Epistemological Dizziness in the Psychology Laboratory: Lively Subjects, Anxious Experimenters, and Experimental Relations, 1950–1970

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Abstract: Since the demise of introspective techniques in the early twentieth century, experimental psychology has largely assumed an administrative arrangement between experimenters and subjects wherein subjects respond to experimenters' instructions and experimenters meticulously constrain that relationship through experimental controls. During the postwar era this standard arrangement came to be questioned, initiating reflections that resonated with Cold War anxieties about the nature of the subjects and the experimenters alike. Albeit relatively short lived, these interrogations of laboratory relationships gave rise to unconventional testimonies and critiques of experimental method and epistemology. Researchers voiced serious concerns about the honesty and normality of subjects, the politics of the laboratory, and their own experimental conduct. Their reflective commentaries record the intimacy of subject and experimenter relations and the plentiful cultural materials that constituted the experimental situation, revealing the permeable boundaries between laboratory and everyday life.

> ***Above all, "observation" means that special care is being taken: the root of meaning of the word is not just "to See," but "to watch over." The scientist observes his data with the tireless passion of an anxious mother.***
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> **—Abraham Kaplan,** ***The Conduct of Inquiry*** **(1964)**

When Mike Freesmith, protagonist of the 1956 novel *The Ninth Wave*, volunteered to participate in a psychology experiment, the Stanford freshman walked to the laboratory by way of a long corridor lined with lobotomized, nearly catatonic rats. Once he arrived in the lab, the experimenters instructed him to press one of two colored cards that would be displayed at five-second intervals; if he pressed the correct color, a penny would be dispensed.

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Isis, volume 106, number 3. 0021-1753/2015/10603-0003$10.00

Initially puzzled by the experimental task, Mike soon realized, "quite intuitively, he was playing the two people in white coats." He needed only to figure out the pattern set in the machine: "Hell with the cards, Mike thought, play the people." Soon he was performing without error, leading the experimenters to end the first trial prematurely because, they conjectured, their subject was being "positively motivated" by the pennies, not the colors (the intended stimulus). The next day's session went no better: instructed to respond to the cards in a cooperative situation with two other undergraduate subjects, Mike insisted on the correctness of his pattern hypothesis. The two experimenters then summarily dismissed Mike, directing him never to participate in an experiment again. Befuddled by his aberrant performance, they attributed it to "political selection."[1] For his part, Mike was stunned to learn that the experiment was in fact only a straightforward study of color preference.

Eugene Burdick's novel of the life of an impoverished American boy who rises to a powerful political position (yet ultimately fails) is as telling about postwar anxieties as is his subsequent novel, *Failsafe*, a scenario of the strategies, counterstrategies, secrets, and lies that trigger catastrophic nuclear attacks between the Soviet Union and the United States. Burdick's play-by-play rendering of a psychology experiment chronicles a psychology of the experiment that invisibly circulates alongside the formally hypothesized one and thereby exposes an excess psychology generated by subject and experimenter alike. Mike's calculus, in fact, rivals the investigators' hypothesis on cognitive grounds (he discovers a winning pattern); it even trumps the alternative hypothesis produced *in situ* by the experimenters. Burdick, trained in psychology, promoted the novel as a study of the individual basis of "irrational trends in politics."[2] By depicting a character who displays exemplary skills at rational calculation yet fails to be an honest performer, he complicated the divides between rational and irrational, real and apparent. Mike's paranoid-like suspiciousness paradoxically affirms rational autonomy just as the laboratory drama reverberates with preoccupations that have come to be associated with postwar America: conspiracy, surveillance, deception, self-deception, and anxieties about control. His hyporational style, guided by mistrust, ultimately prevails. Everyone in the laboratory is suspect, for confused and wary regard of hidden forces is not the prerogative of subjects alone. The alert, cunning (if disturbed) subject imperiled the scientists' autonomy and technical control, confusing them and posing continued, unpredictable threats to their experiment.

For many North American psychologists working during that period, human and nonhuman subjects alike appeared suspect as well as suspecting. The rats Mike observed were surgically incapacitated, their agency destroyed, but B. F. Skinner's fictional rats were not. In a 1956 *American Psychologist* article, he inserted a cartoon of a rat casually commenting to another just how gullible and deceived their human experimenters are. (See Figure 1.) The cartoon underscores Skinner's claim that "the organism whose behavior is most extensively modified and most completely controlled in research of the sort I have described is the experimenter himself." Even researchers in information and systems technologies voiced suspicions. The cyberneticist Norbert Wiener, for instance, assured his readers that nature cannot react to scientists' powerful techniques with "the deliberate purpose of confusing and frustrating us" but warned that "living nature" might react to scientists' investigations, possibly

[1] Eugene Burdick, *The Ninth Wave* (Boston: Houghton Mifflin, 1956), pp. 40–41, 44. For the epigraph see Abraham Kaplan, *The Conduct of Inquiry: Methodology for Behavioral Science* (San Francisco: Chandler, 1964), p. 127.

[2] Eugene Burdick and Harvey Wheeler, *Failsafe* (New York: McGraw-Hill, 1962); and Burdick, *Ninth Wave*, author's note on dust jacket (quotation).

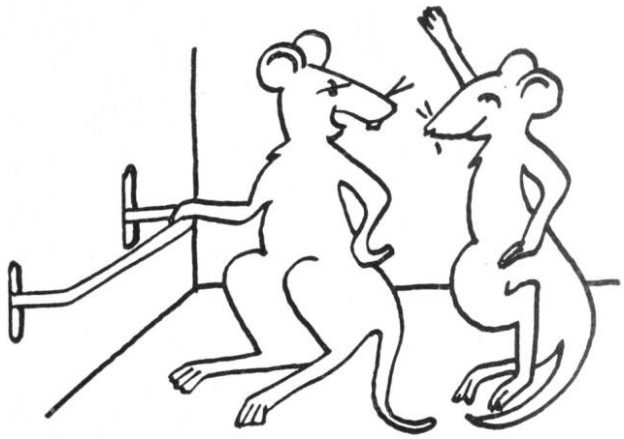

Figure 1. "Boy, have I got this guy conditioned! Every time I press the bar down he drops in a piece of food." Quoted from B. F. Skinner, "A Case History in Scientific Method," *American Psychologist*, 1956, *11*:221–233, on p. 232.

with "the intention, which is frequently unconscious, of bamboozling that audience."[3] Within scientific spaces as well as without, heightened worries about others cultivated ever greater scrutiny of what was real and what apparent.

Woven into Burdick's story is a second analogy of the laboratory and the world. Subject Mike is haunted by another postwar fascination: totalitarianism. His limited success in sussing out the experimenters' invisible controls lends ironic meaning to his surname, Freesmith. His walk through the psychology department to his assignment took him past cages of laboratory animals. The captive, surgically diminished, barely sentient rats metaphorically intimate science's control technologies and inhumanities, and their deteriorated state under experimental conditions anticipates Mike's own experiences. The caged rats are kin to the dogs described in Hannah Arendt's comparison of totalitarian citizens and the canine subjects of Ivan Pavlov's conditioning experiments. Totalitarian aspirations, Arendt claimed, require eradication of human spontaneity: "Pavlov's dog, the human specimen reduced to the most elementary reactions, the bundle of reactions that can always be liquidated and replaced by other bundles of reactions that behave in exactly the same way, is the model 'citizen' of a totalitarian state; and such a citizen can be produced only imperfectly outside of the camps." As never before, the word "experiment" was complicated, metonymically twinned with aesthetics of rationality and inhumanity alike. For Arendt, the concentration camps aimed not only to exterminate but also to "serve the ghastly experiment of eliminating, under scientifically controlled conditions, spontaneity" and transform "human personality into a mere thing, into something that even animals are not."[4]

These two equivalences—between the laboratory and an unruly, untrustworthy, intersubjective world and between the laboratory and a totalitarian state—are echoed in numerous reports issued by North American psychologists in the 1950s and 1960s. Such observations not

[3] B. F. Skinner, "A Case History in Scientific Method," *American Psychologist*, 1956, *11*:221–233, on p. 232; and Norbert Wiener, *The Human Use of Human Beings: Cybernetics and Society*, 2nd ed. (Boston: Houghton Mifflin, 1954), p. 188.
[4] Hannah Arendt, *The Origins of Totalitarianism* (New York: Meridian, 1958), pp. 456, 438.

only unsteadied the ontological status of the human research subjects but also opened spaces for refiguring those subjects—who themselves seem to have been evaluating the situation. Once assumed to be more or less reliable data producers, subjects appear otherwise in these scientific statements: subjects apparently are unreliable, confound experimenters' aim to distinguish apparent from real phenomena, and at times simply perform like caged animals. The scientist, too, became implicated in what seemed increasingly like an unreal laboratory where, on the one hand, technical controls constrict human subjectivity and, on the other, mutable if not cunning subjects blur the real. If, as the philosopher Joseph Rouse has proposed, "laboratory practices guide a massive continuing effort to reconstruct the world in the image of the laboratory," then such accounts of complicated subjects disturb epistemic boundaries between the laboratory and the world. One young experimenter lamented the way the many reports of laboratory problems caused "epistemological dizziness"; others simply expressed a certain anxiousness when reporting challenges to reconstructing the world in laboratory miniature.[5]

Reports of unruliness or politics in the laboratory do not accord with chronicles of modern psychology's rapid expansion, its ambitions to explain and manage human affairs, and an intellectual revolution that replaced behaviorism with cognitive psychology. Conventional histories overlook the routine interactions that transpire in experiments and, in keeping with the science's ethos, take the hundreds of thousands of individuals who have served as subjects to be stable and interchangeable entities whose dispositions are irrelevant to understanding the science.[6] Such oversights have persisted despite compelling work on the ontological dynamics of the human sciences and critical appraisals of the uses of human subjects.[7] Candid evidence as to experimenter–subject relations can be found in researchers' unpublished notes, communications, and laboratory logbooks.[8] Recent archival investigations of these materials are exposing the complex relations between experimenters and subjects.[9] However, the extent

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[5] Joseph Rouse, "A New Image of Science and Nature," lecture at Calvin College, Grand Rapids, Michigan, 20 Apr. 2006, p. 11; and Neil Friedman, *The Social Nature of Psychological Research: The Psychological Experiment as a Social Interaction* (New York: Basic, 1967), p. 5.

[6] One critic claims that such histories of science rely on realist assumptions not unlike those of the science they analyze. See Jouni-Matti Kuukkanen, "The Missing Narrativist Turn in the Historiography of Science," *History and Theory*, 2012, *51*:340–363.

[7] As noted below, concern about who populates psychology experiments increased significantly during the postwar decades. For more recent critiques see David O. Sears, "College Sophomores in the Laboratory: Influences of a Narrow Data Base on Social Psychology's View of Human Nature," *Journal of Personality and Social Psychology*, 1986, *51*:515–530; and Joseph Henrich, Steven J. Heine, and Ara Norenzayan, "The Weirdest People in the World?" *Behavioral and Brain Sciences*, 2010, 33:1–75.

[8] Responses from an APA survey of psychologists undertaken in the 1960s contain plentiful statements on difficult interpersonal problems encountered during research: American Psychological Association Archives, Containers 433–442 (1967–1973), Library of Congress, Washington, D.C.

[9] Weighty and illuminating evidence of subject and experimenter interactions has been found in the unpublished notes and archives of experimental psychologists. In using these sources, however, historians are challenged to ascertain how experimenters understood these behaviors and interactions. An example of careful interpretation is found in David Serlin, "Carney Landis and the Psychosexual Landscape of Touch in Mid-Twentieth-Century America," *History of Psychology*, 2012, *15*:209–216. A number of scholars have examined the archival records of Stanley Milgram's obedience experiments. See Gina Perry, *Behind the Shock Machine: The Untold Story of the Notorious Milgram Psychology Experiments* (New York: New Press, 2013); Ian Nicholson, "'Torture at Yale': Experimental Subjects, Laboratory Torment, and the 'Rehabilitation' of Milgram's 'Obedience to Authority,'" *Theory and Psychology*, 2011, *21*:737–761; Stephen Gibson, "Milgram's Obedience Experiments: A Rhetorical Analysis," *British Journal of Social Psychology*, 2013, 522:290–309; and Gibson, "'The Last Possible Resort': A Forgotten Prod and the *in situ* Standardization of Stanley Milgram's Voice-Feedback Condition," *Hist. Psychol.*, 2013, *16*:177–194. Network analyses are revealing cross-laboratory relations between various actors. See Michael Pettit, Darya Serykh, and Christopher D. Green, "Multispecies Networks: Visualizing the Psychological Research of the Committee for Research in Problems of Sex," *Isis*, 2015, *106*:121–149.

of these complications in postwar psychology is indicated by the fact that they were publicly reported. After the war and over the next several decades, North American experimental psychologists voiced multiple worries about experimental activities. Their public critiques, commentaries, and empirical papers span the research fields of perception, learning, personality, social psychology, and abnormal psychology.[10] Although relatively short lived, the anxious observations disturbed science-as-usual, giving rise to unconventional testimonials (including political essays and fiction) and new vocabularies.

The present study heeds Peter Galison's advice "to take the experimenter's anxiety seriously" and explore how that anxiety might be productive. This case of experimental psychologists' anxiety proves to be productive for historians as well as for the scientific actors. For historians of science, examining these symptoms of anxiety reveals typically unnoted scientific practices. It discloses some of the ways that the subject–experimenter system has been at once sustained and challenged by prevalent cultural conditions, including Cold War tensions, social stereotypes, and widespread psychoanalytic thinking. It shows how the postwar cultural fascination with psychological explanations and "internalization" of psychological language inflected researchers' perception of laboratory troubles as they proffered psychological accounts of the already psychologized experiment. Additionally, attention to scientists' anxieties can inform the historical record. More generally, examining scientists' emotional tenor supports a historiographical aim to consider "human beings as both objects of and subjects acting upon science and politics."[11] For the actors (psychology researchers) considered here, laboratory-related worries were productive in providing a tangible purchase on problems newly posed by the science's rapid expansion, increasingly heterogeneous subject pools, and moral concerns about human research that emerged after the war.[12] Yet the apprehensions also signaled systemic problems, as anxiety often does: they circled around significant quandaries about human nature and a pressing desire to distinguish the real (human nature) from appearances (behaviors, artifices). The following analysis begins by describing these quandaries and the science's purchase on experimental relations. The main sections follow psychologists' concerns as they fixed on experimental objects, the experimenters, and, ultimately, the entire experimenter–subject system.[13] Some analysts regarded the problems as epistemological, while others found them to be political. A few writers interpreted researchers' anxiety as an essential, productive aspect of experimenter–subject relations. However, most psychologists acted otherwise, by translating the concerns into empirical problems or developing technical

[10] Although these problems were reported in nearly every research domain and began appearing soon after the war, the discipline's lore misdates and misplaces the turmoil to a "crisis" of social psychology in the late 1960s and the 1970s.

[11] Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: Univ. Chicago Press, 1997), pp. 233–234; and Greg Eghigian, Andreas Killen, and Christine Leuenberger, "Introduction: The Self as Project: Politics and the Human Sciences in the Twentieth Century," *Osiris*, 2007, 2nd Ser., 22:1–25, on p. 4.

[12] Expansion of the social sciences coincided with expansion of colleges and universities and an increasing diversity of the student population. These changes in the size and makeup of the subject pool likely fostered concerns about "untruth" that Ken Alder has associated with the rise of large organizations in American society. See Ken Alder, "A Social History of Untruth: Lie Detection and Trust in Twentieth-Century America," *Representations*, 2002, 80:1–33.

[13] This focus on the experimenter–subject system encompasses more than the actors' conception of what constitutes an "experiment," although it is indebted to studies of the history of the experiment in psychology. See Andrew S. Winston and D. J. Blais, "What Counts as an Experiment? A Transdisciplinary Analysis of Textbooks, 1930–1970," *American Journal of Psychology*, 1996, 109:599–616; Trudy Dehue, "From Deception Trials to Control-reagents: The Introduction of the Control Group about a Century Ago," *Amer. Psychol.*, 2000, 55:264–269; and Henderikus J. Stam, Lorraine Radtke, and Ian Lubek, "Strains in Experimental Social Psychology: A Textual Analysis of the Development of Experimentation in Social Psychology," *Journal of the History of the Behavioral Sciences*, 2000, 36:365–382.

controls. The concluding section considers how the technical refinements incorporated ontological reconfigurations whereby psychology's objects were increasingly seen as thinking, mostly rational, and autonomous beings whose thoughts could be measured through appropriate experimental controls. Once subjects were ascribed these characteristics and once experimenters were better managed, researchers no longer needed to observe with the "tireless passion of an anxious mother," as Abraham Kaplan recommended in 1964.[14]

HISTORIES AND ONTOLOGIES

A foundational condition of the human sciences is that the subject and the object are of a kind, the human kind. It is around this condition that Roger Smith oriented his history of the modern human sciences, noting, "There is something disturbingly paradoxical about a science that has for its subject the agent that creates the science." Even the act of standing back to observe human conduct "is a way of being human that, in turn, some other person will be able to study." This paradox of the subject–object relation has haunted human scientists' ambitions for objectivity: while they aspire to step outside themselves to observe, their very reflections and subsequent capacity to act differently suggest that those objective observations change events and actors. Even when human scientists adopt experimental practices from the physical and life sciences, they defer to rather than eliminate the fact that "agentic subjects" are responsible for experimental representations.[15] Modern psychology's commitment to producing objective knowledge about human behavior via controlled experiments remains susceptible to reflexive questioning. How do experimenters stand outside themselves as human kinds to generate objective knowledge uninflected by their own subjectivity? How do subjects respond to being cast as "scientific objects" and not autonomous beings? What knowledge of self and other informs these experimental practices? Psychologists' evolving responses to such questions have prompted shifts in nomenclature that have diverged from the philosophical understanding of the "subject" as the agent of action, as the one who perceives. Modern scientific epistemology has figured subjectivity as the antithesis of objectivity, which has come to be understood as the self-discipline and self-restraint requisite for eliminating "the mediating presence of the observer."[16] Late nineteenth-century introspection research took the experimenter/observer to be interchangeable with the object of observation who also was an "observer": both participants were taken to be objective observers, "subjects" in the classical philosophical sense. Introspection's demise in the early twentieth century made way for a new subject/object configuration.[17] The experimenter soon became known as the "experimenter" or "E," and the previously understood scientific observer came to be called the "subject" or "S." Indeed, the initial move to standardize the term "subject" rested on the claim that, in

[14] Kaplan, *Conduct of Inquiry* (cit. n. 1), p. 127. For an account of the approach taken by most psychologists see George Devereux, *From Anxiety to Method in the Behavioral Sciences* (The Hague: Mouton, 1967).

[15] Roger Smith, *The Norton History of the Human Sciences* (New York: Norton, 1997), p. 13; and Evelyn Fox Keller, "The Paradox of Scientific Subjectivity," in *Rethinking Objectivity*, ed. Allan Megill (Durham, N.C.: Duke Univ. Press, 1994), pp. 313–331, on p. 314.

[16] Lorraine Daston and Peter Galison, "The Image of Objectivity," *Representations*, 1992, *40*:81–128, on p. 82. For examinations of the relations of subjectivity and objectivity see Richard J. Bernstein, *Beyond Objectivism and Relativism: Science, Hermeneutics, and Praxis* (Philadelphia: Univ. Pennsylvania Press, 1983); Susan Bordo, *The Flight to Objectivity: Essays on Cartesianism and Culture* (Albany: State Univ. New York Press, 1987); and Evelyn Fox Keller, *Reflections on Gender and Science* (New Haven, Conn.: Yale Univ. Press, 1985).

[17] The standardization of "subjects" in introspection experiments is described in Deborah Coon, "Standardizing the Subject: Experimental Psychologists, Introspection, and the Quest for a Technoscientific Ideal," *Technology and Culture*, 1993, *34*:757–783.

many instances, "the so-called 'observer' does no observing."[18] In what retrospectively might look like rhetorical slippage, the experimenter as agent of action makes objective observations and the subject produces subjective responses (which are sometimes taken as objective but typically are not).[19]

These distinctions between the scientific object, the subject, and the objective experimenter evade the reflexivity-laden paradox of the human sciences and foreclose on the subject's agency. Conventional histories of experimentation mostly neglect the subject who serves as scientific object, aiming instead to chronicle technical advances that foster objectivity.[20] In contrast, Foucauldian-oriented histories highlight the structure of the experiment, finding its particular arrangements productive of "administratively useful knowledge." They find experimental psychology to have borrowed the social arrangements of the classroom, hospital, and workplace wherein "the individuals under investigation became the objects of a certain kind of social power. This was not a personal, let alone, violent, kind of power but the kind of impersonal power that Foucault has characterized as being based on 'discipline.'" The scientific object is understood to be a certain kind of subject, a subject of discipline.[21] These histories overlook the efforts, resources, and even failures to attain or maintain such power relations, and although they contribute to an understanding of human subjects as scientific objects they leave aside much evidence about those objects.[22] They also neglect the dynamics of scientific practice: the back-and-forth relays of information, bodies, techniques, and customs between "science" and "culture"—relays showing the "connectivity of science."[23]

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[18] John F. Dashiell, "Note on the Use of the Term 'Observer,'" *Psychological Review*, 1929, *36*:550–551, on p. 551. On the early twentieth-century conceptual shifts see Andrew Winston, "Controlling the Metalanguage: Authority and Acquiescence in the History of Method," in *Rediscovering the History of Psychology: Essays Inspired by the Work of Kurt Danziger*, ed. Adrian C. Brock and Johann Louw (New York: Kluwer, 2004), pp. 53–74; Michael Sokal, "Scientific Biography, Cognitive Deficits, and Laboratory Practice: James McKeen Cattell and Early American Experimental Psychology, 1880–1904," *Isis*, 2010, *101*:531–554; and Jill Morawski, "Scientific Selves: Discerning the Subject and Experimenter in Experimental Psychology in the U.S., 1900–1935," in *Psychology's Territories: Historical and Contemporary Perspectives from Different Disciplines*, ed. Mitchell Ash and Thomas Sturm (Mahwah, N.J.: Erlbaum, 2006), pp. 129–148.

[19] The more recent replacement of the designation "subject" with "participant" seems not to have affected experimental relations much. In fact, whether the scientific object should be considered a "participant" rather than a "subject" remains an open question. See Roddy (Henry F.) Roediger, "What Should They Be Called?" *APS Observer*, 2004, *17*(4):5, 46–48.

[20] Conventional historical accounts tacitly adopt the assumption of passive subjects that early twentieth-century psychologists borrowed when emulating the model experiment of physiology and biology. For an illuminating study of the complexities that surrounded the desire for passive subjects see Otniel Dror, "Creating the Emotional Body: Confusion, Possibilities, and Knowledge," in *The Emotional History of the United States*, ed. Peter N. Stearns (New York: New York Univ. Press, 1998), pp. 173–194.

[21] Kurt Danziger, *Constructing the Subject: Historical Origins of Psychological Research* (New York: Cambridge Univ. Press, 1990), p. 10. Some of these critical histories view the human sciences as producing changes in the subject. See Nicolas Rose, *Inventing Our Selves: Psychology, Power, and Personhood* (New York: Cambridge Univ. Press, 1996). Such views resemble humanists' claim that the psychological model used (mechanist, reductionist, determinist science) fosters a kind of psychological being. See Sigmund Koch, "Psychological Science versus the Science–Humanism Antinomy: Intimations of a Significant Science of Man," *Amer. Psychol.*, 1961, *16*:629–639.

[22] Emphasis on uninterrogated "impersonal power" tends to sideline the power produced through social hierarchies, notably class, gender, and race. On this tendency in science studies see Donna J. Haraway, *Modest_Witness@Second_Millennium.Femaleman_Meets_Oncomouse: Feminism and Technoscience* (New York: Routledge, 1997); and Betty Bayer, "Critical Contact: Psychology, the Subject, and the Self," *Feminism and Psychology*, 2002, *12*:455–461. On the need for microlevel models to supplement Foucault's see Ian Hacking, "Between Michel Foucault and Erving Goffman: Between Discourse in the Abstract and Face-to-Face Interaction," *Economy and Society*, 2004, *11*:277–302.

[23] Thomas F. Gieryn, *Cultural Boundaries of Science: Credibility on the Line* (Chicago: Univ. Chicago Press, 1999), p. 55; and Karen Knorr-Cetina, "The Couch, the Cathedral, and the Laboratory: On the Relationship of Experiment and Laboratory in Science," in *Science as Practice and Culture*, ed. Andrew Pickering (Chicago: Univ. Chicago Press, 1992), pp. 113–138.

The vibrant connectivity of the human sciences has been described as a "double hermeneutic" by Anthony Giddens, as "dynamic nominalism" by Ian Hacking, and as the "circuitry of the psychological" by Graham Richards. Richards especially notices the multiple reflexive turns, the "circuitry," of the psychological that connects psychologists' personal and professional self-knowledge, the psychological knowledge produced, and the social contexts in which that psychology is grounded and through which the psychological circulates. Laboratory experiments were (and still are) designed to interrupt this so-called folk traffic in values, beliefs, actions, and affect, but in crucial ways experiments depend on these imports and exports.[24] These dynamic models of the human sciences understand experiments as lively, negotiated interactions, despite their standardized administrative arrangements. They indicate how psychology experiments, in staging the "real," depend on fluid exchanges between laboratory experiences and those of everyday life.[25] Understanding experiments as culturally extensive enables seeing the ways in which their participants are agents as well as mechanisms, labelers and calibrators of reality as well as labeled and calibrated reality. Recognizing participants as embodied and lively as well as imagined and standardized reveals the "ontological fecundity of the sciences."[26]

Acknowledging experiments as standardized co-productions and intimate engagements calls for a historiography other than the conventional and Foucauldian ones: it requires a framework for examining what can be called the "experimenter–subject system." In that system, the production of data depends on a relationship of experimenter and subject that is intimate and mutable. Cultural preconditions (e.g., roles, rules of felicity, morals, economic exchanges) are essential to the experiments' execution and success. Cultural know-how grounds experiments from original design to final reports and from recruiting to debriefing subjects. The experimenter–subject system comprises a nexus in the complex circuitry of the psychological, and it affords evidence of how the history of "the human sciences is a history of the social worlds in which the self has existed." Borrowing the historian Robert Kohler's terms, the boundaries of these worlds are better understood as permeable "borders" that are intangible yet real.[27] Whenever the objects are human beings, this permeability is at once a prerequisite of successful experimental relations and an ever-present danger to them. During periods of disquietude, the complexities of laboratory relations often are bared; borders between the laboratory and the rest of culture are breached; and experimenters elucidate new

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[24] Anthony Giddens, *Central Problems in Social Theory: Action, Structure, and Contradiction in Social Analysis* (London: Macmillan, 1979); Ian Hacking, "The Looping Effects of Human Kinds," in *Causal Cognition: A Multi-disciplinary Debate*, ed. Dan Sperber, David Premack, and Ann James Premack (Oxford: Clarendon, 1995), pp. 351–383; and Graham Richards, "The Psychology of Psychology: A Historically Grounded Sketch," *Theory Psychol.*, 2002, *12*:7–36. Richards noted how any account of the circuitry "must itself be a Psychological model": *ibid.*, p. 8.

[25] The epistemic and technological transactions between dramatic experiments and television's *Candid Camera*, self-administered psychological tests offered in magazines or on the Internet, and quiz shows have, in Richards's terms, a "double effect" whereby "people become familiar with, and unselfconscious about, engaging in them but they take it all less seriously than when authority-figure teachers and psychologists were the only people using them": Richards, "Psychology of Psychology," p. 15.

[26] Lorraine Daston, "The Coming into Being of Scientific Objects," in *Biographies of Scientific Objects*, ed. Daston (Chicago: Univ. Chicago Press, 2000), pp. 1–14. For an early and prescient call for biographies of objects see Igor Kopytoff, "The Cultural Biography of Things," in *The Social Life of Things: Commodities in Cultural Perspective*, ed. Arjun Appadurai (Cambridge: Cambridge Univ. Press, 1986), pp. 64–91.

[27] Simon Cohn, "Making Objective Facts from Intimate Relations," *History of the Human Sciences*, 2008, *21*(4):86–103; Smith, *Norton History of the Human Sciences* (cit. n. 15), p. 14 (quotation); and Robert E. Kohler, *Landscapes and Labscapes: Exploring the Lab–Field Border in Biology* (Chicago: Univ. Chicago Press, 2002), pp. 14–17.

or previously tacit beliefs about themselves and their experimental relationships. The circuitry of the psychological is made more visible.

POSTWAR SENSE OF VERTIGO

One period of disquietude came in the two decades following World War II, when North American psychologists of varied specialties (experimentalists, testers, surveyors, and clinicians) reported a complex, at times even frightening, array of problems with their scientific objects. These perturbing queries also raised concerns about the experimenters themselves: if subjects responded to the entire experimental situation and not just the designated stimuli, then the experimenter could be an unnoted "variable" that unintentionally influenced subjects. Concerns infrequently voiced before the war escalated in the 1950s and continued through the 1960s; a relay of doubt traversed nearly every subfield of human psychology (and occasionally nonhuman research).[28]

The questionings do, indeed, seem dizzying, although not everyone reported vertigo. The qualms often were described using technical language ("representativeness," "validity") or philosophical tenets about the nature of reality (asking what is real and what is apparent), human nature (asking what is determined and what is autonomous, what is controlled and what is free), and the unique identity status of the subject and the observer (asking about representations, notably race and gender). Whether discussing abstract quandaries or concrete experimental problems, the reports echo the language and tropes of Cold War culture. They rehearse anxieties about conformity and authenticity, emphasize virtues of democracy in opposition to conditions of authoritarian states, and exercise a psychology of suspiciousness of self as well as others—a psychology of brainwashing, surveillance, and self-defense. The Cold War was a profoundly psychological war: Frederick Dolan described it as "a 'looking glass war' in which the enemy one fought was to an unusual degree an unverifiable creature of one's own imagination." Yet more than a shivering postwar imagination incited vigilance: the human sciences also were inflected by twinned affects of hope and fear. On the one hand, the social sciences were bolstered by renewed faith that scientific knowledge would better the social world. On the other hand, the war left many uncertain (if not wary) about the human condition and about obtaining veridical knowledge of humanity. As one historian described his early career in that period, scholars felt "rudderless" after the war.[29] In this emotional atmosphere of hope and fear, the psychology experiment was submitted to interrogation. The discipline's rapid expansion in size and jurisdiction resonated with optimism, as did its proclaimed correspondence with the physical sciences. Yet faith in the science was not as certain as before the war—nor was belief in the transparency of human nature.[30] The science's purchase on human nature had been unsteadied by a devastating world war; it was influenced as well by psychologists' wartime exposure to clinical perspectives that familiarized them with working

[28] Earlier claims about the reflexive dynamics of psychology experiments are discussed in Jill Morawski, "Reflexivity and the Psychologist," *Hist. Hum. Sci.*, 2005, *18*(4):77–105.

[29] Frederick M. Dolan, *Allegories of America: Narratives, Metaphysics, Politics* (Ithaca, N.Y.: Cornell Univ. Press, 1994), p. 61; and Carl E. Schorske, "A Life of Learning," in *Recasting America: Culture and Politics in the Age of Cold War*, ed. Lary May (Chicago: Univ. Chicago Press, 1989), pp. 93–103, on p. 99. Tangles of suspicions and covert actions orient John Le Carre's Cold War espionage novel *The Looking Glass War* (New York: Putnam, 1965).

[30] The significant effects of the war on the human and social sciences in Europe and North America are reviewed in Eghigian *et al.*, "Introduction: Self as Project" (cit. n. 11). Postwar social scientists' reevaluations of human nature are explored in Mark Solovey and Hamilton Cravens, eds., *Cold War Social Science: Knowledge Production, Liberal Democracy, and Human Nature* (New York: Palgrave Macmillan, 2012).

models of the unconscious, patient–therapist relations, transference, and countertransference, along with clinical skills of empathy, self-presentation, reflection, and communication. Simply put, clinical psychology gave many experimental psychologists models, aesthetics, and vocabulary for assessing laboratory relationships as well as themselves.[31]

The scientific world after 1945 also looked different to ordinary persons. The educated civilians who were to constitute the subject-pool labor forces of psychology experiments gained sobering lessons about science from the atomic bomb, the Nuremberg trials, and sensory deprivation experiments. The techniques of B. F. Skinner, John B. Watson, and Ivan Pavlov often were ascribed sinister intentions, and such ascriptions were woven into popular accounts of experimental cooption, covert force, and subversion. Behavior change models were increasingly seen as hierarchical control technologies that oppressed rather than remediated. Vance Packard's *The Hidden Persuaders* and related popularizations that linked experimentation and mind control ("brainwashing") likewise educated readers about the subterranean psychic dynamics of psychology experiments. Similar accounts appeared in fictional depictions like Burdick's story of a manipulating experimental subject and the learning psychologist James McConnell's tale of a psychologist captured by Martians and subjected to their spacecraft experiments.[32]

Intellectuals' suspicious reflections on the human condition and a popular wariness about the mental sciences bore notable resemblances to the human complications being observed in psychology laboratories.

DISCOVERY OF COMPLEX OBJECTS

By the 1930s, experimental psychology had established relatively standardized laboratory procedures for studying humans. After the demise of introspection, with its exchangeable roles of trained observers, psychologists developed experimental situations that would be peopled by naive subjects who dutifully committed time to science. The adoption of aggregate statistics, random sampling, and experimental replication provided assurance that these subjects represented the larger population to which findings could be generalized. Experimental subjects might be ascribed certain social attributes, such as "Negro schoolchildren" or "college men," yet their status as interchangeable naive objects who generated psychological data was a given. Pronouncing the necessity of operationism in 1935, S. S. Stevens described the science as "*the psychology of the other one*":

> That science relates to verifiable responses obtained from organisms treated as objects of study by capable experimenters who may or may not have served in the role of

[31] An example of these wartime collaborations is OSS Assessment Staff, *Assessment of Men: Selection of Personnel for the Office of Strategic Services* (New York: Rinehart, 1948). Intellectual outcomes of wartime exchanges between research psychologists and psychoanalytic clinicians are traced in Gail Hornstein, "The Return of the Repressed: Psychology's Problematic Relations with Psychoanalysis, 1909–1960," *Amer. Psychol.*, 1992, *47*:254–263. On psychologists' war experiences see James H. Capshew, *Psychologists on the March: Science, Practice, and Professional Identity in America, 1929–1969* (Cambridge: Cambridge Univ. Press, 1999). The aftermath of these shared involvements was not always professionally cooperative, as discussed in Roderick D. Buchanan, "Legislative Warriors: American Psychiatrists, Psychologists, and Competing Claims over Psychotherapy in the 1950s," *J. Hist. Behav. Sci.*, 2003, 39:225–249.

[32] Vance Packard, *The Hidden Persuaders* (New York: McKay, 1957); Edward Hunter, *Brainwashing: From Pavlov to Powers* (New York: Bookmailer, 1956); and James V. McConnell, "Learning Theory," *If*, Dec. 1957, 7:102–113. Alexandra Rutherford has examined public and government fears about behavior modification in the 1970s: Alexandra Rutherford, "The Social Control of Behavior Control: Behavior Modification, Individual Rights, and Research Ethics in America, 1971–1979," *J. Hist. Behav. Sci.*, 2006, *42*:203–220.

> investigated organism. . . . A human being enters the situation as a complex physical system whose characteristics can be investigated by a method essentially the same as the methods used for investigation of all physical systems. The essence of the procedure is the performance of a known operation on the system and the observation of the resulting changes or, in more conventional terms, the application of a stimulus and the observation of a response.

Treating human organisms as objects required, according to the arch-experimentalist Clark Hull, a "prophylaxis against anthropomorphic subjectivism." The use of nonhuman organisms instead of humans provided some protection; but more effective, Hull argued, "is to regard, from time to time, the behaving organism as a completely self-maintaining robot, constructed of materials as unlike ourselves as may be." Steadfast commitment to objectivity cloaked even the paradox of perceiving scientists studying human perception; instead, the scientist's "privileged, even unique position" was taken to be a necessary epistemological "dogma." Subjects were taken to be dutiful performers, needing only clear instruction in conditioning experiments lest they "become self-instructed."[33] Despite psychologists' emulation of the physical sciences, it is possible that the conceit of nonreactive, "naive" subjects was long an open secret, a secret that only occasionally was revealed in commentaries on unruly subjects or the messy psychology of the experimenter–subject system.[34] Open secrets are notable in promulgating public ignorance of something that sits in plain view, and perhaps this helps explain why subjects' feigning or conniving was infrequently discussed before the 1950s.[35]

By the late 1940s, published evidence indicated that all was not well with the standardized scientific object, and such evidence increased dramatically over the next two decades. Empirical studies indicated that the "volunteer" subject might be a special kind of scientific object. If that is the case, then volunteers are not representative of the population to which researchers aimed to generalize their findings. The once casual if cynical observation that subjects were recruited predominantly from colleges and universities grew to become a recurrent, half-joking lament that psychology is a psychology of the college sophomore.[36] The rapid growth of testing and survey work in the 1930s and through the war years heightened the stakes surrounding the volunteer issue. Although largely unencumbered by the problems of using the college sophomore subject, survey researchers confronted problems of sampling and investigator bias. Counterintuitive and socially sensitive findings initially drew attention to the procedures for soliciting respondents. For example,

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[33] S. S. Stevens, "The Operational Basis of Psychology," *Amer. J. Psychol.*, 1935, *47*:323–330, on p. 328; Clark L. Hull, *Principles of Behavior* (New York: Appleton-Century-Crofts, 1943), p. 27; Gustav Bergmann and Kenneth W. Spence, "The Logic of Psychophysical Measurement," *Psychol. Rev.*, 1941, *51*:1–24, on p. 3; and Ernest R. Hilgard, "Methods and Procedures in the Study of Learning," in *Handbook of Experimental Psychology*, ed. Stevens (New York: Wiley, 1951), pp. 517–565, on p. 527.

[34] Saul Rosenzweig's exceptional declaration of the hidden psychology of the psychology experiment went nearly unacknowledged until the 1960s: Saul Rosenzweig, "The Experimental Situation as a Psychological Problem," *Psychol. Rev.*, 1933, *40*:337–354. On prewar reports of subjects' attunements, especially personality researchers' notice of subjects' manipulation of personality measures, see Michael Pettit, *The Science of Deception: Psychology and Commerce in America* (Chicago: Univ. Chicago Press, 2013).

[35] The infrequency of deception techniques prior to 1948 and the rapid increase in their use over the subsequent three decades are thoroughly documented in James Korn, *Illusions of Reality: A History of Deception in Social Psychology* (Albany: State Univ. New York Press, 1997).

[36] Quinn McNemar is credited with noting the "college sophomore" psychology. In 1946 he wrote, "The existing science of human behavior is largely the science of the behavior of sophomores." Quinn McNemar, "Opinion-Attitude Methodology," *Psychological Bulletin*, 1946, *43*:289–374, on p. 333.

Alfred Kinsey noted a volunteer bias in his studies, and although he attended to these problems concern with the Kinsey volunteers persisted. If his was a representative sample, some reasoned, then the finding of homosexual practices among seemingly heterosexual American males indicated that persons are not as they appear and that presumably core psychological attributes are not readily detectable.[37]

Growing increasingly wary as to what they did *not* know about the volunteers who populated most experiments, researchers began investigating the question implicit in the title of Ivan Scheier's experimental report, "To Be or Not to Be a Guinea Pig." Scheier's volunteers, unlike guinea pigs, were recruited and compensated for their participation. The incentives were multiple: "service to one's country and science, $10.00, interpretation of individual scores after the experiment, and excuse from a physical education course examination" (even with those incentives, only 60 percent of the students approached volunteered). The "psychology-of-the-volunteer," Scheier found, was less anxious than the "psychology-of-the-population." Additionally, differences between these psychologies depended on the "perceived severity of experimental situation." Both conclusions—the special nature of volunteers and the influence of the particular experimental conditions—suggested the need for future work on the "immensely significant personal and social ramifications" of volunteering.[38] While some investigators expressed confidence that empirical studies would detect volunteers' "gross" and subtle variations from the population, others were less assured. Howard Axelrod and his colleagues questioned whether their replication of a stylus maze learning experiment obtained results differing from those of the original study because their volunteers were recruited from a freshman English class (not from psychology classes); they wondered whether subjects who were students of English might have been "more threatened by the experimental situation than were the less naïve Ss utilized" in the original experiment. Noting that Kinsey's volunteers were "less inhibited sexually, and possibly less likely to have had socially taboo items, such as premarital intercourse and homosexual contacts, in their past histories," Ephraim Rosen hypothesized that the psychology of volunteers is not simply a matter of group character but also depends on the experimental situation. The college "freshman" volunteers in Rosen's study admitted to more anxiety, discouragement, and feelings of inadequacy when compared with a group of nonvolunteers (freshmen who were required to take a battery of psychological tests). These volunteers also were more "intraceptive and psychological minded" and less likely to attend church. Further, male volunteers produced higher femininity scores than nonvolunteers, while female volunteers generated higher scores on dominance and aggression. The volunteer seemed to look less and less like a representative n of the population N. Convinced that using volunteer subjects can affect experimental outcomes, Rosen nevertheless acquiesced, stating that "dependence on highly cooperative volunteers

[37] Abraham Maslow reported volunteer bias in 1942, finding that the female college students who were willing to participate in a study of sexual practices were more extroverted. Responding to concerns about the representativeness of Kinsey's sample, Maslow and a colleague collected data on some of Kinsey's volunteer subjects. See Abraham H. Maslow, "Self-Esteem (Dominance Feeling) and Sexuality in Women," *Journal of Social Psychology*, 1942, *16*:259–294; and James Sakoda and Maslow, "Volunteer Error in the Kinsey Study," *Journal of Abnormal and Social Psychology*, 1952, *47*:259–262. See also John D'Emilio, *Sexual Politics, Sexual Communities: The Making of a Homosexual Minority in the United States, 1940–1970* (Chicago: Univ. Chicago Press, 1983).

[38] Ivan H. Scheier, "To Be or Not to Be a Guinea Pig: Preliminary Data on Anxiety and the Volunteer for Experiment," *Psychological Reports*, 1959, *5*:239–240, on p. 239. Despite such edicts, researchers like Scheier either missed or skirted one significant ramification—namely, the potentially mind-boggling task of adequately assessing the myriad features of the experimental situation. Scheier likewise overlooked the confounding of volunteering and compensation for participating, despite his meticulous inventorying of compensation.

seems inevitable." Others expressed similar regrets, almost resignation, over the seemingly unavoidable conformism or "docile role" played by "faithful" subjects, worrying too that conformity to the experimental situation jeopardized the generalizability of findings to situations outside the laboratory.[39]

Alongside the empirical troubles posed by volunteers arose a conceptual one. Volunteering was typically assumed to be something of a patriotic act: subjects wanted to contribute—to "volunteer" for science and, accordingly, for their country. Some investigators discerned an inconsistency between seeing volunteering as a dignified agentic act and the denial of agency assumed in determinist models. A study comparing rates of volunteering when subjects were offered attractive consequences (being excused from class) or unattractive ones (being made to stay in class and take a "pop quiz") found that volunteering can be "an avoidance act." The expected outcome (volunteers would avoid an unattractive alternative) occurred, yet it led to an apparently counterintuitive conclusion. The authors asserted that subjects' very belief that they are making a choice to volunteer is mistaken: in actuality, subjects' choices are determined by situational forces, and, therefore, the term "volunteering" should be assigned a new meaning. As the researchers reasoned, "decisions that in personal experience seem to be free, independent, and personal are, from another standpoint, determined by the psychological properties of the force field within which the action occurs."[40] Discipline and pop quizzes are determining forces in this field and, therefore, choice is illusion; volunteering is an illusion. Unnoted by these researchers, but not by others, was the possibility that the experimenter might participate in these illusions inasmuch as he or she, too, inhabits the "force field."

Even less comforting were reports that volunteers in medical studies displayed abnormal characteristics. What was to become an often-cited 1954 study published in *Science* describes non-normal characteristics of volunteer subjects and cautions that "generalizations and predictions deserve to be exceptionally reserved when *volunteers* are the sole source of data." The authors noted that Aldous Huxley's recently published *Doors of Perception* should remind readers that much of the literature on drug research is "derived from the experiences of 'volunteers' with unusual psychological orientation and imagination, including romantic proclivities. Without denying the 'reality' of responses in such people, it has proved scientifically unwise to assume that such responses are typical of those experiences by all individuals under all circumstances." These Harvard medical school researchers discovered that twenty-seven of their fifty-six volunteers "would qualify as deviant"; six of these "maladjusted" subjects were designated "overt homosexuals." Another medical research team found that their National Institute of Mental Health volunteers, a group that included conscientious objectors and members of "peace religious denominations," exhibited high rates of "psychopathology." In addition to the volunteers' stated motivations, these researchers detected "unconscious and preconscious ones."[41]

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[39] Howard S. Axelrod, Emory L. Cowen, and Fred Heilizer, "The Correlates of Manifest Anxiety in Stylus Maze Learning," *Journal of Experimental Psychology*, 1956, *51*:131–138, on p. 136; Ephraim Rosen, "Differences between Volunteers and Non-Volunteers for Psychological Studies," *Journal of Applied Psychology*, 1951, 35:185–193, on p. 192; and Samuel Fillenbaum, "Prior Deception and Subsequent Experimental Performance: The 'Faithful' Subject," *J. Personality Soc. Psychol.*, 1966, *4*:532–537.

[40] Robert R. Blake, Howard Berkowitz, Roy Q. Bellamy, and Jane Srygley Mouton, "Volunteering as an Avoidance Act," *J. Abnormal Soc. Psychol.*, 1956, 53:154–156, on p. 155; and Milton Rosenbaum and Blake, "Volunteering as a Function of Field Structure," *ibid.*, 1955, *50*:193–196.

[41] Louis Lasagna and John M. von Felsinger, "The Volunteer Subject in Research," *Science*, 1954, *120*(3114):359–361, on pp. 360–361, 359; and William Polin and Seymour Perlin, "Psychiatric Evaluation of 'Normal Control' Volunteers," *American*

The accumulating reports on volunteering disturbed tacit boundaries between reality and appearance, agency and determinism, authenticity and the superficial, gifts and economic exchanges. The evidence complicated the scientific object (the psyche of the other one), which just twenty years earlier was assumed to be lawful and observable. The scientific art of distinguishing between "naive" and "sophisticated" subjects no longer could be based simply on whether a subject had prior experience in psychological experimentation.[42] Even more serious complications attended experimenters' heightened doubts about subjects' trustworthiness—their authenticity as well as their honesty. Attuned to subjects' self-awareness during personality testing, researchers devised techniques to detect what was becoming a nearly ubiquitous problem of "faking" ("faking good" and "faking bad"). One investigator introduced what became a popular technique for detecting faking, a scale measuring an individual's tendency to give "socially desirable" responses that are "culturally determined." The flow of cultural know-how between the laboratory and the world seemed extensive, as Normals, "Skid-Row" alcoholics, TB sufferers, and psychiatric patients were found to share tendencies toward socially desirable self-presentations. Such lies and secrets were reasoned to be part of human psychology and may even be secret to the subject himself, noted David Bakan. He proposed a modernized introspection technique that could probe the psychology of secrets. Additionally, he suggested, this secret probing method "would provide us with an insuperable advantage over our enemies and potential enemies."[43]

Whether or not such simulation was reckoned to be detectable, apprehensions about faking escalated investigators' worries and raised the specter of a tangled play of who was faking whom. Many researchers understood faking to be dishonest, socially inappropriate, and a close kin to "malingering." This view of deceit was incorporated in experimental instructions to subjects—for example, asking them intentionally to fake a Rorschach test "as if you wanted to be disqualified from military service for psychiatric reasons."[44] Although such lying was deemed morally unacceptable, experimenters designed studies that instructed subjects to feign or fake in order to ascertain whether researchers could differentiate the feigned from the so-called real. Despite such empirical scrutinizing, the very "as if" quality of faking blurred the lines between authenticity and artifact, hovering as a perpetual threat to so-called experimental realism. Faking, cheating, or otherwise conniving was an "as if," an appearance that could shift even as experimenters engineered measures to guard against it. Worst of all was the idea that such "as if" behavior might actually constitute the reality of the experiment.

One technical solution to such creeping dramatics involved espionage-like tactics of deception. Infrequently used before the war, deceiving subjects about the "real" nature of the experiment or experimental stimuli became increasingly common after 1948. Deceptive measures, however, spawned another problematic, one intimated in Skinner's cartoon rat's keen observation of the experimenter's behaviors (see Figure 1). Who was tricking whom in the experimenter–subject interactions? Upon analyzing 457 experiments employing deception published in 1964, Lawrence Stricker found that these studies rarely attempted to detect

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Journal of Psychiatry, 1958, *115*:129–133, on p. 131. These latter researchers determined that eleven of their twenty-nine subjects suffered from a psychopathology.

[42] The experiential and experimental sophistication of nonhumans is considered in Richard Christie, "Experimental Naiveté and Experiential Naiveté," *Psychol. Bull.*, 1951, *48*:327–339.

[43] Allen L. Edwards, *The Social Desirability Variable in Personality Assessment and Research* (New York: Dryden, 1957), pp. 11–12; and David Bakan, "A Reconsideration of the Problem of Introspection," *Psychol. Bull.*, 1954, *51*:105–118, on p. 114.

[44] Marvin J. Feldman and James Graley, "The Effects of an Experimental Set to Simulate Abnormality on Group Rorschach Performance," *Journal of Projective Techniques*, 1954, *18*:326–334, on p. 327.

subjects' suspicions about the deceptions. The absence of such crucial checks raised the question, Who really is the "true deceiver"?[45] Whether experimenters claimed to monitor subjects' faking or suspected that some faking escapes notice, or whether experimenters believed their deceptions worked or suspected themselves to be deceived, seemed less weighty than the overarching realization that laboratory subjects are not always or not only what they seem to be. Investigators were advised to guard diligently against subjects' urges (conscious as well as nonconscious) to present other than their true selves. In short order, this apprehensive outlook was extended to experimenters themselves, who manifested worries about subjects' reactions to them and even about their own thoughts and actions.

PERCEIVING EXPERIMENTERS

As their scientific objects came to be seen as more alert, complex, and unpredictable, some investigators attended more carefully to their subjects' perceptions, including subjects' perceptions of the investigator. This augmented scientific watchfulness likely was informed by psychologists' work experiences during the war, when many laboratory-trained psychologists gained knowledge of clinical psychology and psychoanalytic ideas through war-related work and many clinically trained psychologists gained greater interest in making their field more scientific. A member of the latter group, Edith Lord, undertook a study of "experimentally induced" variations in Rorschach testing that was inspired by a 1947 Air Force study finding that test examiners influenced subjects' performance. Lord trained three Rorschach examiners to act according to one of three different administrative styles: negative, positive, and neutral. She found that subjects' scores were influenced by the administrative styles enacted by the examiners—but even more by the different examiners themselves. Lord concluded that subjects' performances "may be considered a mirror of the administrator's personality." Although the three examiners "were not different in any grossly apparent way" (all held psychology degrees, all were trained in test administration, etc.), the findings revealed that subjects perceived them differently. The Rorschach scores of subjects who were tested by "Examiner A," for instance, indicated that Examiner A was cold and threatening. To confirm the validity of subjects' perception of the examiner, Lord asked the opinion of "two psychologically sophisticated persons who were acquainted with all three examiners"; these judges corroborated the subjects' view, describing A as "the coldest, most inflexible," one adding that she was "masculine" and a "castrating type of female."[46] In other words, attempts to deceive subjects with enactments of negative, positive, and neutral administrative styles, however assiduously scripted in the experimental design and performed by the three experimenters, failed to conceal the experimenters' subjective selves fully. Perhaps this failure was nonconscious, Lord hypothesized. For their own part, the experimenters seemed not to have full access to their own reality: their limited conscious self-awareness mirrored that of subjects who were sometimes conscious, sometimes not, of their own perceptions and actions. Experimenters' selves appeared to be as complex, engaged, and elusive as those of the subjects. Upon obtaining similar results in an experiment on experimenters, Emmett Baughman pondered whether an ideal objective examiner is possible or even desirable. The experimenter attributes

[45] Lawrence J. Stricker, "The True Deceiver," *Psychol. Bull.*, 1967, *68*:13–20. See also Korn, *Illusions of Reality* (cit. n. 35).
[46] Edith Lord, "Experimentally Induced Variations in Rorschach Performance," *Psychological Monographs*, 1950, *64*(10):1–34, on pp. 27–28. See also Robert G. Gibby, Daniel R. Miller, and Edward L. Walker, "The Examiner's Influence on the Rorschach Protocol," *Journal of Consulting Psychology*, 1953, *17*:425–428.

that warranted empirical scrutiny were many, including physical appearance, personality, encouragement, and even a "slight change in phraseology."[47]

A number of laboratory researchers heeded the warnings of these clinical researchers, along with the suggestions of their phenomenologically informed colleagues who urged them to scrutinize the entire experiential field of the experiment.[48] What was called the "new look" in perception research likewise beckoned researchers to monitor the total dynamics of the experiment and not just the designated stimulus. Whether grounded in theory or empirical evidence, such advisements were weighty with implications. Above all, once experimenters were recognized to be part of the experimental field, then their own psychologies should come under scrutiny, as should their claims to objectivity. When Stevens defined the subject as "the other one" in 1935, he had assured colleagues that operationism would preempt worries about the experimenter: "Science has a lot to say about the subject, but little about the *experimenter*. There is, in fact, little to say, for in all science the experimenter is assumed. He is a being capable of performing the elementary, fundamental operations of observation in the same way that other scientists perform them." Stevens's scientist was assumed to be self-aware in the sense of having distinct and unwavering awareness of his abstract absence. Reality was his: reality was available to him through a rigorously trained scientific gaze. Although Stevens was conversant with psychoanalysis, he represented the ideal scientist as confident not only about the existence of the real but also, and more importantly, about his or her objective access to that reality.[49] By the 1950s this confident observer of the other one's reality seemed less believable. Dramatically different psychological perspectives on the self and the other emerged alongside what has been called the "epistemology of the bunker," as social scientists increasingly entertained a world with real and apparent selves who suspected that all was not what it seemed and maintained vigilance in relations with "the other one." Suspicions that persons might not be who they seemed to be—even to themselves—rendered the other one—and even one's self—cause for apprehension.[50]

An increasing wariness of experimenters is evidenced in a host of projects that ranged from empirical studies of their laboratory comportment to moral evaluations of their scientific attitude. Some researchers urged experimenters to control their laboratory behavior better, down to the minutiae of describing monetary rewards. The matter of experimenters' presence was taken even more literally in experiments that tested (and sometimes caricatured) physical and cultural differences between experimenters. Concern with experimenters' personal char-

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[47] Emmett E. Baughman, "Rorschach Scores as a Function of Examiner Difference," *J. Projective Techniques*, 1951, *15*:243–249, on p. 247. Baughman urged testers' self-evaluation "to determine our own unique deviations, and then determine corrective measures if such appear to be desirable."

[48] Egon Brunswik, *Perception and the Representative Design of Psychological Experiments*, 2nd ed. (Berkeley: Univ. California Press, 1956); Kenneth R. Hammond, "Representative vs. Systematic Design in Clinical Psychology," *Psychol. Bull.*, 1954, *51*:150–159; and Hammond, "Subject and Object Sampling—A Note," *ibid.*, 1948, *45*:530–533.

[49] Stevens, "Operational Basis of Psychology" (cit. n. 33), p. 328. On experimental psychologists' ambivalence toward psychoanalysis see Hornstein, "Return of the Repressed" (cit. n. 31).

[50] Catherine Lutz, "Epistemology of the Bunker: The Brainwashed and Other New Subjects of Permanent War," in *Inventing the Psychological: Toward a Cultural History of Emotional Life in America*, ed. Joel Pfister and Nancy Schnog (New Haven, Conn.: Yale Univ. Press, 1997), pp. 245–267. The anxious suspicions about humans in postwar technology studies are examined in Edward Jones-Imhotep, "Maintaining Humans," in *Cold War Social Science*, ed. Solovey and Cravens (cit. n. 30), pp. 175–195. See also David D. Noble, *The Classroom Arsenal: Military Research, Information Technology, and Public Education* (London: Falmer, 1991); Emily Martin, *Flexible Bodies: The Role of Immunity in American Culture from the Days of Polio to the Age of AIDS* (Boston: Beacon, 1994); Ron Robin, *The Making of the Cold War Enemy* (Princeton, N.J.: Princeton Univ. Press, 2003); and Dolan, *Allegories of America* (cit. n. 29).

acteristics motivated a verbal conditioning study comparing the effects different experimenters had on subjects' rates of learning. One experimenter was "an attractive, soft-spoken, reserved, young lady who was 5'1/2" in height and 90 pounds in weight," the other was a male who "was very masculine, 6'5" tall, 220 pounds in weight, and had many of the unrestrained personality characteristics which might be expected of a former marine captain." The results indicated that the diminutive female, initially hypothesized to be less successful at conditioning subjects to use hostile words than the "big, prestigeful male experimenter," actually proved to be the better conditioner. The *post hoc* explanation of this unexpected outcome was that the feminine experimenter "provided a less threatening environment, and the *S*s consequently were less inhibited in the tendency to increase their frequency of usage of hostile words." An experiment that assessed the effect of experimenters' sex on the performance of child subjects found that female experimenters had a greater effect on young male subjects. The experimenters explained the outcome using psychoanalytic theory, asserting that "the Oedipal relationship is more intense with boys than with girls." Another experiment, a basic verbal learning study, compared the effects of similar experimenters and found "far-reaching consequences of these seemingly minimal differences between *E*s."[51] Whether examining subtle or culturally obvious experimenter attributes, these empirical findings indicated that laboratory dynamics were far from uncomplicated.

Experimenters' self-awareness extended beyond matters of psychological influence or material, bodily presence to include political and moral ones. The Nuremberg trials heightened psychologists' attention to the ethics of research with human subjects, although they produced no consensual lesson. Some writers believed that an ethics code ensured experimenters' proper conduct, whereas others warned that codes would contaminate experiments (for instance, that giving subjects advanced knowledge about the experiment might undermine experimental validity). Still others detected a political economy of experiments that dangerously resembled totalitarianism.[52] Further, the moral conduct of experimenters was weighed not only in macropolitical terms but also in more domestic ones. Pointing to cultural power relations, Catherine Landreth challenged recommendations that experimenters gain the cooperation of children by inviting those subjects to "play some games." She identified the gender politics of asking a four-year-old girl to play games: "Not all girls say no, despite their mother's advice so it is possible that the experimenter will eventually get a little girl's hand in his— but not for long."[53]

Questions about the experimenter began with probing the basic assumption that "objectivity implies that one examiner can be considered as interchangeable with another" but extended to something of an inquisition into the experimenter as well as the subject and, eventually, into the intimacies of their relationship. The experimenter was found to be neither a simple "stimulus object" nor a "passive recorder"—a discovery that mirrored the growing antibehaviorist claim that the subject was neither an object nor a passive responder. Even

[51] Ward Edwards, "Costs and Payoffs Are Instructions," *Psychol. Rev.*, 1961, *68*:275–284; Arnold Binder, David McConnell, and Nancy A. Sjoholm, "Verbal Conditioning as a Function of Experimenter Characteristics," *Journal of Abnormal Psychology*, 1957, 55:309–314, on pp. 309, 313; Harold W. Stevenson, "Social Reinforcement with Children as a Function of CA, Sex of *E*, and Sex of *S*," *J. Abnormal Soc. Psychol.*, 1961, *63*:147–154, on p. 152; and Frederick H. Kanfer, "Verbal Conditioning: Reinforcement Schedules and Experimental Influence," *Psychol. Rep.*, 1958, *4*:443–452, on p. 451.

[52] The adequacy of ethics codes is discussed in Irwin A. Berg, "The Use of Human Subjects in Psychological Research," *Amer. Psychol.*, 1954, 9:108–111. On the risks to validity created by fully implementing the Nuremberg recommendations into an ethics code see Maurice H. Krout, "Comment on the 'Use of Human Subjects in Psychological Research,'" *ibid.*, 1954, 9:589.

[53] Catherine Landreth, "Playing Games," *Amer. Psychol.*, 1961, *16*:604–607, on p. 605.

when scientific observers were considered to be "part of the measuring apparatus," they could undergo "instrument decay" through fatigue, experience, or adaptation. Clinical and experimental perspectives were amalgamated in order to scrutinize "the total stimulus value of the examiner to the patient in the production of the inquiry."[54] Just as the independent variable was designed to prompt the subjects, so the experimenter had stimulus value—although that value might be hidden because experimenters, albeit inadvertently, motivate subjects. According to Edith Henry and Julian Rotter, experimenters can trigger subjects' inhibitions, suspiciousness, caution, defensiveness, and hostility (both overt and latent). Moreover, they activate subjects' "subjective hypotheses" and set off a chain of inferential thinking. Experimenters use so-called "common-sense reasoning," not logic, to apprehend subjects' tendencies toward "caution and inhibition," which, in turn, leads these subjects to respond defensively in ways that merely look like they are "seeing things as most other people are likely to see them." Anxiously, although not always openly, subjects respond to experimenters' anxieties and hostilities; subjects' emotions thus are connected—so the logic goes—to those of experimenters. To complicate matters further, subjects apparently strive to conceal their strategies and anxieties, resisting even persistent postexperimental interrogations aimed at uncovering their true thoughts. Secrecy, double-talk, and hiding run through investigations, and it was suspected that subversion and duplicity are extremely difficult to detect—if indeed detectable at all. Empirical studies and commentaries alike directly implicated experimenters in what resembles episodes of spy against spy or sounded a siren call of epistemological crisis.[55]

Experimenters' perception of subjects' perception of the experimenter frequently relied on clinical concepts of defense mechanisms and anxieties, and these concepts mingled with new constructs of permanent war being waged not only internationally but also interpersonally (and intrapersonally). Interpretations of subjects' lively engagements with experimenters typically relied on dominant social representations, notably those of race, gender, class, and age. For example, subjects' behavior was attributed to their dual identities as scientific subjects and college students, claims that often rehearsed stereotypes about college-aged adolescents. Perhaps the most revealing social representation of subject–experimenter relations was race. Studies of interviewing, projective and intelligence tests, and experimental situations indicated that Negro subjects responded differently to white experimenters than did white subjects. Numerous studies conducted between 1945 and the late 1960s found a "race of experimenter effect," and most investigators explained that effect not in terms of the experimenter (his race or comportment) but, rather, in terms of the subject. That is, Negro subjects were believed to establish less rapport with white investigators than did white subjects: they reportedly were more fearful, withholding, defensive, and reluctant to "express their opinions freely to whites." Negro subjects also behaved in distinctive ways with Negro experimenters. Some researchers concluded that such studies confirm the salience of the "social reality" of the laboratory—ironically, by reasoning that the laboratory reality is so robust that racial dynamics could offset the experimental manipulations. One research team used this logic in reasoning that what were previously understood as Negro subjects' "perceptual defenses"

[54] Baughman, "Rorschach Scores as a Function of Examiner Difference" (cit. n. 47), p. 246 (inquisition); Binder *et al.*, "Verbal Conditioning as a Function of Experimenter Characteristics" (cit. n. 51), p. 243 (neither an object nor a passive responder); Donald T. Campbell, "Factors Relevant to the Validity of Experiments in Social Settings," *Psychol. Bull.*, 1957, 54:297–312, on p. 299 ("instrument decay"); and Gibby *et al.*, "Examiner's Influence on the Rorschach Protocol" (cit. n. 46), p. 252 ("total stimulus value").

[55] Edith M. Henry and Julian B. Rotter, "Situational Influences on Rorschach Responses," *J. Consulting Psychol.*, 1956, 20:457–462, on pp. 458, 461.

against recognizing derogatory race words might actually be "suppression of response," especially in the presence of Negro experimenters, when Negro subjects hesitated to utter racially derogatory terms such as "nigger," "darky," and "coon."[56] The intersubjective dynamics of this experiment were deciphered according to cultural precepts of the Negro psyche, whereas experimenters' (similarly culture-based) racial awareness and even their own race were neglected. In the experimenter–subject system, the hierarchy of experimental actors mapped smoothly onto the hierarchy of race.[57] Thus even in examinations of the effects of experimenters' race, investigators' reasoning often relied on culturally salient interpretations. Further, the culture-dependent interpretations could become iterative: one experimenter might question another experimenter's perception of the subject's perception of the experimenter, adding another layer to the interpretation. Commenting on a study comparing the effects of "Jewish-appearing" and non-"Jewish-appearing" interviewers on subjects' endorsement of anti-Semitic statements, Kenneth Hammond questioned the experimenters' determination of how a Jewish-appearing researcher should appear. Could it be the case, he inquired, that what appeared Jewish to subjects in New York City might not appear Jewish to subjects residing elsewhere?[58]

Questions about the experimenter as stimulus object most often were translated as methodological, not epistemological or psychological. Indeed, some investigators earnestly doubted the possibility or even the desirability of an "objective experimenter," yet many others took evidence of experimenters' effects as a methodological warning that "we must, of course, always strive for as complete uniformity in experimental procedure as possible," even if complete standardization of experimenters' behavior is impossible. So concluded Leo Postman and Rheem Jarrett, whose "Thorndikian" learning experiment revealed that different experimenters produce different learning patterns in subjects. They asserted that making the experimenter another testable variable constituted a methodological advance; further, it would eliminate experimental psychologists' current "double standard" whereby "they scrutinize carefully populational generality yet continue experiments following the classical design of *one* stimulus variable." Enthusiastic commentators believed that such methodological correctives of the experimenter's position would usher psychology from its refuge in eighteenth-century Newtonian physics into the company of twentieth-century physics, notably the theory of general relativity. Quoting Einstein's claim that all observations occur within a certain coordinate system, Hammond argued that experimental psychologists should examine their own coordinate system by a representative sampling of all experimental conditions, including those of the experimenter. The conventional technique of "systematic sampling" (for instance, sampling of male and female experimenters) not only involves arbitrary choices that yield "glaring overgeneralizations" but also violates assumptions of the statistics being used. Genuinely "representative" design accords with general relativity theory, yet it requires more scientific labor than, say, simply comparing the effects of a male and a female experimenter: it demands sampling of any number of experimenter characteristics. Egon Brunswik, devoted methodologist and promoter of representative design, admitted that it

[56] Herbert H. Hyman, *Interviewing in Social Research* (Chicago: Univ. Chicago Press, 1954), p. 159; and Edna M. Whittaker, J. C. Gilchrist, and Jean W. Fischer, "Perceptual Defense or Response Suppression?" *J. Abnormal Soc. Psychol.*, 1952, 47:732–733.

[57] For a history of studies of the effects of the race of the experimenter see Jill G. Morawski, "White Experimenters, White Blood, and Other White Conditions: Locating the Psychologist's Race," in *Off White: Readings on Race, Power, and Society*, ed. Michelle Fine, Lois Weis, Linda Powell, and L. Mun Wong (New York: Routledge, 1997), pp. 13–28.

[58] Hammond, "Subject and Object Sampling" (cit. n. 48).

would constitute "a formidable task in practice," adding, "ideally, it would take concerted research projects of a magnitude hitherto unheard of in experimental psychology."[59]

The task of "attacking" the experimenter problems seemed formidable in ways going beyond the magnitude of the requisite labor, for it raised a number of questions about methods. How many experimenters must be run in an experiment? Which of the myriad characteristics of the experimenter, both gross and subtle, need to be manipulated in experiments? Statistical correction of so-called experimenter effects might not be sufficient, given that not all biases are equal in either quality or magnitude. Even if researchers could determine and control all the experimental "variance" between experimenters, what about the variations in pre-experimental performances of experimenters (their experiences and actions before the experiment begins)? The potential for continual methodological slippage, for discovering yet additional variables to calibrate and monitor, prompted haunting contemplations. What about, Postman and Jarrett wondered, "the possibility that the variation among the results of different Es was due, at least in part, to the differential selection of Ss," which, in turn, was the result of some yet unknown differences between the Es? Such a possibility demanded monitoring yet another formidably complex "source of systematic variance": the world outside the laboratory. Such worrisome commentaries on the experimenters themselves were neither overlooked nor deemed beyond study, and researchers designed studies to appraise fellow experimenters' anxieties and their effects on subjects. A review of empirical studies on experimenter anxiety found its effects to be unpredictable, adding, "To make a notable understatement: more research is needed—much more."[60] Surveillance of experimenters' experimental engagements required constant vigilance over subject selection, experimenter characteristics, dynamic interactions between experimenters and subjects, and the distressing possibility that what experimenter and subject said to and thought about each other might not be the real thing. Given the plethora of possible inner and outer psychological factors and end states, researchers' mandates on the subject of methodology sometimes read like symbolic defense statements made against ever dangerous and escalating intersubjective activities. While some researchers feared that technical correctives would require a massive increase in scientific labor yet offer no guarantee of experimental validity, others submitted the seemingly disorderly experimenter–subject system to deeper levels of empirical study.

PSYCHOLOGY OF THE PSYCHOLOGY OF THE PSYCHOLOGY EXPERIMENT

Two experimenters, now renowned for their acumen, systematically examined the psychology of the experimental system, and their work illustrates the efforts to remedy laboratory problems. Eventually adopting different psychologies of experimenters and subjects, Martin Orne and Robert Rosenthal independently began by observing subjects' reactions to experimenters' psychological operations, overt and covert, conscious and

[59] Leo Postman and Rheem F. Jarrett, "An Experimental Analysis of 'Learning without Awareness,'" *Amer. J. Psychol.*, 1952, *65*:244–255, on p. 254; K. R. Hammond, "Relativity and Representativeness," *Philosophy of Science*, 1951, *18*:208–211, on p. 208; and Brunswik, *Perception and the Representative Design of Psychological Experiments* (cit. n. 48), pp. 131, 43, viii. See also Hammond, "Relativity and Representativeness"; Hammond, "Subject and Object Sampling"; and Hammond, "Representative vs. Systematic Design in Clinical Psychology" (cit. n. 48).

[60] Postman and Jarrett, "Experimental Analysis of 'Learning without Awareness,'" p. 253; and Robert Rosenthal, *Experimental Effects in Behavioral Research* (New York: Appleton-Century-Crofts, 1966), p. 65.

unconscious. Both accomplished their work from the experimenters' vantage point(s) by assuming the position of a disinterested, distanced experimental observer, and both seemed unconcerned about adopting the stance of an objective observer. In fact, the potential paradox of using experiments to investigate the experimenters' actions might have gone unnoticed by them because neither initially made the experimenter a focus of his inquiries: each had set out on a different scientific mission, but suspicions about the experimenter emerged along the way.

Orne was studying hypnotism, a technique with a long history as a means of detecting truths and untruths. His theorizing departed from the orthodox definition of hypnosis as a special or abnormal state of consciousness, and he rejected the notion of "a passive S in a sleeplike state who has amnesia for the events occurring in hypnosis, and responds only to the hypnotist's suggestions." Instead, he proposed, "hypnotic behavior results from the subject's conception of the role of the hypnotic subject as determined by past experience and learning, and by explicit and implicit cues provided by the hypnotist and the situation." His alternative conceptualization understands subjects to be perceptive, responsive, and motivated. Subjects are eager to comply with the experimenter's "wishes" while at the same time "reticent" about revealing their own thoughts on the situation. As reported in a 1959 article and developed extensively over the next five years, Orne considered hypnosis to be analogous to experimentation: both hypnotized subjects and general experimental subjects act "as if" they are hypnotized or so affected by the explicit experimental expectations. Just as subjects to be hypnotized enter the scene with considerable cultural knowledge about hypnosis, so experimental subjects enter the experimental psychology setting with "some sophistication in regard to the philosophy of experimentation." Subjects confront a difficult situation: they are to act naive but they know, too, the necessity of being "honest." Alert and engaged, "Ss are motivated to avoid recognizing explicitly the purpose of the experiment even though it may be communicated by its design. Thus, the response to the direct question, 'What do you think this is about?' tends to be 'I don't know.'"[61] Only with careful (clinical) interrogation not unlike the interrogation of prisoners—only when the "S is pressed"—will the truth of the subject's awareness of the experimenter's objective be revealed.

Orne's investigations revealed previously unseen "demand characteristics" that function to make both hypnosis and experiments work. A series of complex studies situated "real" and "fake" trance subjects in elaborate experimental settings with "naive" or "blind" subjects and informed experimenters. To emphasize the universality of subjects' active role-taking, Orne provided parenthetical anecdotes about other experiments in which subjects falsely denied having foreknowledge; his article thus is replete with deceptions and double deceptions. What is real becomes entangled with what is simulated, and what is objective blurs together with what is subjective. In the end, subjects' "denial" of foreknowledge of the experiment does not necessarily mean what one might suppose, and, therefore, the experimenter's scientific findings do not necessarily mean what he or she thinks they do. Orne found subjects to be so protective of their feigned ignorance that accurate discrimination of their faking required "a clinical diagnosis with an inordinate amount of subjective uncertainty and about 20% error."[62] In other words, faking subjects have a psychology, one that it requires special psychological skills to comprehend. Interwoven

[61] Martin T. Orne, "The Nature of Hypnosis: Artifact and Essence," *J. Abnormal Soc. Psychol.*, 1959, 58:277–299, on pp. 280–281, 277, 281.
[62] *Ibid.*, p. 294.

through this account of subjects' psychology is acknowledgment of the experimenters' psychology and its dynamic influence on the subjects' psychology. Experimenters, too, warrant appraisal. Aiming to assess a purported state of consciousness more accurately—to separate the "valid" aspects of hypnosis from "artifact"—Orne described a theater of the superficial, exposing an "as if" world of duplicitous yet interdependent experimenters and subjects. His understanding of the "demands" of the experimental situation is an understanding of power relations in the laboratory, although he used terms of power and authority only rarely: Orne's was a theater of tacit power. Moreover, his observation that knowledge about experiments is widespread indicates how the circulation of purportedly but not actually confidential information educates the "sophisticated" subject.

As Orne was conducting experiments on hypnosis at Harvard, Rosenthal was completing a dissertation on projection at UCLA. His thesis, "An Attempt at the Experimental Induction of the Defense Mechanism of Projection," hypothesized that projection has "surplus meaning" in the sense that it appears so elusive that a clinician who interprets a patient's fantasies as projection could himself be described as projecting. His objective was to avoid "knowledge by revelation, by edict or by authority" and "epistemological meanderings" by conducting an experiment on the "process of projection by inducing it." Rosenthal disguised the experiment's true purpose and then again deceived the subjects into thinking they had performed either successfully or unsuccessfully on an intelligence test. Before and after the test both groups of subjects were instructed to rate portraits in terms of the pictured person's success or failure. Rosenthal hypothesized that the subjects who believe themselves to be failures (as a result of receiving low scores on the intelligence scale) would project their negative self-feeling onto the individuals represented in the portraits. All the measures of group differences failed to reach statistical significance, except one: the pretest scores of the subjects assigned to the success test group differed significantly from those of the subjects assigned to the unsuccessful group. The data indicated that an effect had occurred *before* the experimental intervention took place—and despite the fact that both groups had received identical pretest instructions. The only logical explanation for this pretest effect is unconscious processes, which are not only in the subjects but also, and more importantly, in the experimenter. The pretest difference between groups was just what the experimenter would have desired in order to confirm his hypothesis. Rosenthal deduced that the findings resulted from "unconscious experimenter bias" that had influenced subjects' thought processes. "There are subtle, important processes occurring within the experimenter which bias the outcome of his research," he noted, adding, "Thus it is felt in this research that the Es hopes as to the outcome led to his treating the experimenter groups differently in subtle ways even while reading them identical instructions."[63]

Rosenthal asserted that experimenter biases could occur at any point in the scientific process—from the choice of theory and experimental design all the way to the interpretation of results. However, the bias occurring "during the performance of the experiment itself" was different from other occurrences in that it is not public (it is not accessible to public scrutiny) and might even be inaccessible to experimenters themselves. The bias is serious and called for necessary correctives, including empirical studies. Experimenters should be submitted to

[63] Robert Rosenthal, "An Attempt at the Experimental Induction of the Defense Mechanism of Projection" (Ph.D. diss., UCLA, 1956), pp. 10, 64. Rosenthal aimed to study projection by simulating it in the laboratory. On simulation of the world in physics experiments see Peter Galison and Alexi Assmus, "Artificial Clouds, Real Particles," in *The Uses of Experiment*, ed. David Gooding, Trevor Pinch, and Simon Schaffer (Cambridge: Cambridge Univ. Press, 1989), pp. 225–274.

experimentation, even if that meant exposing them to the disguises, duplicity, and surveillance in experimental "as if" worlds. This experimental project, Rosenthal admitted, dramatically altered the experimenter–subject system because "the Es used would really in a way be Ss and it would be necessary for them to remain in ignorance of the real nature of the research."[64]

The belief that all subjects are sophisticated, although experimenters often fail to acknowledge this, along with recognition that the experimenter's unconscious messages might serve as either stimulant or "antidote" to subjects, contributed to the growing uncertainty about psychology studies. Some researchers perceived that experiments were reproducing even as they sought to remedy a tangle of the real and the apparent, the authentic and the artificial; even Orne's experiments were criticized for being artifactual.[65] Such tangles were becoming well known in laboratories and beyond, and Orne argued that by the 1950s subjects' understandings of psychology experiments had reached "philosophical sophistication." For one thing, experimenters had gained public renown for compelling subjects to act other than how they would choose, as reports of conditioning experiments, brainwashing, and experimentally induced humiliations entered the cultural lore. Even when engineered without deception or duplicity, psychology experiments gained a reputation for being game-like, further priming subjects' vigilance if not their skepticism and disbelief. Knowledge about experiments was also believed to come directly from college students who had taken a psychology course or participated in experiments.

Recognizing the dynamic and recursive layers of the experimenter–subject system, some psychologists called for work on reflexivity, imploring colleagues to take seriously the fact that, unlike in other sciences, psychology's observer and object are of the same kind—human. W. Donald Oliver and Alvin Langfield urged serious attention to this "unfaced issue of psychology," insisting that failure to acknowledge reflexivity undermined the very project of a scientific psychology because "*what is to be accounted for must be accounted for, else the venture is a failure; and any psychological venture is a failure if in its accounting it fails, or refuses, to take into account its own accounting.*" Don Bannister assayed the weighty implications of reflexivity, finding "that any notions that we can scrape together and articulate, our subjects, being men like us, may also scrape together and articulate." Bannister held that subjects formulate theory frameworks that are as extensive as psychologists' theories and, further, are integrally connected to them. One consequence of these dynamic relations is an "infinite regress or, more properly, infinite progress in which we elaborate a theory large enough to subsume them, they being armed only with the earlier more primitive theory, and they in turn rethink and subsume the experimenter and so goes the race."[66] Calls for reexamining psychology's epistemological and ontological foundations approximated the "epistemopathic surgery" advocated by Sigmund Koch, who was, at the time, arguably

[64] Rosenthal, "Attempt at the Experimental Induction of the Defense Mechanism of Projection," pp. 67, 71 (quotation).

[65] Orne's demonstration of artifacts received criticism for itself being an artifact of experimentation. See Theodore X. Barber and Maurice J. Silver, "Fact, Fiction, and the Experimenter Bias Effect," *Psychological Bulletin Monograph Supplement*, 1968, 70(6):1–29.

[66] W. Donald Oliver and Alvin W. Langfield, "Reflexivity: An Unfaced Issue of Psychology," *Journal of Individual Psychology*, 1962, *18*:114–124, on pp. 117, 123; and Don Bannister, "Psychology as an Exercise in Paradox," *Bulletin of the British Psychological Society*, 1966, *19*:21–26, on p. 25. Bannister's article was reprinted in a collection of critical, philosophically oriented essays on psychology: Duane P. Schultz, ed., *The Science of Psychology: Critical Reflections* (New York: Appleton-Century-Crofts, 1970).

psychology's most ardent yet revered critic.[67] But not all who were concerned about the psychology (or the epistemology) of psychology experiments turned to psychological or epistemological interpretations. Instead, some judged the psychology experiment to be a *political*, not just a *psychological*, situation.

POLITICAL ECONOMY OF THE PSYCHOLOGY EXPERIMENT

Experimenters' conscientious regard of previously neglected psychologies of psychology experiments developed at the same time as psychological models were being generously and painstakingly extended to explain a host of social and political conditions, from international politics to domestic race relations.[68] However, some critics observed that the lines connecting the psychological and the political were not unidirectional: politics could be extended to explain psychology. In drawing an analogy between the animal laboratory and the totalitarian state, Arendt not only intimated a psychology of political states but also exposed the institutional arrangements of power constitutive of experiments. Making a similar connection between politics and psychology, several researchers argued that the intersubjective relations transpiring in experiments are political ones. Exemplary of this work are critical essays by Henry Riecken and Joan Criswell, both accomplished empirical researchers with impressive credentials. Writing in the late 1950s, Riecken and Criswell independently delineated a political economy of the experiment and asserted that a full understanding of experiments depended on analysis of their relations of power, rewards, and institutionalized rationalizations. Put simply, the psychology experiment is a political system complete with economic exchanges and structures of governance (authority), and it must be understood as such.

Riecken first presented his work on the political economy of the psychology experiment at a 1958 Air Force–supported behavioral science conference, just two years after coauthoring a monograph on social psychologists' covert infiltration of an apocalyptic religious sect.[69] He dismissed the idea that the experiment's internal psychology is merely a technical challenge of "unintended variance" or "error variance." What actually transpire in experiments, he countered, are elaborate and often undetected "processes of negotiation." Drawing on Erving Goffman's *Presentation of Self in Everyday Life*, Riecken described how impression management profoundly informs these negotiations. Given that subjects want to "put their best foot forward," he asked, "How does the subject in an experiment decide what virtues are relevant and what faults must be concealed?" The answer required acknowledging subjects' self-defensive double mission (to please and to strategize) as well as the experiment's political structure. The experimental domain is totalitarian, for the experimenter maintains the symbolic authority of professor and psychologist; he is almost an "*in loco parentis* for the subject" and is a "powerful figure" who, among other privileges, has the "power of insight into the subject." While considered fairly trustworthy, the experimenter nevertheless can trick the subject or, worse, he can "expose the subject's pretenses, inflict humiliation and mental pain." Disregarding the subject's past and future, "the experimenter usually wants to use the subject as an instance of behavior, use him just one time, and then forget everything about him except

[67] Koch, "Psychological Science versus the Science–Humanism Antinomy" (cit. n. 21).

[68] Ellen Herman, *The Romance of Psychology: Political Culture in the Age of Experts* (Berkeley: Univ. California Press, 1995); and Capshew, *Psychologists on the March* (cit. n. 31).

[69] Leon Festinger, Henry Riecken, and Stanley Schacter, *When Prophecy Fails: A Social and Psychological Study of a Modern Group That Predicted the Destruction of the World* (Minneapolis: Univ. Minnesota Press, 1956). On Riecken's career in psychology see Clayton P. Alderfer, "Henry W. Riecken: Present at the Beginning (Many Times)—Biography of an Applied Behavioral Scientist," *Journal of Applied Behavioral Science*, 1999, 35:142–144.

the data he has produced." Experiments comprise a "one-sided distribution of information" that enables the experimenter to withhold, conceal, misrepresent, entrap, hint, or otherwise control information. Quite simply, experimenters are unyielding authoritarians who refuse to listen to the subject or grant him or her any "legitimate status." The experiment constitutes a hyper-rationalized world governed by an "inflexible" leader; in fact, the subject's "every move in an experiment has been predetermined and can be said to be 'programmed.'" Insofar as the experimenter believes that the subject believes in the experimental situation, he himself believes this rationalized reality. In these senses, the experimenter stands in contrast to his subjects, who, however subjugated, still enter the experiment with several ambitions and, in a Hegelian master–slave relation, can see more and more accurately. Seeking at once rewards, accurate discernment of the experimenter's ultimate aims, and favorable regard, subjects engage in an "iterative procedure" to negotiate their way through the experiment. The subject's multiple aspirations can camouflage the meaning of his or her actions. The subject, too, can conceal, withhold, lie, and rely on preconceptions in order to maximize "the worth of his behavior."[70] Riecken proposed that psychologists conduct empirical studies of the subject yet continued to have doubts, wondering whether experimenters' "anxiety" and even their confederates' "tensions" and "guilt" could be effectively eliminated. Refined research methods might just shift "the burden of communication" to some other dimension of the experiment and initiate an unending regress of negotiations between experimenter and subject. There appeared to be no straightforward remedy to the undemocratic political economy of the experiment.

Criswell also initially presented her essay at a military-sponsored event, a 1957 Perception Symposium held at Harvard University. She introduced "The Psychologist as Perceiver" with a metaphor of detective work, likening good scientists to detectives who compel their "informants to reveal various significant facts" and do so in ways that reveal nature, not mirror the investigator. Unlike Einstein and other exemplary detective-scientists, she asserted, psychological scientists bungle their detective work because they are disoriented and disabled by their faulty perceptions of "themselves, other scientists, experimental subjects, and the public in general." The "orthodox psychologist" assumes his subjects to be machine-like, devotes himself to prediction and control, and, consequently, adopts an "authoritarian" posture. He manipulates rather than helps people; ascribes creativity only to himself, not to others; and concocts experiments as substitutes for developing meaningful scientific constructs. Experimental research is "facilitated and rationalized by a scientific self-image which establishes as a source of prestige and as an end in itself a demonstration of power over our subjects." Criswell was unrestrained in likening experimentation to authoritarian regimes, describing how its top-down network of control and near absence of cooperation and choice bear stark resemblances to dystopias, Soviet propaganda, and fictional "dictator psychologists" along the lines of the characters described by Skinner and Huxley. This objectionable political economy makes the experimenter anxious in several ways, Criswell observed, because he actually impresses neither the physical scientists he reveres nor the public. Moreover, the experimenter fails to attain "the desired control over his subject": he rationalizes his "neurotic conflicts"; and while his experimental abstractions exude "a strange air of omniscience," subjects readily penetrate that disguise. The psychologist's faulty perceptions cohere with the implicit political economy of the experiment, wherein authoritarian acts yield dependent yet

[70] Henry W. Riecken, "A Program for Research on Experiments in Social Psychology," in *Decisions, Values, and Groups*, ed. Norman F. Washburne (New York: Pergamon, 1962), pp. 25–41, on pp. 27–28, 29, 30–31, 33, 36.

not fully controlled subjects who, in fact, are not entirely slaves because they perceive far more of the experimental situation than experimenters believe they do. Unlike experimenters, who act as though subjects have choices, subjects more accurately comprehend the severe constraints on their actions and proceed accordingly. Forewarned with "some standards regarding the proper behavior of a docile cooperative experimental subject," subjects perform "adequately" and are not motivated to give it their all. Therefore, experimenters should be neither surprised nor horrified when subjects conform to group pressures but, instead, "should take some comfort in the strong possibility that these subjects were simply giving the response most likely to win them a prize in a game."[71]

Criswell dissected the undemocratic government of the laboratory, contrasting it with clinical psychologist–patient relationships, which she saw as cooperative, developing (temporal), and having "provision for the fallibility and correction of the psychologist" (as opposed to the assumed "infallibility of the psychologist"). What is required, she argued, is nothing less than a democratizing of the experimenter: he must give up his status as mythic "scientific father figure" and "heal himself, working his way out of his dilemma through a reorganization of his self-concept."[72]

Riecken and Criswell portrayed psychology experiments as politics operating under the guise of science: although appearing to be science, experiments in reality constitute game-like engagements. Their political-economic critiques gestured toward making the lab more democratic. Criswell demanded a political reformation wherein experimenters relinquish power and subjects acquire greater freedom. Riecken, although also loathing orthodox experimental relations, stood fixed in a liminal space between the real and the apparent yet nevertheless held that subjects are vulnerable, sometimes untrustworthy citizens of a dangerous world where democracy is ever at risk. In a world where distinguishing the apparent from the real posed a constant challenge, something akin to democratic social engineering seemed necessary, and the laboratory was no exception. Other psychologists proffered similar proposals for the democratization of their science. Critical of a scientific method that assumed a distanced, disinterested experimenter, Carl Rogers and David Bakan each elucidated the authoritarian, impersonal nature of the experimenter. They appraised the experimenter's stance of epistemological "loneliness and estrangement," along with his presupposition that the subject is somehow different from himself. To remedy the alienating scientific culture, they urged a democratic politics based on intersubjective regard, one that replaced an epistemology of difference with recognition of similarity. The psychologist Sidney Jourard described a democracy of experimentation in a fictional correspondence between experimenter and subject. In a letter to the "E," Jourard's imaginary college student "S" deplored the undemocratic, authoritarian, scheming, and profiteering atmosphere of experiments. Feeling "pressured, bulldozed, tricked," and "manipulated everywhere I turn," S disclosed that he, too, resorted to tricks and lies, confessing that he fulfilled experimenters' greatest fears about cheating as well as transmitting experimental information to others. S proposed that he and E establish a new experimental relationship, one built on democratic premises that included acknowledgment of their similarities as well as an economy of fair trade: "If you'll trust me, I'll trust you, if you're trustworthy."[73]

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[71] Joan H. Criswell, "The Psychologist as Perceiver," in *Person Perception and Interpersonal Behavior*, ed. Renato Tagiuri and Luigi Petrullo (Stanford, Calif.: Stanford Univ. Press, 1958), pp. 95–109, on pp. 95–96, 98, 104–105.

[72] *Ibid.*, pp. 106, 108.

[73] Bakan, " Reconsideration of the Problem of Introspection" (cit. n. 43); David Bakan, "Clinical Psychology and Logic," *Amer. Psychol.*, 1956, *11*:655–662; Carl R. Rogers, "Persons or Science? A Philosophical Question," *ibid.*, 1955, *10*:267–278; and

NORMALIZING LABORATORY RELATIONS

By the 1970s efforts were under way to quell the civil disorder of the psychology laboratory with methodological remedies and some rhetorical proclamations, and these efforts paralleled attention to civil disorders outside the laboratory. Rosenthal's subsequent research typifies the ways that the problems were addressed. After completing his dissertation, he undertook experiments on what he came to call "experimenter expectancies" or "experimenter bias" but abandoned his original ideas about unconscious processes. The invisible, elusive unconscious forces described in his earlier work were replaced with notions of "the E-S dyad as a signal exchange system" and "impression management." Experimenters needed to learn "how people 'talk' to one another without words and without intent."[74] Just as the experimenter's potentially duplicitous, unmanageable unconscious was being removed from the experiment's intersubjective dynamics, so other disturbances were summarily quieted, transferred, or eliminated through assorted controls—methodological, statistical, ethical, and linguistic. The overriding message was that experimenters needed to manage subjects and their data better. Rosenthal's extensive review of the psychology of "volunteer" subjects exemplifies the technical approach to fixing the subject problems. Taken together, these protocols signaled realization of a self-managed, objective experimenter whose scientific objects appeared ever more cooperative if not resilient.[75]

Soon studies of laboratory relations and strategies for better controlling one or another of their ambiguities were being marshaled as evidence of what came to be described as a "crisis."[76] Although few of the modifications were novel, they nevertheless offered reassurance. More stringent subject-selection protocols safeguarded against enrolling non-normal or non-compliant subjects; deception techniques—sometimes called "technical illusions"—reduced subjects' capacity to con, ingratiate, or otherwise fake their responses; hidden measures detected subjects' socially desirable or reactant responses; using experimenters who were "blind" to the hypothesis reduced if not eliminated experimenter bias; postexperimental interviews checked whether subjects had responded to the experimental stimuli or other laboratory conditions. Along with these correctives came a conversational shift whereby worrisome comments about duplicity gave way to talk about "methodological" means to eliminate "artifacts"; accounts of the experimenter's unconscious gave way to discussions of "bias"; and reports of subjects' sophistication gave way to statements about "expectancies." In due course, the psychology of the psychology experiment no longer seemed to require its own psychological explanation, for all that seemed necessary was heightened attention to "error variance." The myriad laboratory problems once understood as complications of human nature were transvalued as resolvable problems of method (of regulating humans who were

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Sidney M. Jourard, *Disclosing Man to Himself* (Princeton, N.J.: Van Nostrand, 1968), pp. 10, 10–11. It should be noted that the American Psychological Association's code of research ethics, published in 1973, asserts the goals of "openness and honesty" in research.

[74] Robert Rosenthal, "On the Social Psychology of the Psychology Experiment: The Experimenter's Hypothesis as Unintended Determinant of Experimental Results," *American Scientist*, 1963, *51*:268–283; and Rosenthal, N. Friedman, and D. Kurland, "Instruction-Reading Behavior of the Experimenter as an Unintended Determinant of Experimental Results," *Journal of Experimental Research in Personality*, 1966, *1*:221–226, on p. 226.

[75] The American Psychological Association's project to create an ethics code for the conduct of research was undertaken with a sense that subjects were vulnerable but ultimately adopted the standpoint that subjects actually were "resilient" and, therefore, needed a modicum of protective regulation. See Laura Stark, "The Science of Ethics: Deception, the Resilient Self, and the APA Code of Ethics, 1966–1973," *J. Hist. Behav. Sci.*, 2010, *46*:337–370.

[76] Social psychologists produced an extensive literature on the crisis. A summary can be found in Ralph L. Rosnow, *Paradigms in Transition: The Methodology of Social Inquiry* (New York: Oxford Univ. Press, 1981).

undergoing experimental scrutiny). This shift in values and language buttressed the belief that better control of the experimental situation would *de facto* reduce subjects' complexity (at least in the lab) and thereby yield more compliant scientific objects. Even researchers who admitted the experiment's dynamic, even complicated conditions did so with a "curious mixture of acceptance and denial."[77] By the end of the decade, many psychologists had reached what William McGuire calculated to be a third, and final, stage of what he dubbed the "suspiciousness problem" in the experimenter–subject system. According to McGuire, an initial stage of unawareness or even denial of experimental artifacts was followed by a second, "coping," stage, when the researcher recognized the problems that sometimes came to "terrorize and divert him from his main interest." Researchers, he surmised, had finally reached the "exploitation phase," when the suspiciousness problem is transformed into an "interesting independent variable in its own right." Eliminating "our secretiveness compulsion" brought a recognition that subjects, contrary to suspicions that they are suspicious, are really rather apathetic (evidence the disinterested students enrolled in introductory psychology classes). By employing the rhetoric of psychological stage theory, McGuire proposed that the psychological investigations into the previously neglected psychology of the psychology experiment were themselves caused by psychological processes. The 1970s saw a number of monographs on these problems or their remediation, and the 1980s brought histories claiming the experimental malaise to be a "minor perturbation" in psychology's progress or a minor movement spearheaded by a coterie of disaffected researchers.[78]

Transformation of the psychic, epistemic, and politically charged laboratory conditions into measurable variables provided comfort to many, although such transformations continued to rely on psychological explanations and produced few substantive alterations of experimental procedures. Knowledge about the intersubjective melee of the experimental situation did not compel new experimenter–subject relations (as Criswell and Jourard had proposed). Likewise, knowledge about the college sophomore, volunteering, and sophisticated subjects problems, and the ethical caveats about the regress of deception, did not notably influence the use of college sophomore volunteers or deceptive experimental designs.[79] Rather, the technical management of impressions made way for experimental scrutiny of humans' psychic interior. As George Miller stated in his 1969 American Psychological Association presidential address, "giving psychology away" requires no epistemological reconstruction, only a heightened commitment to "pure," tough-minded research.[80]

Epistemic virtues of tough-mindedness and technical refinements did not, however, fully resolve criticisms concerning the morality and politics of laboratory life. They did not address

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[77] Ralph L. Rosnow and Robert Rosenthal, eds., *Artifacts in Behavioral Research* (New York: Academic, 1969), p. viii (quotation); and Jerry M. Suls and Rosnow, "Concerns about Artifacts in Psychological Experiments," in *The Rise of Experimentation in American Psychology*, ed. Jill G. Morawski (New Haven, Conn.: Yale Univ. Press, 1988), pp. 163–187.

[78] William McGuire, "Suspiciousness of Experimenter's Intent," in *Artifacts in Behavioral Research*, ed. Rosenthal and Rosnow, pp. 13–57, on p. 14; and Edward E. Jones, "Major Developments in Social Psychology during the Past Five Decades," in *Handbook of Social Psychology*, 3rd ed., Vol. 1, ed. Gardner Lindsey and Elliot Aronson (New York: Random House, 1985), pp. 47–108, on p. 49.

[79] Efforts to reconceptualize psychology's subject continued, but mostly at the margins of the science, undertaken mainly by researchers allied with feminism, race politics, community psychology, or ethnomethodology.

[80] George A. Miller, "Psychology as a Means of Promoting Human Welfare," *Amer. Psychol.*, 1969, *24*:1063–1075. In response, Sigmund Koch argued that, contrary to Miller's humanitarian and democratic avowals, he actually makes humans into "objects" and advocates that psychologists "control the subject's control of the subject." Sigmund Koch, "Psychology and Its Human Clientele: Beneficiaries or Victims?" in *Psychology in Human Context: Essays in Dissidence and Reconstruction*, ed. David Finkelman and Frank Kessel (Chicago: Univ. Chicago Press, 1999), pp. 291–311, on p. 299.

claims of authoritarian experimental arrangements or vulnerable subjects. Nor did they incorporate recommendations for a more democratic experimenter–subject system or a subjects' "bill of rights." Some of the problems were addressed in the American Psychological Association's ethics guidelines for research with human subjects, published in 1973 (and followed several years later by the Belmont Report and creation of Institutional Review Boards). These ethics codes assume that experimenters' laboratory conduct is governable. They also assume that subjects are autonomous and resilient, not vulnerable.[81] Consistent with these assumptions about subjects' ontological status, the APA recommended in 1984 that psychology's scientific objects be called "participants," for that term describes them more accurately than does "subjects."

REPLACING MIRRORS?

Even if psychologists never took literally the ideas of Hull's robot or Stevens's pliant organism, even if they never fully believed their scientific object to be a "passive responder" or a "naive" lab inhabitant, for a brief period many reported encounters with a dramatically different scientific object. The object that many witnessed in the several decades after World War II blurred the divides between the real and the apparent as well as the authentic and the enacted. The lively if not cunning objects psychologists observed and described challenged the image of trustworthy, conforming, or otherwise obediently submissive citizens who volunteer themselves for science. Observations of unpredictable participants provoked researchers to devise psychodynamic and socially complex explanations of laboratory activities. Subjects were found to be knowing, deceptively clever, and guarded, and consequently experimenters needed to introduce more clever tactics. These vibrant scientific objects not only prompted reconsideration of the very nature of their subjectivity; they also signaled troubles with the domestic relations of the laboratory. Recurrent, oftentimes stereotypic reports on the implications of participants' race, gender, and sexuality were but symptomatic of more pervasive problems that implicated the experimenter—his comportment, motives, unconscious wishes, anxieties, and defenses.

Psychologists' apprehensions about experimentation transpired in a period fixed on epistemic tensions between representation and simulation and between authenticity and dramatic contrivance. These tensions strained the normative circuitry of the psychological. The period featured, in Dolan's words, "a politics entirely given over to phantasms and simulacra but whose actors are driven by the need to reduce the interpretive ambiguity of their world to the reassuring forms of a metaphysical allegory."[82] Yet considerably more than the militarized Cold War culture facilitated experimenters' apprehension of the experimenter–subject system. Contacts between clinicians and experimenters made during their wartime work together undoubtedly motivated some experimenters to apply clinical and psychological perspectives to see how laboratory life triggered psychic defenses, anxieties, repressions, and denials.[83] And uncertainties about the human condition motivated experimenters to reexamine precepts

[81] American Psychological Association, *Ethical Principles in the Conduct of Research with Human Participants* (Washington, D.C.: American Psychological Association, 1973). On possibilities for a subjects' bill of rights and subject unionizing see Herbert C. Kelman, "The Rights of the Subject in Social Research: An Analysis in Terms of Relative Power and Legitimacy," *Amer. Psychol.*, 1972, *27*:989–1016.

[82] Dolan, *Allegories of America* (cit. n. 29), p. 2. On central tensions of the 1960s see Howard Brick, *Age of Contradiction: American Thought and Culture in the 1960s* (Ithaca, N.Y.: Cornell Univ. Press, 2000).

[83] Even philosophers of science were drawn to psychoanalytic understandings of science. See Herbert Feigl, "Philosophical Embarrassments of Psychology," *Amer. Psychol.*, 1959, *14*:115–128.

about the nature of human nature.[84] Psychology's episode of epistemological dizziness briefly unsettled beliefs that well-designed experimental procedures would detect elements of a real human nature. In the nervous assessments, notions of what was real had pressed up against an ontology of the nominal—the "as if" world of dramaturgy; the established mechanical objectivity of psychology had pushed against personal investments in the laboratory; and the standardized, rationalized work of professional science had rubbed against the autonomous, responsible individual of liberal democracy. Deeply concerned researchers earnestly, if momentarily, began the "epistemopathic surgery" urged by Koch yet ultimately opted for technical remedies. These remedies included measures to regulate the experimenter's motives, to make her yet more objective. More significantly, they constituted "impression management" of the subjects to reduce feigning and channel compliance. Exemplary of these management techniques is the "bogus pipeline paradigm," a technique its inventors promoted as an effective, concrete substitute for the pipeline to the soul about which many psychologists purportedly fantasized. The bogus pipeline was a deception: it was presented to subjects as capable of assessing the assertion of true beliefs and detecting false reports, but in actuality the machine was bogus and served only to impel subjects to report honestly.[85] (It is notable that the bogus pipeline was used to detect racist attitudes held by avowedly liberal-minded undergraduates.)

However, anxieties were not quelled simply by replacing the mirrors held up during the restive period with another mirroring made up of technical tools to make experimental relations real—or seem real. The anxious uncertainty and its demise corresponded with emerging cognitive theories of mind. Apprehensive evaluations of the experimenter–subject system had engaged ideas of phenomenology, the unconscious, and states of unawareness, all of which are matters of cognition that the new cognitive psychology would subsume. As John Adair succinctly described it, "the discipline shifted from subjects' thought processes as artifact to subjects' cognitions as data." Bogus pipelines and other technical illusions eventually were replaced by techniques, like the implicit attitude test, that are believed to measure cognitions beyond the subject's awareness. One instrument designed to assess automatic activation of cognitions and "'get inside the head' of the participant" has been claimed to offer "a potentially bona fide, not bogus, pipeline."[86] Viewed in the context of the emerging cognitive psychology, the nervous confessions of laboratory relations might reflect growing worries about behaviorism. The anxieties also might have bolstered a substantive refocusing on the mind instead of behavior, and this intensified gaze on the interior likely dimmed attention to bodies and social relations. Yet, as historians are finding, the cognitive movement was political as well as scientific, representing an intellectual prophylactic against authoritarianism and an affirmation of democracy. The cognitive sciences emphasize human traits of autonomy, flexibility, and rationality, and, as Jamie Cohen-Cole has argued, they had political meanings: "Cogni-

[84] Such scrutiny of precepts informed the emergence of cognitive psychology when, as Jamie Cohen-Cole found, researchers reflexively extended ascriptions of scientists' psychological capacities (flexibility, creativity, rationality, and capacious reasoning) to their scientific objects. See Jamie Cohen-Cole, "The Reflexivity of Cognitive Science: The Scientist as Model of Human Nature," *Hist. Hum. Sci.*, 2005, *18*:107–139. Cognitive psychology developed, however, to understand humans as not always rational, though the experimenter might be. See Lola L. Lopes, "The Rhetoric of Rationality," *Theory Psychol.*, 1991, *1*:65–82.

[85] Edward E. Jones and Harold Sigall, "The Bogus Pipeline: A New Paradigm for Measuring Affect and Attitude," *Psychol. Bull.*, 1971, *76*:349–364.

[86] John Adair, "Social Cognition, Artifact, and the Passing of the So-called Crisis in Social Psychology," *Canadian Psychology*, 1991, 32:445–450, on p. 445; and Russell H. Fazio, Joni R. Jackson, Bridget C. Dunton, and Carol J. Williams, "Variability in Automatic Activation as an Unobtrusive Measure of Racal Attitudes: A Bona Fide Pipeline?" *J. Personality Soc. Psychol.*, 1995, *69*:1013–1027, on p. 1014.

tive scientists not only epitomized the democratic character, but their account of humanity was more attractive."[87] Emphases on human autonomy, reason, and problem solving offset concerns about undemocratic experimental arrangements. The experimental anxieties of the postwar decades, like those of the Cold War, thus might have been alleviated by a political-ontological transformation of both subjects and experimenters.

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[87] Jamie Cohen-Cole, *The Open Mind: Cold War Politics and the Sciences of Human Nature* (Chicago: Univ. Chicago Press, 2014), p. 6. Other scholars have found in cognitive psychology an ideal of flexible individuals and an eschewal of material, bodily conditions suited to the needs of modern capitalism. See, e.g., Edward E. Sampson, "Cognitive Psychology as Ideology," *Amer. Psychol.*, 1981, 36:730–743. The Cold War quest to understand rationality is examined in Paul Erickson, Judy L. Klein, Lorraine Daston, Rebecca Lemov, Thomas Sturm, and Michael D. Gordin, *How Reason Almost Lost Its Mind: The Strange Career of Cold War Rationality* (Chicago: Univ. Chicago Press, 2013).