call rational choice theory positivist, whereas Somers would call it partially so and Steinmetz would deem it an example of methodological positivism.

Wisely, Hauptman demurs from "asking baldly, Is rational choice positivistic or not?"[11] The desire to choose between any one of the ideal types on offer seems like the driest kind of scholasticism. It is hard to see what historical insight can be derived from reducing methodological diversity to variants of a single epistemological model. The downside is not only terminological confusion of the sort Hauptman highlights; it is the flattening out of the historical landscape into two great continents: positivism and its

[11] Emily Hauptman, "Defining 'Theory' in Postwar Political Science," in *The Politics of Method in the Human Sciences*, 220.

"others." Alas, this strategy has a surprisingly long pedigree, both in American intellectual history and in philosophy.

The immensely influential philosophical writings of Charles Taylor are a major source of such views. In a series of classic essays published during the 1970s and early 1980s, Taylor set out the antipostivist case in the human sciences. He acknowledged that the mechanistic view of the universe—articulated first and most powerfully in the New Science of the seventeenth-century—had proven its value in the understanding and manipulation of the natural world.[12] The philosophical problem for Taylor was that natural science after the Scientific Revolution could evidently not be left in its place, as a useful tool for coping with nature. Instead, it became suffused with a profound *moral* significance in the West. We moderns, it seems, became enamoured of the idea that human freedom was won by objectifying our world, including our own selves, just as Descartes or Newton had secured for the modern subject a measure of control over nature by adopting the mechanistic view. An "ideal of disengagement," Taylor argued, worked its way into "the modern identity" from this original, exemplary image of human capacity.[13] Nowhere, for Taylor, was this natural science-guided view of human agency so powerfully and disastrously expressed as among twentieth-century practitioners of the human sciences. Taylor insisted that all purportedly naturalistic and value-free accounts of the human world partook of the seventeenth-century disavowal of "subjective properties": those aspects of experience and social action explicable only in terms of the

[12] See Charles Taylor, *Human Agency and Language: Philosophical Papers 1* (Cambridge: Cambridge University Press, 1985); idem., *Philosophy and the Human Sciences: Philosophical Papers 2* (Cambridge: Cambridge University Press, 1985).

[13] Charles Taylor, "Introduction," in *Human Agency and Language*, 5. For a sceptical reaction to Taylor's historical remarks on the making of the modern identity, see Quentin Skinner, "Who Are 'We'? Ambiguities of the Modern Self," *Inquiry* 34 (1991): 133-53; and, for a restatement, idem., "Modernity and Disenchantment: Some Historical Reflections," in *Philosophy in the Age of Pluralism: The Philosophy of Charles Taylor in Question*, ed. James Tully (Cambridge: Cambridge University Press, 1994), 37-48.

evaluations of a human agent.[14] Just as Steinmetz finds positivism and its variants around every corner, Taylor found naturalism everywhere in the humanistic disciplines: in the precepts of behaviorist psychology and its cognitivist rivals; in "designative" theories of meaning running from Locke to the truth-conditional semantics of Donald Davidson; in behavioral political science and in Rawlsian political philosophy.[15] Against these naturalistic models, Taylor argued that the human sciences were methodologically and epistemologically unique. They required interpretive understanding, and hence stood closer to the literary and hermeneutic disciplines than to the physical sciences.

Taylor has been widely assailed for this dichotomizing analysis.[16] My major point here is simply that Taylor offers further evidence of the polarizing and reductive effects of mapping the human sciences according to epistemic models. So powerful is the attraction to such models, however, that they have come to define much of the historiography of the American human sciences. The central histories of the origins of the social sciences in the United State foreground the desire among the first academic practitioners of sociology, economics, political science, and psychology to acquire the status and cultural authority of natural science. From the late nineteenth century to the

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[14] Charles Taylor, *Sources of the Self: The Making of the Modern Identity* (Cambridge, MA: Harvard University Press, 1989).

[15] See Charles Taylor, *The Explanation of Behaviour* (London: Routledge & Kegan Paul, 1964); idem., "Cognitive Psychology," "Language and Human Nature," and "Theories of Meaning" in *Human Agency and Language*, 187-212, 215-92; "Neutrality in Political Science," "Atomism," and "The Nature and Scope of Distributive Justice," in *Philosophy and the Human Sciences*, 58-90, 187-210, 289-317. Taylor's critique of these projects as part of a broader attack on reductionism in the human sciences formulated by the Ford Foundation-sponsored Study Group on the Unity of Knowledge. For the fruits of their collective deliberations, see Marjorie Grene, ed., *Interpretations of Life and Mind: Essays Around the Problem of Reduction* (London: Routledge and Kegan Paul, 1971).

[16] Quentin Skinner, "Who Are 'We'? Ambiguities of the Modern Self," *Inquiry* 34 (1991): 133-53; Thomas Kuhn, "The Natural and the Human Sciences," in idem, *The Road Since Structure: Philosophical Essays, 1979-1993, with an Autobiographical Interview* (Chicago: University of Chicago Press, 2000) 216-23; Clifford Geertz, "The Strange Estrangement: Charles Taylor and the Natural Sciences," in *Available Light: Anthropological Reflections on Philosophical Topics* (Princeton, NJ: Princeton University Press, 2000), 143-59.

interwar decades, it has been argued, social scientists claimed expertise and professional status by emphasizing the rigorously empirical, quantitative, and objective status of their research findings. Eager to cross the threshold from good works to scientific vocation, university practitioners of the social disciplines placed their faith in measurement and instrumentation, treating the accumulation of statistics and observational data as the cardinal scientific virtues. Alarmed by the benighted conditions of a rapidly industrializing, urbanizing, and stratifying society, whilst at the same time anxious to establish their own cultural authority, American social scientists from Lester Frank Ward to Wesley Clair Mitchell grasped the scientific gospels of their day—Spencer's evolutionism, Pearson's statistical reasoning, human ecology—and rigorously naturalized an increasingly messy socio-historical reality. America's encounter with modernity was made the object of empirical laws and transhistorical norms.[17]

It is not that such accounts are straightforwardly wrong. The motivations they reveal behind the project to make social science into a natural science are rich and diverse: the crisis of American exceptionalism, a burgeoning secular culture, and the desire for professional standing are just a few of the factors invoked for explain the attraction toward naturalism among American social scientists. The faulty assumption is rather that epistemological commitments provide the index according to which changes within, and affinities among, particular disciplines can be tracked. Instead of "positivism," the preferred terms for scientifically informed research programs in these

[17] Dorothy Ross, *The Origins of American Social Science* (Cambridge: Cambridge University Press, 1991); Thomas Haskell, *The Emergence of Professional Social Science: The American Social Science Association and the Nineteenth-Century Crisis of Authority*, new ed. (Baltimore, MD: Johns Hopkins University Press, 2000); Mary O. Furner *Advocacy and Objectivity: A Crisis in the Professionalization of American Social Science* (Lexington: University of Kentucky Press, 1975); Robert C. Bannister, *Sociology and Scientism: The American Quest for Objectivity, 1880-1940* (Chapel Hill: University of North Carolina Press, 1987); Mark C. Smith, *Social Science in the Crucible: The American Debate Over Objectivity and Purpose, 1918-1941* (Durham, NC: Duke University Press, 1994), 13-48.

accounts are "objectivism," "scientism," and "empiricism."[18] But, just as with the concept of positivism, the historical complexity of the human sciences induces a series of supplements and qualifications. For example, we find "modernist empiricists" in mid-twentieth-century political science[19]; across the American social sciences, we are informed, scientism shifted after World War I toward an "engineering" approach that differed from earlier forms.[20] Once again, terminological supplements are put in place to preserve the application of the epistemological framework. And once again, this patchwork system of categories is intended to sort the positivist sheep from the interpretive goats: Dorothy Ross has explicitly outlined a back and forth between scientism and historicism in the history of American social science.[21] In epistemic maps, the Great Divide continually reasserts itself.

The claim I now want to defend is that our first question about the human sciences should not be "Which model of knowledge are the human sciences committed to?" Rather, we should ask about the subcultural activities that constitute different disciplines and research traditions. Questions of this form lead to answers that encourage less dichotomous historical mappings of the human sciences.

II. KNOWLEDGE AS SUBCULTURE

[18] See, e.g., Ross, *Origins*; Bannister, *Sociology and Scientism*; Peter Novick, *That Noble Dream: The 'Objectivity Question' and the American Historical Profession* (Cambridge: Cambridge University Press, 1988); Robert Adcock, Mark Bevir, and Shannon C. Stimson, *Modern Political Science: Anglo-American Exchanges Since 1880* (Princeton: Princeton University Press, 2007).
[19] Mark Bevir, "Political Studies as Narrative and Science, 1880-2000," *Political Studies* 54 (October 2006): 583-606.
[20] Dorothy Ross, "Changing Contours of the Social Science Disciplines" in *The Cambridge History of Science*, Vol. 7: *The Modern Social Sciences*, ed. Theodore M. Porter and Dorothy Ross (Cambridge: Cambridge University Press, 2003), 205-37.
[21] Dorothy Ross, "A Historian's View of American Social Science," in *Scientific Authority & Twentieth Century America*, ed. Ronald G. Walters (Baltimore, MD: Johns Hopkins University Press, 1997), 32-49.

The course I have charted so far parallels the one taken by Peter Galison in *Image and Logic*. Just as, for Galison, entity maps and theory maps reduce the historical understanding of physics to a set of dichotomies about theory and observation, so in the history of the human sciences I have argued that epistemic maps impose brute and unwieldy distinctions—various kinds of positivism on one side, and assorted nonpositivisms on the other. In the remainder of this essay, I want to sketch out an alternative to epistemic maps that is similar in spirit to the new picture of physics offered in *Image and Logic*.

Galison's key insight is that in the modern physics profession, theorists, experimentalists, and laboratory engineers follow distinct "ways of life." This idea could already be glimpsed in Thomas Kuhn's notion of the "disciplinary matrix," which laid particular emphasis upon the concrete models and practices that bound scientific communities together around a common theory.[22] Clifford Geertz, meanwhile, issued a call in the early 1980s for an "ethnography of modern thought" in which the pursuit of pure mathematics or string theory was to be viewed as a culturally defined way of being-in-the-world.[23] But Galison has pushed this idea furthest. In the modern physics, theorists, experimentalists, and technicians have created their own journals, graduate programs, conferences, and institutional bases. Employing different understandings of theory, instrumental techniques, and standards of argumentation, these "subcultures" evolve at varying speeds and experience "revolutionary" conceptual breaks at different

[22] Thomas Kuhn, "Second Thoughts on Paradigms" in idem, *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977), 293-319; idem, *The Structure of Scientific Revolutions*, 3rd ed. (Chicago: University of Chicago Press, 1996), 182-87.
[23] Clifford Geertz, "The Way We Think Now: Toward an Ethnography of Modern Thought," in idem, *Local Knowledge: Further Essays in Interpretive Anthropology*, new ed. (New York: Basic Books, 2000), 156-63.

times. To suppose this variety could be reduced to a single story about the history of the atom or evolution of relativity theory is to miss the historical complexity of modern physics.

In recent years, the return to the study of scientific theories has entailed the extension of this subcultural vision. Historians of science such as Andrew Warwick, Ursula Klein, Andrew Pickering, and David Kaiser have insisted that theories should be conceived not as systems of propositions or perceptual gestalts, but precisely as forms of life.[24] These historians have shown that the appropriate use of mathematical formulae and related theoretical tools cannot be mechanically stipulated: such capacities rest on culturally structured forms of expertise. Already in *The Structure of Scientific Revolutions*, Kuhn had attacked the notion that scientific knowledge involved memorizing a body of formal laws and the algorithmic rules for their application. Rather, trainee scientists learned to perform certain model puzzle solutions and then, with the appropriate examples, extend that model to new problems in the field.[25] Drawing on the work of Michel Foucault, Warwick and Kaiser have attempted to flesh out Kuhn's pedagogical theory of knowledge. The "disciplinary regimes" of schools and universities, they suggest, operate on the bodies and dispositions of scientists-in-training; supervised drills, classroom demonstrations, and graded levels of examination are designed to shape the bodily movements and self-comportment of the student so that they

[24] Andrew Warwick, "Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell and Einstein's Relativity 1905-1911: Part I: The Uses of Theory," *Studies in the History and Philosophy of Science* 23 (1992): 632; Kaiser, *Drawing Theories Apart*, 8. For Klein's work, see Ursula Klein, "Paper Tools in Experimental Cultures," *Studies in the History and Philosophy of Science* 32 (2001): 265-302; idem, "Techniques of Modelling and Paper-Tools in Classical Chemistry," in *Models as Mediators: Perspectives on Natural and Social Science* (Cambridge: Cambridge University Press, 1999), 146-67. See also Andrew Pickering and Adam Stephanides, "Constructing Quarternions: On the Analysis of Conceptual Practice," *Science as Practice and Culture*, ed. Andrew Pickering (Chicago: University of Chicago Press, 1992), 139-67.

[25] Kuhn, *Structure*, 187-91.

can become the subjects of certain forms of scientific knowledge. Such top-down mechanisms of power, Warwick and Kaiser insist, are intertwined with "complex regimes of self-discipline" in which the student acts to constitute themselves as the recognizable subject of a particular scientific truth. These internal and external mechanisms are the means by which a theoretical subculture generates its own particular styles of thinking and skills.[26]

One this reckoning, a theory brings a subculture into being because it mandates specific transformative exercises upon the subject of the knowledge—that is to say, upon the scientist-in-training. In seizing upon these aspects of conceptual practice, historians of science find themselves—to date unwittingly—making common cause with an emergent history of "theory" in the twentieth-century humanities.[27] Ian Hunter has decried the tendency, in existing accounts of the historicity of philosophy and theory, to reduce historical understanding to "quasi-transcendental structures." These "structures" include the "paradigm, problematic, *Weltanschauung*, and discourse."[28] Hunter's response is to offer "falsifiable accounts of theory in terms of particular arts of reasoning, modes of cultivation, pedagogical routines, and so on."[29] Like Warwick and Kaiser, Hunter eschews the notion that theories must be studied as bodies of formal propositions. Inspired by the historian of philosophy Pierre Hadot and the later Foucault, he conceives of theoretical texts as encoding sets of "spiritual exercises" that a subject performs on his

[26] Andrew Warwick and David Kaiser, "Kuhn, Foucault, and the Power of Pedagogy," in *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives*, ed. David Kaiser (Cambridge, MA: MIT Press, 2005), 393-409.
[27] Recent retrospectives on the "moment" of theory in the humanities include Terry Eagleton, *After Theory* (London: Allen Lane, 2003); Jonathan Culler, *The Literary in Theory* (Stanford: Stanford University Press, 2007); Timothy Brennan, *Wars of Position: The Cultural Politics of Left and Right* (New York: Columbia University Press, 2006).
[28] Ian Hunter, "The History of Philosophy and the Persona of the Philosopher," *Modern Intellectual History* 4 (November 2007): 574.
[29] Hunter, "Time of Theory," 6.

or herself in order to embody a certain kind of knowing subject. The practice of theory in the humanities represents a "technology of the self." The theories and philosophies confronted by the intellectual historian are thus "not serial expressions of a universal human desire for knowledge and understanding, but regimes for inducing such a desire in those whom circumstance or chance have selected to cultivate a philosophical persona."[30]

In both the history of science and of philosophy, then, epistemic maps are taking a back seat. Historical attention has turned to the practical processes whereby certain kinds of subjects of knowledge are formed in local institutional and cultural contexts. The usual circuit of intellectual production is reversed, from historical agents who make knowledge about the world to knowledge that mandates the creation of certain kinds of people and intellectual practices. I now want to show how this subcultural perspective can be employed in the history of the human sciences. I shall do so by addressing the history of the postwar American human sciences.

The maelstrom of problem-oriented, interdisciplinary research during World War II plunged American social scientists into a new world of concepts, methods, and research practices.[31] The postwar intellectual landscape was scarcely recognizable from that of the interwar years. Overlapping constellations of theory were emerging everywhere; distinctive theoretical subcultures began to form both between and within the human science disciplines. One prominent network was that of the "cyborg" or systems sciences, encompassing game theory, cybernetics, operations research,

[30] Hunter, "The History of Philosophy": 587.
[31] On the war years as a watershed in American scientific traditions, see Peter Galison, "The Americanization of Unity," *Daedalus* 127 (Winter 1998): 45-71.

information theory and early computer science.[32] If, as Philip Mirowski argues, economics became a cyborg science in the 1940s, so too sociology, political science, psychology, and urban studies had a stake in the systems approach.[33] Standing close to the systems sciences was rational choice theory. With its origins in economic decision theory and the mathematical logic of relations, the rational choice research program blurred into the cyborg sciences. But its associations with political theory, political science, public policy, and even Rawlsian political philosophy prevent it from being assimilated entirely to systems research.[34] Lying athwart these ambiguously imbricated traditions of theory is "behavioral science"—a phrase on the lips of many social scientists and foundation officers after World War II. Pioneers like James Grier Miller demanded their own "general theory" for the behavioral sciences that drew on psychology, psychiatry, functional sociology, and quantitative methods, as well as the systems approach.[35]

A connected, but again distinct, endeavor was modernization theory. Incubated in the hothouse of Talcott Parsons' interdisciplinary Department of Social Relations

[32] Peter Galison, "The Ontology of the Enemy: Norbert Wiener and the Cybernetic Vision," *Critical Inquiry* 21 (Autumn 1994): 228-66.
[33] Philip Mirowski, *Machine Dreams: Economics Becomes a Cyborg Science* (Cambridge: Cambridge University Press, 2002); Crowther-Heyck, *Herbert A. Simon*; Jennifer Light, "Taking Games Seriously," *Technology and Culture* 49 (April 2008): 347-75; Sharon Ghamari-Tabrizi, *The Worlds of Herman Kahn: The Intuitive Science of Thermonuclear War* (Cambridge, MA: Harvard University Press, 2005); Agatha Hughes and Thomas Hughes, eds., *Systems, Experts, and Computers: The Systems Approach in Management and Engineering, World War II and After* (Cambridge, MA: MIT Press, 2000); Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Postwar America* (Cambridge, MA: MIT Press, 1996).
[34] S. M. Amadae, *Rationalizing Capitalist Democracy: The Cold War Origins of Rational Choice Liberalism* (Chicago: University of Chicago Press, 2003); Emily Hauptman, *Putting Choice Before Democracy: A Critique of Rational Choice Theory* (Albany: State University of New York Press, 1996); Hall, "Dilemmas of Contemporary Social Science."
[35] James Grier Miller, "Toward a General Theory for the Behavioral Sciences," in *The State of the Social Sciences*, ed. Leonard D. White (Chicago, University of Chicago Press, 1956), 29-65; Bernard Berelson, *The Behavioral Sciences Today* (New York: Basic Books, 1963); "Crowther-Heyck, "Patrons of the Revolution."

modernization theory put down roots in Chicago and at MIT, drawing into its ambit economists, area studies specialists, anthropologists, political scientists, social psychologists, and sociologists. Modernizationists studied political, economic, and social systems, and they drew deeply from the well of behavioral science. But, as Nils Gilman has shown, their ideas trace a different genealogy from that of the cyborg sciences, rational choice theory, or behavioral science.[36] Robert Redfield does not stand with Vilfredo Pareto, Norbert Wiener, and Adam Smith as the founding father of any single human science; but each finds their place in historical accounts of the ramified strands of postwar social science.

The cyborg, modernizationist, rational choice, and behavioralist traditions of inquiry are ripe for subcultural interpretation. Much of Philip Mirowski's *Machine Dreams: How Economics Became a Cyborg Science* is given over to detailed accounts of the research culture at RAND (in operations research) and the Cowles Commission (as it moved from quantitative economic analysis to embrace a panoply of systems models).[37] Like Galison, Mirowski is interested in locally formed theoretical practices, each with their own favoured tools and norms of good practice. Again like Galison, Mirowski aims to reveal the ways in which these discrete subcultures combine to form an intercalated culture, although now the culture in question is that of neoclassical economics rather than

[36] Michael E. Latham, *Modernization As Ideology: American Social Science and 'Nation Building' in the Kennedy Era* (Chapel Hill, NC: University of North Carolina Press, 2000); Engerman, *Modernization from the Other Shore: American Intellectuals and the Romance of Russian Development* (Cambridge, MA: Harvard University Press, 2003); David C. Engerman, Nils Gilman, Mark H. Haefele, and Michael Latham, eds., *Staging Growth: Modernization, Development, and the Global Cold War* (Amherst: University of Massachusetts Press, 2003); Nils Gilman, *Mandarins of the Future: Modernization Theory in Cold War America* (Baltimore, MD: Johns Hopkins University Press, 2003); On the deep origins of modernization theory, see Clifford Wilcox, *Robert Redfield and the Development of American Anthropology* (Plymouth: Lexington Books, 2004).

[37] Mirowski, *Machine Dreams*, 207-22, 232-308; idem, "Cowles Changes Allegiance: From Empiricism to Cognition as Intuitive Statistics," *Journal of the History of Economic Thought* 24 (June 2002): 165-93.

microphysics.[38] E. Roy Weintraub, meanwhile, has explored the changing meaning of the principal stylistic motif of neoclassicism in postwar American economics: mathematical expression. Weintraub shows how conflicts about the meaning and purpose of mathematics among economists emerged in response both to changing images of what mathematics was, and to the goals of economic analysis. The stories different economists have told about mathematical expression and styles of economic reasoning have helped to determine the identity of economics as a discipline. The ability to follow an existence proof of competitive equilibrium is on this reckoning a marker of subcultural style in mathematical economics in much the same way as the model puzzle-solutions performed by Kuhnian scientists.[39]

The capacity of the postwar human sciences to generate distinctive subjects of theoretical knowledge has also been demonstrated. In his biographical studies of Herbert Simon, Hunter Crowther-Heyck tracks the parallels between Simon's intellectual concern with the systems sciences and rational choice theory, on the one hand, and his performance as a "scientific broker" within the organizational maze of the postwar scientific patronage, on the other. Simon's attempts to build up the Graduate School of Industrial Administration at Carnegie Tech during the 1950s reveal an exemplar, as much as a student, of organizational behavior.[40] Jamie Cohen-Cole's microhistory of interdisciplinary exchange at Harvard's Center for Cognitive Studies in the 1960s provides the most detailed example of how an emphasis upon subcultural practices can

[38] See also D. Wade Hands and Philip Mirowski, "A Paradox of Budgets: The Postwar Stabilization of Neoclassical Demand Theory" in *From Interwar Pluralism to Postwar Neoclassicism, Annual Supplement to Volume 30, History of Political Economy*, ed. Mary Morgan and Malcolm Rutherford (Durham, NC: Duke University Press, 1998), 260-92.
[39] E. Roy Weintraub, *How Economics Became a Mathematical Science* (Durham: Duke University Press, 2002).
[40] Hunter Crowther-Heyck, "Herbert Simon and the GSIA: Building an Interdisciplinary Community," *Journal of the History of the Behavioral Sciences* 42 (Fall 2006): 311-34; idem, *Herbert A. Simon*.

illuminate the history of the human sciences. In this analysis of the institutionalization of the field of cognitive science, Cohen-Cole's principal focus is on economies of tool exchange at the Center. In the interdisciplinary enthusiasm of the postwar human sciences, tool trading was valued in its own right as the mark of progressive science. Cohen-Cole shows how these economies were established in seminars, lunch-halls, and corridor conversations. Students of human cognitive creativity were themselves encouraged to embody the principles of creativity, informality, and open-mindedness.[41]

The literature on modernization theory is anchored in detailed case studies of the institutional spaces where modernization theorists forged ideas and plied their trade. These include Harvard's Department of Social Relations, the Social Science Research Council's Committee on Comparative Politics, and the Center for International Studies at MIT.[42] Precisely because these institutions were (with the partial exception of the SSRC's Committee on Comparative Politics) conceptual trading zones for practitioners of various disciplines, historians have inquired after the norms of intellectual exchange and theoretical research in such characteristic sites of academic practice in the Cold War era. David Engerman has undertaken a similar task for the quasi-discipline of "Sovietology," which flourished primarily in interdisciplinary research institutes and centers within the expanding mid-century academy.[43] Equally institutional in orientation

[41] Jamie Cohen-Cole, "Instituting the Science of the Mind: Intellectual Economies and Disciplinary Exchanges at Harvard's Center for Cognitive Studies," *British Journal for the History of Science* 40 (December 2007): 567-97; idem, "The Reflexivity of Cognitive Science: The Scientist as Model of Human Nature," *History of the Human Sciences*, 18 (November 2005): 107-39. For a general history of cognitive science as an interdisciplinary research field, see Howard Gardener, *The Mind's New Science: A History of the Cognitive Revolution*, new ed. (New York: Basic Books, 1987)

[42] See Gilman, *Mandarins of the Future*; David C. Engerman, "West Meets East: The Center for International Studies and Indian Economic Development" in *Staging Growth*, 199-223.

[43] David C. Engerman, 'New Society, New Scholarship: Soviet Studies in Interwar America', *Minerva* 37 (March 1999): 25-43; idem, "The Ironies of the Iron Curtain: The Cold War and the Rise of Russian Studies in the United States', *Cahiers du Monde russe*, 45 (2004): 465-96.

are Emily Haputman's investigations into the ramified meanings of "theory" in postwar political theory. Her attempts to tease out the contested claims to the mantle of theory among behavioralists, rational choice theorists, and traditionalist political theorists draw her into explorations of theoretical ideologies at Harvard, Berkeley, and the Social Science Research Council.[44] S. M. Amadae and Bruce Bueno de Mesquita have pursued similar ends in their study of William Riker's school-building in "positive political theory" at the University of Rochester.[45]

Hence many of the typical features of academic subcultures identified by the likes of Warwick, Kaiser, and Hunter are found in the American human sciences after World War II: tool use, pedagogical discipline, and subject-making are all present in recent histories of the postwar ferment of the social and psychological disciplines. I want to draw particular attention to one other subcultural device that was especially important to postwar human scientists. This is what I call the "epistemological narrative." An understanding of the role of such narratives in the subcultures of the human sciences allows us to preserve the obvious importance of epistemological commitments to practising human scientists whilst avoiding the temptation of epistemic maps.

An important means of identifying oneself with a subculture is learning and then reproducing communal stories about the disciplinary subculture in question: its history, its aims, and the nature of the enterprise as a whole. The recounting of such tales not only demonstrates allegiance and signals membership; it may also justify and rationalize

[44] Emily Hauptman, "A Local History of 'The Political'," *Political Theory* 32 (February 2004): 34-60; idem, "From Opposition to Accommodation: How Rockefeller Foundation Grants Redefined Relations Between Political Theory and Social Science in the 1950s," *American Political Science Review* 100 (November 2006): 643-49; idem, "Defining 'Theory' in Postwar Political Science," in *The Politics of Method in the Human Sciences,* 207-32. See also Robert Adcock and Mark Bevir, 'The Remaking of Political Theory', in *Modern Political Science,* 209-33.

[45] S. M. Amadae and Bruce Bueno de Mesquita, "The Rochester School: The Origins of Positive Political Theory," *Annual Review of Political Science* 1 (1999): 269-95.

the ongoing practices and norms of the subculture. What I want to suggest is that narratives about the nature of knowledge and the epistemological standing of a particular discipline serve exactly these purposes. In the history of the American human sciences, such narratives have been a defining feature of a wide spectrum of disciplines since "theory" itself became a keyword in the 1930s. Although the appeal to scientific method and naturalistic ontologies had long been made by social scientists, the rhetoric of the "conceptual scheme" and "scientific explanation" was appropriated by the burgeoning theoretical subcultures of the interwar and postwar decades—just as precarious new enterprises in the human sciences were being launched.

There were both lay and specialist versions of epistemological narratives in these years, with a shift from the preponderance of the former to the latter coming during the 1950s. The non-specialist trope of the "conceptual scheme," for example, was taken up first by the Harvard biochemist and part-time social theorist Lawrence Joseph Henderson, who used it to legitimate his own constructivist view of scientific knowledge. Through the interdisciplinary meeting places he dominated at Harvard—the Fatigue Laboratory at the Business School, the Pareto seminars and "Applied Sociology" lecture courses he led, and the web of informal contacts he had among the faculty—Henderson disseminated this notion among historians, philosophers, and social scientists. Talcott Parsons appropriated the term to buttress his theory of human action, whilst Crane Brinton, James Bryant Conant, and W. V. Quine used the Hendersonian motif as the epistemological foundation of their own diverse projects.[46] Another homespun epistemology issuing from interwar

[46] Henderson's account of conceptual schemes and the nature of scientific knowledge was so influential that a comprehensive list of works where it is expounded must remain provisional. A partial list of works by those directly and explicitly influenced by the Hendersonian vision would include George P. Homans and Charles P. Curtis Jr., *An Introduction to Pareto: His Sociology* (1934; New York, H. Fertig 1970), 15-

the Harvard faculty, Percy Bridgman's operationalism, gained adherents in psychology and economics during the same period.[47]

Emily Hauptman's description of the three starkly divergent accounts of theory offered by Sheldon Wolin, David Easton, and William Riker offers a further example of the importance of lay narratives about knowledge in defining the meaning of theory in political science. Easton was in fact drawing on Parsons' image of social systems theory, whilst Riker sought to echo an epistemological narrative about the nature of economic theory laid down by Milton Friedman in his 1953 essay "The Methodology of Positive Economics."[48] The specialization of such epistemological narratives ran more or less parallel to the emergence of the professional subfield of the philosophy of science, which was defined in its early years by a strongly linguistic and empiricist reading of the work of Carnap and Hempel.[49] Chicago-based students of Carnap such as Herbert Simon and the political scientist Abraham Kaplan began to frame their stories about knowledge in social science in terms of the syntactic or formal models of their teacher.[50] In later years, many behavioral scientists began to cite Kuhn in an effort to narrate themselves as

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47; Crane Brinton, *The Anatomy of Revolution*, 2nd ed. (London: Cape, 1953), 8-12; Talcott Parsons, *The Structure of Social Action*, 2nd ed. (Glencoe, IL: Free Press, 1949), 41-2; James B. Conant, *On Understanding Science* (New Haven: Yale University Press, 1947), 24; Bernard Barber, *Science and the Social Order* (1952; New York: Collier, 1962), 37-50. Even the philosopher W. V. Quine admitted that he adopted the term "conceptual scheme" from Henderson, although he insisted that he "meant it as ordinary language, serving no technical function": W. V. Quine, "On the Very Idea of a Third Dogma," in idem, *Theories and Things* (Cambridge, MA: Harvard University Press, 1981), 41.

[47] P. W. Bridgman, *The Logic of Modern Physics* (New York: Macmillan, 1928); Gary L. Hardcastle, "S. S. Stevens and the Origins of Operationism," *Philosophy of Science* 62 (September 1995): 404-24.

[48] Hauptman, "Defining 'Theory'"; David Easton, "The Decline of Modern Political Theory," *Journal of Politics* 13 (February 1951): 36-58; idem, *The Political System* (New York: Knopf, 1953); William H. Riker and Peter C. Ordeshook, *An Introduction to Positive Political Theory* (Englewood Cliffs, NJ: Prentice-Hall, 1973); Milton Friedman, "The Methodology of Positive Economics," in idem, *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), 3-43.

[49] On the declension from scientific philosophy to philosophy of science, see Gary L. Hardcastle and Alan Richardson, eds., *Logical Empiricism in North America* (Minneapolis: University of Minnesota Press, 2003); George A. Reisch, *How the Cold War Transformed Philosophy of Science: To the Icy Slopes of Logic* (Cambridge: Cambridge University Press, 2005).

[50] John G. Gunnell, *The Descent of Political Theory: The Genealogy of an American Vocation* (Chicago: University of Chicago Press, 1993), 222-25.

moving beyond pre-paradigmatic chaos in social science. Economists, deep into the process of mathematization by the 1950s, worried less about philosophy of science, and held to Friedman's catechism about idealized assumptions and testable hypotheses.[51]

In each case, whether homespun commonsense or technical articulations, stories about how disciplines made knowledge were woven into the practices that gave theoretical subcultures an identity. Epistemological narratives helped to stabilize modes of knowledge-making. This, I think, is how we should understand the function of epistemological commitments in the human sciences. Rather than supposing that they define the activities of social scientists, we should conceive of them as one among a number of elements that help to embed a research program within a set of subcultural practices. This role of epistemological narratives, I shall now argue, flags up a more general issue: the ideological (or interest-guided) nature of subcultural practices. This ideological dimension of subcultures in the human sciences, I suggest, points up a perhaps fruitful set of categories for mapping the human sciences.

III. SUBCULTURE AND IDEOLOGY IN THE HUMAN SCIENCES

Everything I have said so far indicates the applicability and fruitfulness of the subcultural understanding of knowledge for the history of the postwar American human sciences. But I now want to show that we cannot rest there. We should, I propose, evaluate the assorted tools, pedagogical drills, and epistemological narratives of a subculture as ideological instruments. The elements of a theoretical subculture in the human sciences are linked in a variety of ways to social interests and social practices. The embedding of

[51] See, e.g., Kenneth Arrow's epistemological remarks in "Mathematical Models in the Social Sciences" in *The Policy Sciences*, ed. Daniel Lerner and Harold D. Lasswell (Stanford: Stanford University Press, 1951), 129-54.

theoretical models, epistemological narratives and disciplined subjects in institutions inside and outside of the academy is what gives them an ideological character. My argument in support of this claim moves in three steps: from a description of the looping effect to which knowledge in the human sciences is subject, to the significance of the "performance" of that knowledge, to a threefold typology of the ideological mechanisms whereby human-scientific knowledge is embedded in social practices.

I begin with the looping effect. The kinds of people posited in the social and human sciences are not neutral or purely external classifications. As Ian Hacking has observed, our knowledge of people of a certain type is meant to provide "principles according to which we can interfere, intervene, help, and improve." That is why so many of the "human kinds" studied by the human sciences are linked to deviance and abnormality: child abusers, suicides, schizophrenics, and so forth. This is one very obvious ideological entanglement in which the human sciences find themselves.

Crucially, however, the lack of objective distance between the human sciences and the objects they study goes further than this. Theories and classifications in the human sciences do not "discover" an independently existing reality; child abuse and schizophrenia were not always "out there" among the phenomena of the world waiting to be found. There are in part created *by* theories and classifications. Much of this comes down to the publicity of knowledge. Insofar as scientific descriptions of people are made available to the public, they may "change how we can think of ourselves, [and] change our sense of self-worth, even how we remember our own past."[52] As such, the descriptions and classifications of persons generated and studied by the human sciences

[52] Ian Hacking, "The Looping Effect of Human Kinds" in *Causal Cognition: A Multidisciplinary Debate*, ed. Dan Sperber, David Premack, and Ann James Premack (Oxford: Clarendon Press, 1995) 352, 361, 369.

"alter the space of possibilities for personhood": they become part of the descriptions that the individuals being classified may draw upon when formulating their actions and identities.[53] Those labelled delinquents, for instance, may feel ashamed when they learn the meaning of this concept, and change their behavior in order to conform to the "normal" type. Or, to take an alternative example, those identified as homosexuals may incorporate that category into their own positive sense of personhood.[54] But these reactions in turn generate feedback effects: "New sorting and theorizing induces changes in self-conception and in behavior of the people classified. Those changes demand revisions of the classifications and theories. . . . Kinds are modified, revised classifications are formed, and the classified change again, loop upon loop."[55]

In the human sciences, then, the practices of knowledge-making do not stop at the institutional border of the subcultures in which they are developed; they can move out onto the wider landscape of human action. This dimension of the looping effect brings me to the second stage of my argument. Theories are embedded in certain kinds of performance. If concepts and classifications from the human sciences enter into the descriptions drawn upon for carrying on a social practice, those concepts and classifications are logically expressed in, and thereby define, the social practices that the theory attempts to comprehend.[56] This claim has been pushed most forcefully in recent anthropological studies of modern financial economics and the operation of markets. Michel Callon has sought to combat a prevalent "externalism" which holds the discipline

[53] Ian Hacking, "Making Up People," in idem, *Historical Ontology* (Cambridge, MA: Harvard University Press, 2002), 107.
[54] On the emergence of the homosexual as a social role, see Mary McIntosh, "The Homosexual Role," *Social Problems* 16 (Autumn, 1968): 182-92; Hacking, "Making Up People," 103.
[55] Hacking, "Looping Effect," 368-70. See also Ian Hacking, "The Making and Molding of Child Abuse," *Critical Inquiry* 17 (Winter 1991): 254-55.
[56] See Charles Taylor, "Social Theory as Practice," *Philosophy and the Human Sciences*, 91-115.

of economics apart from the empirical existence of economies. Callon asserts that the centrality of economic ideas to economic activity makes it "meaningless to distinguish between an existing reality (economy) and the analytical discourse explaining it." Rejecting the tendency of economic sociologists to bemoan the thin account of economic agency provided by neoclassical economics, Callon declares: "economy is embedded not in society but in economics."[57] To put this claim in Hacking's terms, Callon is arguing that economic theories provide many of the descriptions needed to carry on market practices.

Is economics performative in so strong a sense? Is psychology? Sociology? It is up to historians of the human sciences to examine in what ways, and to what extent, knowledge has looped into social practices. Much of Ian Hacking's recent historical research has examined how the human kinds of demography, social statistics, and psychology have been performed in the uses of social concepts such as the "normal" citizen, child abuse, multiple personality disorder, and autism. The critical intent of Hacking's diverse inquiries has been to demonstrate how tangled the looping of human kinds has been. Once released among the public, kinds have been stretched, restricted, and applied for a variety of social purposes.[58] Sarah Igo has carried out a similar project in her history of the circulation of social scientific surveys and the creation of a mass public the United States during the middle decades of the twentieth century. [59] Jennifer

[57] Michel Callon, "Introduction: The Embeddedness of Economic Markets in Economics," in *The Laws of Markets* (Oxford: Blackwell, 1998), 29-30.
[58] Ian Hacking, *The Taming of Chance* (Cambridge: Cambridge University Press, 1990); idem, "Multiple Personality Disorder and Its Hosts," *History of the Human Sciences* 5 (May 1992): 3-31; idem, "The Making and Molding of Child Abuse"; idem, *Mad Travellers: Reflections on the Reality of Transient Mental Illnesses* (Charlottesville: University Press of Virginia, 1998).
[59] Sarah Igo, *The Averaged American: Surveys, Citizens, and the Making of a Mass Public* (Cambridge, MA: Harvard University Press, 2007). Hacking's work on "making up people" is also invoked in Jamie

Light, meanwhile, has shown how urban planners of the Johnson-Nixon era encouraged the inhabitants of neighborhoods targeted for reform to perform the systems analysis that had already been carried out by experts to think about urban problems.[60]

Inspired by Callon, the history and sociology of economics has seen the most explicit exploration of these themes. Turning anew to the sociology of capitalist markets, one of the great themes of classical social theory, scholars are rejecting the traditional notion that economics attempts to create free-standing representations of market processes (which economic sociologists must then insist leaves out power, or cultural context, or the fullness of human agency). Advocates of the performative approach "recognize economics not as a (misguided) science of capitalism but as its technology, that is, as one of the active ingredients in the production and reproduction of the market order."[61] Rather than interposing causal arrows between people and cultures, on one hand, and the operation of markets, on the other, economic sociologists are now investigating how knowledge, custom, and modes of subjectivity are built into, and generated by, markets.[62] The most radical forms of performativity are those in which an economic model is incorporated into the "algorithms, procedures, routines, and material devices" by which a market operates.[63] This is looping with a vengeance: the theoretical subculture from which the model arose cannot claim to represent an independent reality; it is formative of that very reality.

Cohen-Cole, "Thinking About Thinking in Cold War America," unpublished PhD dissertation, Princeton University, 2003.

[60] Light, "Taking Games Seriously."

[61] Marion Fourcade, "Theories of Markets and Theories of Society," *American Behavioral Scientist* 50 (April 2008): 1025.

[62] Marion Fourcade and Kieran Healy, "Moral Views of Markey Society," *Annual Review of Sociology* 33 (2007): 285-311.

[63] Donald MacKenzie, *An Engine, Not a Camera: How Financial Models Shape Markets* (Cambridge, MA: MIT Press, 2006), 19

These considerations bring us to the third and most important stage of my argument: ideology. We will miss some of the significance of what postwar human scientists were doing if we seek *only* to read their texts, tools, and epistemological narratives as elements of a particular subculture. Cognizance of the looping effect makes the attempt to embed knowledge in a subculture seem a peculiar sort of enterprise. It also means that elements of that subculture can be seen as vehicles for a much wider range of social interests. Historians of the human sciences should therefore take subculture formation as a special case within a broader range of embedding techniques through which knowledge of human beings becomes entangled with the world.

The most obvious mode of embedding is *intervention*. The human sciences have long nurtured the desire to forge knowledge about people and their behaviour that will allow for intervention, reform, and aid. Theories about the origins of multiple personality disorder or the sources of economic poverty are obvious examples of knowledge that may be incorporated into psychiatric therapies or social policy.[64] Another embedding technique is *incorporation*.[65] There are more or less reified versions of this kind of embedding. Perhaps the least reified form of incorporation involves the taking up of aspects of a theory into the descriptions that underpin a social practice. This is what the philosopher Charles Taylor claims for social theory *as* a practice.[66] Accounts of incorporation of this sort must speak in general terms about concepts in the human

[64] Ian Hacking, *Rewriting the Soul: Multiple Personality and the Sciences of Memory* (Princeton, NJ: Princeton University Press, 1995); Alice O'Connor, *Poverty Knowledge: Social Science, Social Policy, and the Poor in Twentieth-Century U.S. History* (Princeton, NJ: Princeton University Press, 2001).
[65] This term figures in Donald MacKenzie's discussion of varieties of performativity in *An Engine, Not a Camera*, 19.
[66] See footnote 56.

sciences entering into the "social imaginary" or "normative vocabulary" of a society.[67] But there are other, more reifying forms of incorporation. A mathematical model in finance theory may serve as a *technology* of financial markets; it is part of the machinery and calculations that allow a market to function. In this case, a "paper tool" becomes materially embedded in practices outside of the particular academic subculture in which it was formed.

We come, finally, to a third form of embedding: *embodiment*. This is the technique whereby a given theoretical paradigm is embodied in an array of subcultural elements—formulae, diagrams, proofs, epistemological narratives, training regimes, and so on. I hope that what I have said about looping, performativity, and embedding has highlighted how tendentious and tricky enterprise it is to try to insulate a theory within a subcultural regime. Knowledge in the human sciences stands on unstable social and epistemological ground: insofar as elements of a theoretical subculture are embedded in the world through the techniques of intervention or incorporation, the use and hence the meaning of the theory must escape the control of that subculture. As such, the cultural work of embodying a theory in a subculture—the tasks of establishing drills, creating tools, articulating narratives—can be seen as ideological. The attempt to embody claims to knowledge in these ways is at the same time an attempt to establish the authority of that knowledge. The demonstration that one has tools and persuasive epistemological narratives to describe what one is doing is evidence for the viability of a theory.

These three forms of embedding may reinforce or hinder one another. The bid to embody a theory can by aided by its incorporation into a social practice, as would seem

[67] Charles Taylor, *Modern Social Imaginaries* (Duke, NC: Duke University Press, 2004); Quentin Skinner, *Visions of Politics*, Vol. 1: *Regarding Method* (Cambridge: Cambridge University Press, 2002).

to have been the case with modern finance theory and the emergence of derivatives markets. Conversely, incorporation can be facilitated by the appeal to the epistemological narrative in which a theory is embodied. As Donald MacKenzie and Yuval Millo have shown, one reason why the Chicago Board Options Exchange got off the ground in the early 1970s was because of the scientific status granted the proofs of mathematical neoclassical economics. When professional economists demonstrated the rationality of options and futures markets, the Securities and Exchange Commission dropped its long-held suspicion of derivatives.[68] The same holds for intervention. Public policies aimed at alleviating poverty have drawn legitimacy from the epistemological standing of social scientific knowledge. Finally, embedding techniques can also rebound upon one another as feedback in the looping process occurs; interventions or incorporations may act to discredit, rather than to legitimize, a theory. This is what Donald MacKenzie calls "counterperformativity."[69]

IV. EMBEDDING AND THE HISTORY OF THE HUMAN SCIENCES

Talking about the human sciences in terms of embedding techniques and ideological purposes avoids the temptation to draw epistemic maps. No longer to do we need to ask what version of positivism a given discipline or research program espouses. We are concerned instead with the looping of human kinds into human action; we want to know how particular models or classifications have been embedded in social practices, and with what effects. The vocabulary of intervention, incorporation, and embodiment allows us to track the fortunes of the human sciences against a new set of standards. Historians

[68] Donald MacKenzie and Yuval Millo, "Constructing a Market, Performing a Theory: The Historical Sociology of a Financial Derivatives Exchange," *American Journal of Sociology* 109 (July 2003): 107-45
[69] MacKenzie, *An Engine, Not a Camera*, 33-4, 259-60.

such as James Kloppenberg, Daniel Rodgers, Sarah Igo, Jennifer Light, Alice O'Connor, and Alan Petigny have already laid down paths in American history for studies of intervention and incorporation.[70] They have examined the social and political history of academic knowledge: the looping of welfare economics, social surveys, and Freudian psychology into public policy and social change in the modern United States. By the same token, however, the looping framework also flags up limitations in the existing literature. For example, several accounts of the American social sciences during the Cold War view these disciplines exclusively through the lens of intervention. The social sciences are conceived as arms of the national security state, whose purpose was to exert technical control over both the masses at home and postcolonial peoples abroad.[71] Such histories tend to ignore the incorporation and contestation that is the hallmark of the looping effect. Experts seldom had it all their own way; even the most recondite systems models could loop back upon its creators once let loose in the public arena.

Those of us who are interested in the internal histories of the human science disciplines can train our focus elsewhere. One possible line of inquiry is the examination of how and why powerful theoretical and methodological orthodoxies emerged in some postwar disciplines but not in others. It is useful to describe this trend in terms of the success or failure of attempts at embodiment. A brief example of the approach I have in mind may help to clarify this point. The following sketch is meant only offer a sense of how a historical narrative focussed on techniques of embedding might unfold. Given the

[70] James T. Kloppenberg, *Uncertain Victory: Social Democracy and Progressivism in European and American Social Thought, 1870-1920* (New York: Oxford University Press, 1986); Daniel Rodgers, *Atlantic Crossings: Social Politics in a Progressive Age* (Cambridge, MA: Harvard University Press, 1988); Alan Petigny, "Illegitimacy, Postwar Psychology, and the Reperiodization of the Sexual Revolution," *Journal of Social History* 38 (Fall 2004): 63-79; Igo, *Averaged American*; Light, "Taking Games Seriously"; O'Connor, *Poverty Knowledge*.

[71] For a broader critique of this approach in the history of the Cold War human sciences, see Joel Isaac, "The Human Sciences in Cold War America," *Historical Journal* 50 (September 2007): 725-46.

brevity of the analysis, I do not wish to suggest that this is a complete or sustained argument.[72]

In the United States, economics and social theory had superficially similar trajectories through the middle decades of the twentieth century. Both are said to have been heterodox or pluralist enterprises during the 1930s. Historians of economics emphasize the sheer variety of research programs which found adherents in the years following the end of World War I.[73] As Malcolm Rutherford has observed, the absence of any single figure in the United States who could command the same degree of allegiance among economists as marginalist Alfred Marshall in Great Britain meant that pluralism could flourish. "Marginalist, Austrian, institutional, historicist, and various evolutionary ideas all contended and were variously intermixed" in the 1920s and early 1930s.[74] Sociology, meanwhile, was "riven" by the ideological fissures of the Great Depression. With the Chicago School in eclipse, American sociology was defined by a number of competing schools, from statisticians and survey researchers to functionalists, Marxists, and symbolic interactionists.[75] The passage from World War II to the Cold War, however, saw the collapse of pluralism in both disciplines and the consolidation of

[72] I try to give a fuller account of the two cases I discuss below in Joel Isaac, "Theorist at Work: Talcott Parsons and the Carnegie Project on Theory, 1949-1950," unpublished manuscript; and idem, "Theories of Knowledge and the American Human Sciences, 1920-1970," PhD Dissertation, University of Cambridge, 2005, chapters 3 and 6.

[73] References to the essential "pluralism" of interwar economics abound in the contemporary literature on history of American economic thought. The most explicit treatment of interwar pluralism is to be found in many of the essays collected in Morgan and Rutherford, eds., *From Interwar Pluralism to Postwar Neoclassicism*. The existence of heterodoxy in interwar economic thought has become an accepted fact among critics of neoclassical economics. For an illustration of this point, see the remarks in Mirowski, *Machine Dreams*, 190.

[74] Malcolm Rutherford, "American Institutionalism and the History of Economics," *Journal of the History of Economic Thought* 19 (Fall 1997): 182.

[75] George Steinmetz, "American Sociology Before and After World War II: The (Temporary) Settling of a Disciplinary Field" in *Sociology in America: A History*, ed. Craig Calhoun (Chicago: University of Chicago Press, 2007), 319.

powerful orthodoxies in each: mathematical neoclassicism in economics, and functionalism in sociology.[76]

Recent scholarship, however, has brought to light some important qualifications to this picture. As noted earlier, Mirowski has suggested that the power of neoclassical orthodoxy in postwar economics lay in its diversity: the separate strands of economic analysis at Chicago, MIT, and Cowles/RAND made for a robust but loose methodological consensus.[77] The notion of a functionalist "mainstream" in postwar sociology, dominated by Talcott Parsons' "general theory of action" has been called into question.[78] The resonance of Parsonian functionalism within the sociological profession would seem to have been exaggerated by influential treatises on social theory.[79] The general theory of action, despite Parsons' best efforts, remained marginal in the discipline. The other two alleged figureheads of postwar sociological orthodoxy, Robert K. Merton and Paul Lazarsfeld, were in reality only loosely associated with the functionalist paradigm. The blend of theory and empiricism that they promoted yielded not doctrinal consensus but rather a particular attitude toward the conduct of social inquiry.[80]

A comparative assessment of postwar economics and sociology might ask why the former succeeded in forging orthodoxy from interwar pluralism whilst the latter did

[76] For versions of this story, see Pierre Bourdieu, "Epilogue: On the Possibility of a Field of World Sociology" in *Social Theory for a Changing Society*, ed. Pierre Bourdieu and James S. Coleman (Boulder, CO: Westview Press, 1991), 373-87; Steinmetz, "American Sociology."
[77] See fn. 37-38.
[78] Craig Calhoun and Jonathan VanAntwerpen, "Orthodoxy, Heterodoxy, and Hierarchy: 'Mainstream' Sociology and Its Challengers" in *Sociology in America*, 367-410.
[79] See especially Jürgen Habermas, *The Theory of Communicative Action,* vol. 2: *The Critique of Functionalist Reason*, trans. Thomas McCarthy (Cambridge: Polity, 1987); Jeffery C. Alexander, *The Modern Reconstruction of Classical Thought,* vol. 4*: Talcott Parsons* (Berkeley: University of California Press, 1983).
[80] Calhoun and VanAntwerpen, "Orthodoxy, Heterodoxy, and Hierarchy," 390-96.

not. It is in response to questions such as these that we can usefully discuss attempts at subcultural embodiment. One reason why Parsonian functionalism did not become the dominant research program in postwar sociology was that Parsons failed to generate subcultural practices for the theory of social action. This was not for want of trying. The aforementioned Department of Social Relations, established in 1946, was intended to serve as the institutional home of general theory in the social sciences. But "SocRel" was born before anything more than a desire to forge a "common language" for the social sciences had been agreed among Parsons and his allies on the Harvard faculty.[81] The Carnegie Corporation-sponsored "Project on Theory" of 1949-1950 was designed by Parsons explicitly to make good on the promise of a general theory in social science. But as the chief published product of the Project made clear, Parsons and his associates could not agree on much more than a general statement of what a general theory would look like, and how it would be used. Published in 1951, the collaborative volume *Toward a General Theory of Action* displayed the sheer complexity and heterodoxy of action theory.[82] The epistemological narratives put forth by Parsons, Edward Shils, Richard Sheldon, and Robert Sears (among others) did not mesh into a coherent story that action theorists could tell themselves. Parsons' and Shils' attempts to craft analytical tools for the general theory, meanwhile, were abortive. The pattern variables schema was

[81] J. T. Dunlop et al., "Toward a Common Language for the Area of Social Science," (Harvard University, 1941), 4-5. On the origins and early organization of the Department of Social Relations, see Talcott Parsons, "On Building Social System Theory: A Personal History," *Daedalus* 99 (1970): 841; Benton Johnson and Miriam M. Johnson, "The Integrating of the Social Sciences: Theoretical and Empirical Research and Training in the Department of Social Relations at Harvard," in *The Nationalization of the Social Sciences*, ed. Samuel Z. Klausner and Victor M. Lidz (Philadelphia: University of Pennsylvania Press, 1986), 131-9; Howard Brick, "Talcott Parsons's 'Shift Away from Economics," *Journal of American History* 87 (2000): 507-11; Rebecca M. Lemov, "The Laboratory Imagination: Experiments in Human and Social Engineering," PhD dissertation (University of California at Berkeley, 2000), 297-311.

[82] Talcott Parsons and Edward A. Shils, eds., *Toward a General Theory of Action: Theoretical Foundations for the Social Sciences* (1951; New York, Harper & Row, 1962).

supposed to be a very basic instrument for classifying social systems; but it ramified out into a bewildering array of tables, diagrams, and prose descriptions.[83] It was not at all clear what kind of training and expertise were programmed by the theory.

These problems of embodiment were compounded by limitations on intervention and incorporation. As a basis for intervention, the Byzantine logic of Parsonian action theory was a non-starter. The possibilities of incorporation were somewhat wider, but even these remained narrow. Nils Gilman and others have pointed out that aspects of the pattern variables schema and related typologies of modern social systems were taken up by modernization theorists.[84] But this was weak incorporation. No specific tools, models, or formulae in action theory were included in the writings of Gabriel Almond, Walt Rostow, or even Shils himself; it was the orientation toward the lineaments of modern societies that carried over from the pages of Parsons' books to the research projects launched at the Center for International Studies and the Committee on Comparative Politics.

Attempts at subcultural embodiment met with greater success in economics, and thereby laid the foundations for neoclassical hegemony. Undoubtedly, the transmission of mathematical technique was the foundation of neoclassical subcultures. Mirowski is surely correct that the rise of the systems sciences, along with economists' early adoption of the OR creed, remade the professional culture of American economics during World War and Cold War. But the ground for the reception of model-based mathematical analysis was laid in the midst of the earlier pluralist moment. The Great Depression

[83] Talcott Parsons and Edward Shils, "Values, Motives, and Systems of Action" in *Toward a General Theory of Action*, Fig. 1-15.
[84] Gilman, *Mandarins of the Future*; Calhoun and VanAntwerpen, "Orthodoxy, Heterodoxy, and Hierarchy," 387-88.

reordered priorities in economics. The failure of senior economists to predict the slump, or to offer any immediate remedies, heightened the sense among younger scholars that a mathematical rigour was needed to buttress the foundations of economic knowledge. Nobel prizewinner Lawrence Klein has described his attempts in the early 1940s to "gain acceptance for a methodology in economics, namely, the mathematical method."[85] In the 1930s, Milton Friedman was a talented mathematician who had been a hair's breadth away from undertaking doctoral study in applied mathematics at Brown University before opting for economics at Chicago.[86] He would be among the first American economists systematically to use mathematics in his research.

The most influential champion of mathematics among the new generation was Paul Samuelson. As an undergraduate at the University of Chicago he had begun using the calculus in his economic studies, but had encountered resistance from Frank Knight, who rejected the use of "jargon" in economics. Moving on to graduate school at Harvard in 1935, Samuelson developed his mathematical skills further, but was again stymied when his earliest papers were rejected by journals alarmed by the symbolic machinery on display. His faith in the scientific potential of economics, however, was undimmed. "[T]o a person of analytical ability, perceptive enough to realize that mathematical equipment was a powerful sword in economics, the world of economics was his or her oyster in 1935. The terrain was strewn with beautiful theorems begging to be picked up and arranged in unified order."[87] The use of mathematical techniques was consolidated

[85] Lawrence R. Klein, "Lawrence R. Klein" in *Lives of the Laureates: Eighteen Nobel Economists*, ed. William Breit and Barry T. Hirsch, 4th ed. (Cambridge, MA: MIT Press, 2004) 18.
[86] Milton and Rose D. Friedman, *Two Lucky People: Memoirs* (Chicago: University of Chicago Press, 1998), 33.
[87] Paul Samuelson, "Economics in a Golden Age: A Personal Memoir," *Paul Samuelson and Modern Economic Theory*, ed. E. Cary Brown and Robert M. Solow (New York: McGraw-Hill, 1983), 6.

by the interdisciplinary, tool-based scientific research of World War II. Competent proofs of the major theorems in the discipline gradually became the mark of graduate training in economics (although not without a rearguard attack from surviving institutionalists).[88] Epistemological narratives about theoretical abstraction, elegance, and prediction settled into the profession. At the same time, the bedding down of subcultural devices in economics was aided by the incorporation of economic models into the logistical operations and geopolitical calculations of the American state. Where Parsonian theory had suffered from its inability to secure techniques of intervention and incorporation, mathematical neoclassicism prospered from its successes in this regard.

CONCLUSION

This all-too-brief comparison of postwar economics and social theory is meant only to illustrate that we can write histories of the human sciences without drawing epistemic maps. The rise and fall of theoretical doctrines in the human science disciplines is never simply a matter of epistemology, predictive success, or falsification. Theories also prosper by providing instruments for disciplinary training; they can become attached to the epistemic and moral norms of a community of specialists. A theory's success in so embedding itself in this subcultural form helps to determine its historical prominence, its waxing or waning in the culture of its time. The major postwar academic entrepreneurs in the human sciences—men such as Talcott Parsons, Herbert Simon, Paul Samuelson, and

[88] For a general overview of post-1945 reforms in graduate training in economics, see William J. Barber, "Postwar Changes in American Graduate Education in Economics," *The Post-1945 Internationalization of Economics: Annual Supplement to Volume 28* History of Political Economy, ed. A. W. Bob Coates (Durham, N. C.: Duke University Press, 1996), 12-30; and, from the inside, Martin Shubik, "Game Theory at Princeton, 1949-1955: A Personal Reminiscence" in *Toward a History of Game Theory: Annual Supplement to Volume 24, History of Political Economy*, ed. E. Roy Weintraub (Durham: Duke University Press, 1992), 151-63.

Jerome Bruner—seem to have understood that the authority of their research programs rested to a large degree on their ability to establish subcultures that would embody them. They sought to institutionalize modes of tool creation and exchange, epistemological narratives, and replicable techniques of inquiry. They tried to find procedures for making people (i.e. graduate students) who could perform and develop the theory. In the Department of Social Relations, in the GSIA at Carnegie Tech, or at RAND, academic entrepreneurs tried to make their theories stick in disciplinary practices.

And stick they often did. Today, behavioral science is out of vogue, but it carried all before it in the heady days of the late 1940s and early 1950s. So to modernization theory, which fell from grace only in the 1970s. (Some would argue it has been reincarnated since then.) Some specific theoretical enterprises—the general theory of action developed in the Department of Social Relations, or behavioralism in political science—never became "orthodoxies," but even these offered influential models for practising theory in the human sciences. Many more postwar subcultures adapted and survived. If cybernetics flared and faded out in the 1940s, game theory, cost-benefit analysis, and others branches of systems thinking have endured. The same is true of rational choice theory, which stands today as perhaps the dominant strand in political science, despite repeated and often devastating methodological critiques.

My recommendation, then, is that we take the subcultural perspective seriously. It frees us from assessing the fortunes of the human sciences solely in terms of their epistemological commitments; and it opens up a realm of practices and instruments in which human-scientific knowledge is embedded. We should evaluate those elements as ideological instruments that may be used to undergird research programs in such a way as

to play a role in their disciplinary authority and institutional endurance. The embodiment of human-scientific knowledge in subcultural practices, moreover, should be understood against the backdrop of the looping effect. Theoretical subcultures occupy just one particular point on the ceaseless looping between the human sciences and human beings. Historians of the American human sciences thus take up one spot in the broader study of social ideas and cultural change in the modern United States.