

“Hypothetical Machines”

The Science Fiction Dreams of Cold War Social Science

*By Rebecca Lemov**

ABSTRACT

The introspectometer was a “hypothetical machine” Robert K. Merton introduced in the course of a 1956 how-to manual describing an actual research technique, the focused interview. This technique, in turn, formed the basis of wartime morale research and consumer behavior studies as well as perhaps the most ubiquitous social science tool, the focus group. This essay explores a new perspective on Cold War social science made possible by comparing two kinds of apparatuses: one real, the other imaginary. Even as Merton explored the nightmare potential of such machines, he suggested that the clear aim of social science was to build them or their functional equivalent: recording machines to access a person’s experiential stream of reality, with the ability to turn this stream into real-time data. In this way, the introspectometer marks and symbolizes a broader entry during the Cold War of science-fiction-style aspirations into methodological prescriptions and procedural manuals. This essay considers the growth of the genre of methodological visions and revisions, painstakingly argued and absorbed, but punctuated by sci-fi aims to transform “the human” and build newly penetrating machines. It also considers the place of the nearly real-, and the artificial “near-substitute” as part of an experimental urge that animated these sciences.

Why has not man a microscopic eye?
For this plain reason, man is not a fly.
—Alexander Pope,
“An Essay on Man,” Epistle I

STREET-LEVEL WORK

In 1932, first-year Harvard sociology graduate student Robert K. Merton took a summer job interviewing “just about all the hoboes and homeless men and women” of Boston.¹ He

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¹ The historical-background account of the creation of the focused interview technique is found in Robert K.

did this less out of keen sociological interest in the plight of the destitute than concern for his own plight. The job was a way, in his phrase, to “ke[ep] myself alive.” A child of eastern European immigrants, born in south Philadelphia, Merton né Meyer Schkolnik lacked the financial advantages of many of his cohort in the Department of Sociology. Aside from sustenance, it provided some unforeseen technical opportunities. Interviewing down-and-outers for long hours under “sometimes strenuous conditions,” Merton—early in what would eventually become one of the longest sociological careers of the twentieth century—developed respect for the possibilities of honing and improving a social science method.

By trial and error, his feet literally on the ground, he saw what made interviews work when they did and how at times they failed. Only a few years later, other New Deal documentarians would use their research to unveil the mottled lives of otherwise unseen subjects in works such as *You Have Seen Their Faces* and *Let Us Now Praise Famous Men*. Some felt “seared” by the suffering they witnessed. Merton, by way of contrast, seems to have absorbed a lesson in how-to.² The significance of the hobo research job, in short, was that he learned how best to learn about people: *how to refine a method for gathering social and human knowledge*. It was less a substantive than a methodological turning point. It was no less revolutionary for that.

The methodological entrepreneurship of Merton and many other coming-of-age American social scientists—quite simply, their zeal for method—is my focus here. What would eventually become a continual, near-Talmudic process of revision and augmentation gained results at the height of Cold War social science. An avid exchange of “working documents” systematized social-science-as-métier. In a newly self-conscious manner, its authors expounded the fine points of interviewing (delineating along the way the non-directive interview, the expressive interview, the depth interview, the direct interview, the focused interview, the coercive interrogation, and other forms of controlled conversation), the doing of fieldwork, the making and charting of firsthand observations, and the protocol for giving projective tests. Their literature also provided painstaking considerations of what to do with the resultant unmatched stock of data. Altogether it constituted a genre devoted to practice-as-method and the methods proper to practice and, as such, it offers

Merton, “Focus Groups: Continuities and Discontinuities,” *Public Opinion Quarterly*, 1987, 51:550–566; on p. 553. A subsequent, slightly different version of these events appeared as part of Merton’s Introduction to Robert K. Merton, Marjorie Fiske, and Patricia L. Kendall, *The Focused Interview: A Manual of Problems and Procedures*, 2nd ed. (New York: Free Press, 1990), pp. xiii–xxxi; the first edition was published in 1956. The quotation is from the second version (1990 edition), p. xvi.

² This, at least, is how Merton framed the experience and his concomitant education. In noting that the firsthand work with indigents and drifters did not seem a formative experience in Merton’s life save in regard to *methodology*, one might speculate about personal reasons; however, here the purpose is to suggest a new historiographical angle on the period among certain soon-to-be eminent social science researchers. Whereas, for example, James Agee and Walker Evans used their WPA work as a route to engage in existential-slash-self-revelatory journeys, this was not the case for Merton, possibly because he was quite familiar with how the other half lives. (Note, too, that Merton was at this time a socialist and age-mate of the youngest of the New Deal documentarians.) See also Alfred Kazin on the New Deal documentarians’ “signal literature of empiricism” which served as “evidence of how deeply felt was the urge born of the crisis to recover America as an idea—and perhaps only thus to build a better society in the shell of the old”: Kazin, *On Native Grounds: An Interpretation of Modern American Prose Literature* (1942; New York: Mariner Books, 1995), p. 489. Eudora Welty described feeling “seared” in her WPA job interviewing poor, rural southerners. Others used new methods to innovate with representational capacities. Dorothea Lange began in this period to interview the subjects she photographed, and saw her representations transformed by “gathering this sort of information” about their lives: Jonathan Raban, “American Pastoral,” *New York Review of Books*, 19 Nov. 2009, 56, p. 16. See also Erskine Caldwell and Margaret Bourke-White, *You Have Seen Their Faces* (1937; Athens: Univ. Georgia Press, 1995); and James Agee and Walker Evans, *Let Us Now Praise Famous Men* (1939; New York: Mariner Books, 2001).

a largely unlooked-at resource for describing “the nodes at which matter and meaning intersect.”³

To grasp the pitch at which this activity took place is to get close to a historiographically neglected aspect of Cold War social science. Since the official end of the Cold War, historiography of the American social sciences has often focused on debunking the era’s heroic narratives. Scholars show the grandest of grand theories to be bound up with eminently un-grand concerns over politics, money, professional advancement, or other sometimes unseemly motivators.⁴ Debunking can deflate universal-theoretical projects by revealing them to have been merely practical-political. Big social science becomes small. Turning to the period’s method-based literature may offer another view, for such writings take place at the juncture of highest-minded ideas and firmly footed practices, of grand theories and day-to-day minutia. Not separating out these two registers—that is, not diminishing one to reveal the other—allows one to focus on the combinatory crux, the place where people get defined and things get made.

What kinds of things? As a way to begin answering this, let me pause to sketch a fuller picture. An explosive quality to postwar American life has often been remarked on, usually in reference to the overlapping spheres of politics, economics, the military, physics, birthrate, culture, pop culture, and home-ec devices. Arthur Miller characterized America itself in this time as undergoing “the biggest boom in the history of the world.” Commentators and opinion makers saw atomic explosions as mere symbols of—and even dwarfed by—the extent and scale of social change: big, loud, powerful, and fundamental. In the early Cold War, American social science was itself booming, with a new moniker to mark this new status: the behavioral sciences. This surge received much comment at the time (from participants, for example, given to remark that the Manhattan Project inspired

³ On “working documents” see Ron Robin, *The Making of the Cold War Enemy: Culture and Politics in the Military-Intellectual Complex* (Princeton, N.J.: Princeton Univ. Press, 2001), p. 8. The phrase on “nodes” refers to the usefulness of considering “things” and the practices associated with them when studying science: see Lorraine Daston’s Introduction to Daston, ed., *Things that Talk: Objects Lessons in Art and Science* (New York: Zone Books, 2007), p. 16 (quotation). More broadly, Merton in his role as sociologist of science spoke of “strategic research materials” which might include strategic research sites, objects, or events. These are useful to the sociologist or historian of science because they “exhibit the phenomena to be explained or interpreted to such advantage and in such accessible form that they enable the fruitful investigation of previously stubborn problems and the discovery of new problems for further inquiry.” My claim is that the methodological materials described here constitute a key set of strategic research materials. See Robert K. Merton, “Three Fragments from a Sociologist’s Notebook: Establishing the Phenomena, Specified Ignorance, and Strategic Research Materials,” *Annual Review of Sociology*, 1987, 13:1–28, p. 3.

⁴ Pursuing this “debunking” strategy, important and groundbreaking works have addressed the history of Cold War expert knowledge, and particularly the social sciences and behavioral sciences; key contributions include Michael A. Bernstein’s, “American Economics and the National Security State, 1941–1953,” an essay in “The Cold War and Expert Knowledge: New Essays on the History of the National Security State,” *Radical History Review*, 1995, 63:8–27; Laura Nader, “The Phantom Factor: Impact of the Cold War on Anthropology,” in *The Cold War and the University: Toward an Intellectual History of the War Years*, ed. Noam Chomsky (New York: New Press, 1997), pp. 107–146; David Price, “Cold War Anthropology: Collaborators and Victims of the National Security State,” *Identities*, 1998, 4:389–430; and Christopher Simpson, “Universities, Empire, and the Production of Knowledge: An Introduction,” in *University and Empire: Money and Politics in the Social Sciences during the Cold War*, ed. C. Simpson (New York: The New Press, 1998). Such literature has often originated within a discipline—say, anthropology—and this may explain the vigor with which certain actors and programs are unveiled as subject to (borrowing from Nader, “The Phantom Factor,” p. 108) the “phantom factor,” that is, a complex of external factors and forces that contributed to a putatively neutral and universalist-aspiring social science. Overall, these works aim to “add context” to disciplinary history where context has been hidden or not talked about for decades. On debunking strategies implicit in science studies see Lorraine Daston, “Science Studies and the History of Science,” *Critical Inquiry*, 2009, 35:798–813, p. 801.

them in turn to try their hand at “splitting the social atom”), and since then by historians.⁵ If nothing else, it seemed, the heyday had to do with theory’s sway. “The World is the World through its Theorists” were the Wallace Stevens lines Robert Bellah borrowed to evoke the day’s valorization of its thinkers and theories: grand ideas and the grand men who had them. (“Nobody’s Theories Were Bigger,” headlined the *New York Times* obituary for Talcott Parsons, who once epitomized sociology’s aspirations.) What has been as yet little discussed is the Cold War as a *methodological* boom time for the social sciences, in which experts turned a fine-tuned, hyperfocused eye on their dedicated tools, the “special instruments,” “special procedures,” and “special rooms” of the social sciences. Systematicity and standardization were the goals of what one scholar has dubbed an “administrative rubric” but not in a dull or winnowing sense; rather, the sheer volume of methodological manifestos and manuals, as well as the mounting enthusiasm of their producers, are of note.⁶

Merton’s method resulted in at least two things: a full-fledged practical device (the focus group) and a machine that never existed (the introspectometer). My claim is that the ultimate technology—the telling technology, so to speak—was the latter, the one that never was, or to be more exact the one that was not anything more than a few paragraphs in a technical manual. Following this Mertonian developmental arc will show how sci-fi aspirations and exorbitant fancies can be seen to inhabit even the most straightforward how-to manual and ready-to-use data collection protocol.

THE NEXT STEPS

A decade or so later, in 1941, Merton’s hard-won know-how came to the fore. Taken impromptu one night from Paul Lazarsfeld’s dinner party to the radio lab where Lazarsfeld was testing new methods, Merton (as he recalled many years later) immediately drew

⁵ See the Ron Rifkind interview with Arthur Miller, *Bombsite* 49, Fall 1994 (<http://www.bombsite.com/issues/49/articles/1821>). Talcott Parsons delighted to report that his children were given to marching around the house with toy megaphones blaring, “The Sociology is About to Begin Said the Man with the Loudspeaker.” Parsons made the social-atom-splitting comment most famously in his presidential address to the American Sociological Association, as one of his students, Clifford Geertz, recalled: “And for a while it really seemed so.” See Clifford Geertz, “Disciplines,” *Raritan*, 1995, 14:65–102, p. 66. On the growth of behavioralism and the new entity of behavioral sciences see also Hunter Crowther-Heyck, “Patrons at the Revolution: Ideas and Institutions in the Behavioral Sciences,” *Isis*, 2006, 97:420–446; Nils Gilman, *Mandarins of the Future: Modernization Theory in Cold War America* (Baltimore: Johns Hopkins Univ. Press, 2003), pp. 115–117; Dorothy Ross, “Changing Contours of the Social Science Disciplines,” in *Cambridge History of Science*, Vol. 7: The Modern Social Sciences, ed. Ross and Theodore Porter (Cambridge: Cambridge Univ. Press, 2003), pp. 229–234; and Ron Robin, Ch. 1 “Inventing the Behavioral Sciences,” in *The Making of the Cold War Enemy* (cit. n. 3) (Princeton, N.J.: Princeton Univ. Press, 2001).

⁶ For Bellah’s quotation of Wallace Stevens see Robert Bellah manuscript dated 28 Aug. 1979 for the eulogy delivered at the annual Sociology meeting, “The World is the World through its Theorists. In Memory of Talcott Parsons (1902–1979),” Harvard Archives, Parsons Papers, HUGFP, 15.2, Box 20. Robert Redfield refers to “special procedures” of the social sciences, as quoted in Oscar Lewis, “Controls and Experiments in Field Work,” in *Anthropology Today: An Encyclopedic Inventory*, ed. Alfred Kroeber (Chicago: Univ. Chicago Press, 1953), pp. 188–189. Special instruments (the “interaction recorder”) and the Special Room (an experimental room with adjoining observation room) were trademarks of Harvard sociologist Robert Freed Bales, the action-oriented partner of Talcott Parsons at the Laboratory of Social Relations: see, for example, Robert Freed Bales, *Interaction Process Analysis* (Chicago: Univ. Chicago Press, 1950). For an insightful study of the fine-tuning of social scientific tools for polling, sampling and quantifying the American public see Sarah Igo, *The Averaged American: Surveys, Citizens, and the Making of a Mass Public* (Cambridge, Mass.: Cambridge Univ. Press, 2006). On “administrative rubric” see Jonathan Sterne, “C. Wright Mills, the Bureau of Applied Social Research, and the Meaning of Critical Scholarship,” *Cultural Studies*, 2005, 5:65–94, p. 70. For Talcott Parson’s obituary see “Nobody’s Theories Were Bigger,” *New York Times*, 13 May 1979.

on his street-level work. At the radio lab he was watching the birth of a technology, primitive but with potential. Subjects in armchairs pressed buttons to register their emotional response (pro or con) to a radio broadcast, and the Lazarsfeld-Stanton Program Analyzer then tabulated composite results and presented their contours graphically. Afterwards, Lazarsfeld's assistants interviewed respondents to probe the subtleties of their ups and downs. Observing this last step, Merton frowned. Instantly he knew what was going wrong. Lazarsfeld's team of interviewers was both too directive, for they were guiding people's answers, not allowing a full range of responses, and yet too free, for they neglected to target areas of desired inquiry. Instead of alienating his host with exacting criticism, Merton seems to have endeared himself, and this encounter inaugurated several decades' worth of collaboration. (Another story.) Here, however, it is a methodological thread we are tracing—the emergence of a critical Cold War technique, as I will argue, and the emergence of “methodological thought” itself as a preeminent concern—and so we must bypass the story of that fruitful partnership, and of the institutions and patrons that bolstered it, to follow the technique itself through a few further iterations.⁷

The next step for Merton's interview technique took place in the course of his work with the Research Branch of the Army's Information and Education Division. There, he and research partner Herta Herzog—the soon-to-be scientific doyenne of Madison Avenue marketing, and also then Lazarsfeld's wife—together polished a method for interviewing GI's one at a time or in groups.⁸ The method became more penetrating. Their assigned topic was to investigate whether propaganda films could build morale. Could exposure to graphic images and information stimulate soldiers' motivation to fight? At what moment might a reversal occur in a soldier's willingness to risk his life and possibly die? By means of which exact cue or “effective stimulus” (which “x or pattern of x's”) did a measurable change happen? The post-film interview was the key to discovering the answer with exactitude. Always in Merton's mind was the accumulated experience of the past decade. Question-asking and listening—the “art and craft of interviewing,” as he came to call it—were not simple matters. They were at root technical, but they were also total. (“Total” in the sense that the researcher sought a full portrait of the inner and outer impulses that led a subject to behave one way or another. In this way the interview rose in epistemological esteem. It constituted in and of itself a special situation, and a rich one. “This situation”—the interview encounter, Merton later observed, referring to his years of working with it—“strikes me as providing almost privileged access to people's states of mind and affect.”⁹

The privileged situation became more privileged at the Bureau of Applied Social

⁷ “Methodological thought” is a phrase from Jennifer Pratt's pathbreaking *A History of Sociological Research Methods in America* (Cambridge: Cambridge Univ. Press, 1999), p. 2.

⁸ Herzog worked closely with Merton for the Research Branch and subsequently took her academic and war *bona fides* to become a vice president for marketing at McCann Erickson. The agency acquired through Herzog exclusive commercial rights to the Lazarsfeld-Stanton Program Analyzer; in addition, Herzog innovated in developing with ecologist E. H. Hess of the University of Chicago an “Eye Camera” that recorded pupil-dilation in the presence of a product or scenario. On Herzog see Elizabeth M. Perse, “Herta Herzog,” in *Women in Communication: A Biographical Sourcebook*, ed. Nancy Signorielli (Westwood, Conn.: Greenwood Publishing, 1997), pp. 202–211.

⁹ The concept of “effective stimulus” derived from wartime work, according to Merton; it appears in published form in Merton *et al.*, *The Focused Interview* (cit. n. 1), p. 6. Note that Merton further refined the *post hoc* interview in additional, extensive wartime work published as *Mass Persuasion*, in which project teams systematically interviewed over 2,000 radio marathon respondents—people who pledged to buy war bonds, responding to an airborne message of a Kate Smith war bond drive, and those who resisted it. Robert K. Merton, Marjorie Fiske, and Alberta Curtin, *Mass Persuasion: The Social Psychology of a War Bond Drive* (New York:

Research in the mid-1940s. Merton and others reworked the fledgling GI-based method, testing it on a range of subjects exposed to print pamphlets, audio bulletins and broadcasts, and film clips. Drawing on these experiences, Merton along with Patricia Kendall in 1946 debuted what they described as a new social research technique, the “focused interview.” The authors named as their quarry nothing less than “subjective experiences.” The best way to trap this inherently elusive entity was to search for ways to name the structure of the experiences people had when exposed to an “objective situation”—that is, a designated and to some degree “controlled” situation. (Five types of situations were said to qualify: a person or group of people who read a written pamphlet, listened to a radio program, watched a propaganda film, entered a controlled situation, or took part in a scientific experiment.) By calling the situation objective, the researchers meant to indicate that this part of the encounter was known, understood, and preemptively analyzed. As a result, the subject’s or group’s responses to it—particularly “symbolic or functional silences, ‘distortions,’ avoidances, or blockings,” and all sorts of “apparent inconsistencies”—highlighted themselves. A “flow of concrete and detailed reporting” then followed. Note the oxymoronic language. By means of this technique, Merton and Kendall claimed, the “concrete” could be made to “flow,” like water from a stone, while still retaining its detailed reliability. “Finally, a clear picture of the total response emerges,” the authors stated.¹⁰ Their logic was this: the very texture of a life as it was unfolding, warp and woof, texture and temporality, became free for access when you used this well-developed research interview technique.

So far we have followed a developmental thread from forgotten men to radio listeners to soldiers to all-purpose subjects. In each case, the method got better, more refined, more satisfactory. All this is very well, but neglects what would otherwise seem an incidental footnote. In the process of these successive revisions arose a vaunted machine—a sort of tandem ghost—that worked even more felicitously than the actual technique. It would give universal access to the consciousness of no one less than everyman, in nothing less than every situation. Many have noted that the focused interview was the nascent form of that soon-to-be ubiquitous political-marketing tool, the focus group. Less noted is that somewhere further along in his manual Merton took up a tangent. Along with the focused interview, which according to its primary inventor was already quite effective, Merton introduced an imaginary machine, not yet built, and perhaps not ever to be built, that was the ultimate social science technology. It was a dream of a machine that also gave access to people’s dreams (and everything else they thought, felt, or saw). It could provide not only a route into people’s subjective experiences but also “in cinematic style” a sort of real-time description of an unfolding life experience *rendered as data*.¹¹ This was the introspectometer.

Harper and Brothers, 1946). All quotations in the paragraph are from Merton and Patricia Kendall, “The Focused Interview,” *American Journal of Sociology*, 1946, 51:451–557 p. 553.

¹⁰ All direct quotations in the paragraph are from *ibid.* on pp. 541–2 and *passim*; the five prime situations are described on p. 547.

¹¹ That an iteration of the focused interview metamorphosed, within a decade, into the soon-to-be ubiquitous focus group made Merton the incidental inventor of a nearly \$1 billion a year industry (a fact about which Merton expressed rue late in life, joking he never had a chance to share the profits). Merton’s role is no secret, although it should be noted that the use of the focus group is a technique that attracts very little historical interest. Historians of the method are as rare as practitioners are common. However, one of its most prominent boosters, Frank Luntz, acknowledges its roots in Merton’s research on his website: <http://www.pollingreport.com/focus.htm>. On Merton’s machine see Merton *et al.*, *The Focused Interview* (cit. n. 1), pp. 22–23.

USEFUL NEAR-EXPERIMENTS

It took only two paragraphs in the middle of *The Focused Interview* for Merton to conjure his “hypothetical machine,” but in that short space, amid exacting prescriptions for carrying out a special social-scientific interview, he allowed himself to pursue musings about the unique powers of literary techniques and how social science might borrow from them. The symbolists’ interior monologue, subsequently perfected in stream of consciousness writing, constituted in his view the closest extant “device” for this purpose of achieving subject-oriented omniscience. Social science, however, might go some way further than either James Joyce or Edouard Dujardin:

In place of literary craft, we can conceive a technological contrivance—an introspectometer, so to say—which would record, in accurate and intimate detail, all that the individual perceives as he takes part in social interaction or is exposed to various situations. If the raw stuff of the experience is to be adequately recorded, this hypothetical machine would have to synchronize its record of the situation and the details of selective responses to it. It would provide, in other words, *a motion picture of the individual's stream of experience as he is engaged in the situation*. And to avoid the distortions that might result from intrusion of the apparatus into the situation, it would be necessary that the individual not be aware that the apparatus was at work.¹²

Merton did not hesitate to aver that the beginnings of such an instrument “have of course been made” in several nascent forms, including the Lazarsfeld-Stanton Program Analyzer and the Bruner-Postman tachistoscope-coupled-with-interview technique. (One might add others, as discussed below.)¹³ As with much of Cold War technology, not least the atomic bomb, the dreamed of machine was also a nightmare. After all, the person under scrutiny in the most intimate manner must never know the machine was at work siphoning off her experiences. “That such an instrument would make for a collective nightmare if it were used for anything but disinterested inquiry is evident, just as it is evident that it would be a powerful instrument for discovering new truths about human behavior,” observed Merton and his coauthors in a perspicacious if perhaps understated footnote.¹⁴ Notwithstanding its Philip K. Dick-style alternative applications, the introspectometer’s neo-Enlightenment, mirror-of-nature goal was to uncover new truths about human behavior. To achieve this, the unreal machine needed to combine a transparent rationality (activated via viewing techniques), a qualified objectivity (holding subjective data within a putatively known situation), and a capacity to trigger and capture something called *flow* (that experiential matrix between subject and situation, called life).

The fantasy that was the introspectometer arose at a time of tremendous methodological enthusiasm and widespread hopes for technology-based teleology. Calls for “rich innovations in method,” as one formulation had it, echoed from all kinds of working groups, institutional nexuses, and intellectual *cul de sacs*. In a tone of modest pride-of-accomplishment, Merton and Kendall framed their own focused interview as an “addition, however slight, to the growing number of critical self-examinations of method by soci-

¹² Merton *et al.*, *The Focused Interview* (cit. n. 1), pp. 22–23.

¹³ See J. S. Bruner and L. Postman, “An Approach to Social Perception,” in *Current Trends in Social Psychology*, ed. Wayne Dennis (Pittsburgh, Penn.: Univ. of Pittsburgh Press, 1948), pp. 71–118.

¹⁴ Merton *et al.*, *The Focused Interview* (cit. n. 1), p. 23, n. 1.

ologists and psychologists which lead to closer scrutiny of prevailing procedures.”¹⁵ The numbers continued to grow and, along with them, the available options. So close did the goal of true systematization and experimental readiness appear that a chiding tone often accompanied notice that certain methods were insufficiently secure and inadequately standardized, and therefore unable to give replicable results. (As in, these dalliers are holding us all back.) Less established methods, having sprung up weedlike, required a guiding hand to insure quality and useable results. Such a cultivating hand could turn weeds into flowers: “Systematic research on the interview method blossomed in the 1940s,” one researcher observed, naming only one area of blossoming. Other methods emerged out of particular disciplines and spread to others, or adapted to common cross-disciplinary team projects. And behind, or beside, resided a common cause: groups of people with specific social scientific training “deeply interested in learning wherever possible from the other social sciences,” as a political scientist understatedly put it.¹⁶

What was new, starting in the mid-1940s, was not so much a concern for exactitude of method as a self-consciousness about method—and this self-consciousness was so well-honed as to be nearly meta-methodological. “The self-examination of social science,” in Robert Redfield’s phrase, was an ongoing process, almost like a dramatic play in which successive published contributions were likened to “events.” A brief comparison with pre-World War II procedures highlights the change: although much work from the interwar period addressed methods used in particular studies, and it was clear that exemplars such as *The Polish Peasant* rested on methodological innovations, still the consensus was that development remained slow. Ethnographic fieldwork arose with Alfred Haddon’s 1898 Torres Strait expedition and came to fruition with Bronislaw Malinowski’s 1922 *Argonauts of the Western Pacific*, yet Kluckhohn could complain decades later of finding it “astonishing” that ethnologists had so uniformly failed to articulate their techniques properly, or to speak in a coherent way to scholars outside of their own field.¹⁷ For the postwar American social sciences, the hope was that a repository of special methods, when properly elaborated in easy-to-use compendia, would enable a

¹⁵ Clyde Kluckhohn, “Southwestern Studies of Culture and Personality,” *American Anthropologist*, 1954, 65:685–697, p. 691; and Merton and Kendall, “The Focused Interview” (cit. n. 9), p. 545.

¹⁶ For the quotation on “systematic research” see Dan Gollen, Foreword to Merton *et al.*, *The Focused Interview* (cit. n. 1), p. x. Whole careers were made not only on the development of an effective, productive test, but on the second-generation streamlining and standardizing of such tests. One example of many is Charles E. Thompson, “The Thompson Modification of the Thematic Apperception Test,” *Rorschach Research Exchange*, 1949, 13:469–478. For the second quotation see Robert A. Dahl, “The Behavioral Approach in Political Science: Epitaph for a Monument to a Successful Protest,” *American Political Science Review*, 1961, 55:763–772, p. 766.

¹⁷ Robert Redfield, Foreword to Louis Gottschalk, Clyde Kluckhohn, and Robert Angell, *The Use of Personal Documents in History, Anthropology and Sociology* (New York: Social Science Research Council, 1945), pp. vii–xi, on p. vii. Clyde Kluckhohn, “The Personal Document in Anthropological Science,” in *ibid.*, pp. 79–162, on p. 106. Florence Kluckhohn contributed to systematizing field work for audiences outside anthropology with “The Participant-Observer Technique in Small Communities,” *American Journal of Sociology*, 1940, 56:331–343. Allan Holmberg further refined participant-observation by introducing the “participant-intervention” method for use in Vicos, a Peruvian village leased by Cornell University and declared an experimental field site for new methods. See Allan Holmberg, “Experimental Intervention in the Field,” in *Peasants, Power and Applied Social Change: Vicos as Model*, ed. Henry Dobyns, Paul Doughty, and Harold Lasswell (Beverly Hills, Calif.: Sage, 1964); pp. 21–32. Bronislaw Malinowski, *Argonauts of the Western Pacific* (1922; Prospect Heights, Ill.: Waveland Press, 1984); see especially the introductory chapter on method of inquiry. See also W. I. Thomas and Florian Znaniecki, *The Polish Peasant in Europe and America: Monograph of an Immigrant Group*, vols. 1 and 2 (Chicago: Univ. of Chicago Press, 1918), vols. 3–5 (Boston, Mass: Badger Press, 1919, 1920). A SSRC series on the “Appraisal of Research” included Herbert Blumer, *An Appraisal of Thomas and Znaniecki’s “The Polish Peasant in Europe and America”* (New York: Social Science Research Council, 1939); and Gottschalk, Kluckhohn, and Angell, *The Use of Personal Documents in History*.

new flow of data as well as solidity of purpose. Researchers tried out particular methods self-consciously, with verve, and with an eye to reporting back on efficacy. Continual self-survey led to a next step, to “undertake methodological experiments,” that is, to combine newly systematized methods in new ways.

In addition to emphasis placed on systematizing, formalizing, and recombining methods, the late 1940s and 1950s saw great technophilic fervor for what could be called the “x-ray” approach to seeing more penetratingly the core mechanisms of what it meant to be human. In the process, what it meant to be human was flattened and foreshortened in a post-Freudian manner, as Galison and others have pointed out. One sees a common urge to develop a “window-in” method in diverse and overlapping fields. In this, the introspectometer was not an outlier. Consider, for example, the projective test offered as x-ray technology. As projective test inventor Henry A. Murray described his “Thematic Apperception Test,” serial images were designed to be so stimulating that they would spring loose stories revealing the test-taker’s preoccupations without the test-taker actually knowing she had revealed anything: “As a rule, the subject leaves the test happily unaware that he has presented the psychologist with what amounts to an X-Ray picture of his inner self.” Groups of such tests formed “batteries” and, when researchers administered them rapidly and regularly to as many people as possible in a field area, they became particularly efficient techniques for gathering massive amounts of “subjective materials.” In the culture-and-personality movement among anthropologists and psychologists, so eager was the embrace of these new tools that no less a figure than Alfred Kroeber felt the need to hope that new comprehensive work would help “silence any notions that part of the motivation of field studies in culture and personality is a love of playing with favorite techniques.”¹⁸ Methodological zeal could appear as a kind of fetishism. By analogy, methodologists argued, other techniques—even unaided, human-eye-based observation itself—shared in the special status of such tests, used to gain access to otherwise inaccessible materials.¹⁹

What I am identifying as tool-based social science (or tool-based revisionism, perhaps) calls into question lingering views of the 1940s and 1950s as concerned with repressing actual laboratory subjects and potential real-life subjects into a submissive conformity. Rather, there was an explosion and proliferation of ways to achieve this “norm.” The normalization process was not so much a clamping down as an amping up. As David Serlin’s *Replaceable You: Engineering the Body in Postwar America* serves to remind

¹⁸ Peter Galison, “Image of Self,” in *Things that Talk*, ed. Daston (cit. n. 3), pp. 257–296. Henry A. Murray, *The Thematic Apperception Test: A Manual* (Cambridge, Mass.: Harvard Univ. Press, 1943), p. 4 (quotation). “Subjective materials” was a commonly used locution. Another description of how the tests worked was that the subject faced with stimulating materials such as vivid pictures or inkblots “‘projects’ himself and his own world of thoughts and feelings onto the stimuli presented to him”: Jules Henry and Melford Spiro, “Psychological Techniques: Projective Tests in Field Work,” in *Anthropology Today*, ed. Kroeber (cit. n. 6), p. 419. See Kroeber, Foreword to *Microcard Publications in Primary Records in Culture and Personality* (Madison, Wis.: Microcard Foundation, 1957), available at <http://lccn.loc.gov/sv92080005> [Library of Congress permanent link].

¹⁹ Cutting-edge researchers such as anthropologist Jules Henry and psychologist Ivan Mensh argued that “direct observation” was best understood as sharing the mechanics and power of projective tests. Thus the human capacity to see with the “naked eye” appeared on a continuum with other, technology-aided faculties. Likewise, Melville Herskovits proposed a parallel method called the “hypothetical situation” that combined the advantages of projective test technologies with a literature-inspired technique to enable the ethnographer to “probe” those materials that subjects were unable or unwilling to talk about: “in essence the method of the hypothetical situation is a novelistic technique, the utility of which arises out of its character as a kind of projective device.” See Mensh and Henry, “Direct Observation and Psychological Tests in Anthropological Field Work,” *American Anthropologist*, 1953, 55:461–480; and M. J. Herskovits, “The Hypothetical Situation: A Technique of Field Research,” *Southwestern Journal of Anthropology*, 1950, 6:32–40, p. 39.

readers: “Rather than merely containing physical or sexual differences, medical procedures served as tools of consensus building, and the fanfare they generated helped to promote medicine as the apotheosis of domestic engineering.”²⁰ This was true also of social science procedures. A second generation social-engineering view of the postwar period, then, emerges, in which an endless productivity and utopian artificiality came to dominate people’s hopes and dreams for transformation.

Social sciences, as the working arm of much of this transformation, had never been so central to the American project. Science-fiction-style aspirations rose to the surface, and sometimes gave voice. “[New methods] are making it possible for the researcher to follow the events he is studying in a microscopic and systematic way to an extent that would only have been dreamed of by a science-fiction writer ten years ago,” crowed the Harvard Laboratory of Social Relations’ Robert Freed Bales.²¹ A true-believer feeling of being *on the verge* of a new regime and range of possibilities permeated social scientists’ methodological jousting. A properly microscopic eye sharpened along with systematic procedures. Existing walks and ways of life (the objects of social research) became building materials for the design of new environments.

Thus it is not surprising that the question of what purpose methodological entrepreneurship served—was it more than love of *minutia*, navel gazing, and fine-tuning, *ad infinitum*?—leads potentially to a new perspective. The answer is the pull of experiment. Improving method led close, to the very tipping point, of true experimental power within the realm of the social sciences. Just so, Merton and Kendall argued pivotally that their focused interview “provides a *useful near-substitute* for . . . a series of experiments; for, despite great sacrifices in scientific exactitude, it enables the experimenter to arrive at plausible hypotheses concerning the significant items to which subjects responded.”²² The funny phrase “useful near-substitute” captures the paradox of what it means to pursue objects at the edge of experimental-experiential reality. It referred to experiments that might take place, could never take place, but already were taking place. In this sense they mark the flip side of a recent definition of ideology as “a means of producing a more formidable unreality.”²³ Methodological thought and practice may be said to produce a formidable near- or quasi-reality.

With the passage of decades, a merging of the Cold War’s actual and hypothetical machines seemed to occur: which was real and which was not quite real? Which experiments were run and which were only thought of? Was the Doomsday Machine actually built or not, or (somehow) both?²⁴ The interweaving of the two seemingly definite

²⁰ David Serlin, *Replaceable You: Engineering the Body in Postwar America* (Chicago: Univ. Chicago Press, 2004), p. 3.

²¹ Robert Freed Bales, “The Strategy of Small Group Research,” presented to the Sociological Research Society of the American Sociological Society, Sept. 1950, Denver, Colorado, Rockefeller Archive Center, RF: R.G. 1.1, Series 200S, Box 521, Folder 4449.

²² Merton and Kendall, “The Focused Interview” (cit. n. 10), p. 543 (emphasis added). See the similar argument in *Mass Persuasion* in which the study of a “real life situation” can be considered to act as “a prelude to experimentation, by providing a realistic basis for selecting the variables to be included in experiments. It provides a bridge, so to speak, from the concreteness of everyday life to the abstractions of experimental inquiry”: Merton *et al.*, *Mass Persuasion* (cit. n. 9), p. 11.

²³ Howard Hampton, *Born in Flames* (Cambridge, Mass.: Harvard Univ. Press, 2008), p. 36.

²⁴ The answer is, apparently, both; the machine that overheated the imaginations of some was never realized, but an actual if somewhat pedestrian system called Perimetr was activated along these lines by the Soviets in the early 1980s. See *Slate* article: <http://www.slate.com/id/2173108/pagenum/all/>. On a Strangelovian dream of automated nuclear destruction by a Doomsday machine see P. D. Smith, *Doomsday Men: The Real Dr. Strangelove and the Dream of the Superweapon* (London: St. Martin’s, 2007). Likewise, the fake social science

states—what could be more clear than the divide between real and unreal, at least in the realm of machines?—characterizes the Mertonian introspectometer as well. The hypothetical machine: it was both nearly built, and never possible to build. It existed in quasi form, but never could exist. What was claimed about it was not so very different from the results of what already was being done.

This curious ontological status provides an entry point for reassessing Cold War social science and placing its practitioners' fantastic sounding claims at the center of their practice. (An echo can be heard today in claims for the power of introspectometer-like machines such as fMRIs.) If all this cooperative enterprise was later, and mostly critically, labeled “abstracted empiricism” (C. Wright Mills), “sterile social science” (Brewster Smith), and “the very model of a modern, policy-conscious, corporate social science” (Geertz), still its methodological aspirations highlight a paradox built in to the shared project of using high-tech methods to establish the hope of a super-secure science of social and human relations. A productive ambiguity comes across in the echt Parsonian descriptor, the “impossible possible.”²⁵ Things, it was felt, could be done that had never been doable in the past. New funding, new prestige, and new research alliances pushed open a window once closed; all this munificence often as not created little more than new opportunities for people talking method and exchanging tools who otherwise might not have. These effects should not be underestimated. After all, the dream of an introspectometer, a regular and reliable way to discern the thoughts of others, was certainly nothing new. It was only the way Merton conjured it up, and the way researchers' actual inventions mimicked its working, that were remarkable. The ‘strangely neglected’ genre of procedural manuals and field guides with their synaptical co-referencing, cross-disciplinary congratulations, and excitatory group thinking produced a common understanding of exactly what an introspectometer might be.

“Report from Iron Mountain” (1968) came from real think tank insiders and was very often received as real despite its unveiling as parody. The supposed clandestine report postulated that the most stable national course was to pursue a state of permanent war—the “war system.” See Robin, *The Making of the Cold War Enemy* (cit. n. 3), pp. 226–238. The place of the hypothetical threat of thermonuclear war as Herman Kahn expounded it, is explored skillfully in Sharon Ghamari-Tabrizi, *The Worlds of Herman Kahn: The Intuitive Science of Thermonuclear War* (Cambridge, Mass.: Harvard Univ. Press, 2005). The “power of . . . tools” to measure and promote creativity as an object of postwar research is discussed in Jamie Cohen-Cole, “The Creative American: Cold War Salons, Social Science, and the Cure for Modern Society,” *Isis*, 2009, 100:219–262, p. 243.

²⁵ Mills is quoted in M. Brewster Smith, “The American Soldier and its Critics: What Survives the Attack on Positivism?” *Social Psychology Quarterly*, 1984, 47:192–198. See Clifford Geertz, “An Inconstant Profession: The Anthropological Life in Interesting Times,” *Annual Revue of Anthropology*, 2002, 31:1–19; p. 3. On the “impossible possible” see Bellah, “The World is the World Through Its Theorists” (cit. n. 6).