

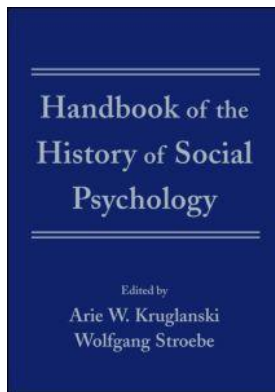
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2 The importance of history to social psychology

Jill Morawski

When invited to contribute a chapter on the importance of history—on why social psychologists should consider their discipline’s history—I delighted at the opportunity to share some of the innovative, sometimes provocative histories that lay outside the purview of canonical histories familiar to social psychologists. Soon, however, the project encountered perplex, a sticking point: Readily available textbook histories of social psychology suffice in presenting an orderly narrative of the field’s evolution. Canonical (internal and textbook) histories recount a science that adopted a particular understanding of the social as quintessentially *psychological* phenomena along with methods that privileged experimentation and quantification. These insider chronicles highlight a social psychology that absorbed as well psychology’s presuppositions that phenomena are *transhistorical* and *universal* (holding for all humans across time). These narratives travel smoothly into the present, for contemporary social psychology still consorts with what became its master discipline, assimilating current trends toward cognitive, neuroscientific, and evolutionary psychology. In so much as official histories outline social psychology’s trajectory and scientific allegiance, why would social psychologists need or want more?

The perplex thickens when introducing the histories that I entreat fellow social psychologists to explore. These histories, broadly described as “contextual,” understand the discipline’s developments to be choices made: They were neither inevitable nor even empirically determined. More importantly, contextual histories scrutinize the consequences of disciplinary decisions along with options abandoned or overlooked. They trace social psychology’s theories and epistemological premises, examine its economic and political imperatives, and probe the cultural origins and meanings that made today’s social psychology possible. Why explore social psychology’s complicated, sometimes even messy, hidden, or vestigial heritage? This perplex stymied me notwithstanding training in experimental social psychology and a research program committed to the history of psychology. Yet in this rather idiosyncratic career lay a key to loosening the perplex: the commitments to science shared by both social psychologists and recent historians of science.

Three commitments highlight what importance history can have for social psychology: evidence, methods, and theory. Contextual, evidence-based histories bring to the fore the actual practices, decisions, and even elisions of social psychology. In

so doing they encounter theories that were abandoned and evidence laid aside, uncovering the ways that social psychology has both depended upon and contributed to material conditions of the social world—economics, technologies, institutions, public policy, and cultural beliefs. With attention to the evidence from the broader cultural and scientific context, they bring to the fore some of the intellectual developments in the human sciences, such as the revolution in linguistics and studies of reflexivity, that were largely eschewed by social psychology. These are facts about social psychology’s past from which the matter of methods follows. Mainstream histories, in outlining a trajectory that accords with contemporary self-understandings, use evidentiary bases akin to those of autobiography and, as such, they are susceptible to the evidentiary limitations of self-report found in autobiographical memory. The methods of contextual histories are hardly singular, yet they engage methodological standards more closely aligned with aims of empirical observation. Attending not only to esteemed experiments or research techniques, these studies also examine the financial incentives, political climates, personal influences, normative assumptions, and intellectual relations with other social sciences. In other words, they investigate more closely—and broadly—the actual scientific practices that made social psychology mature as it has. Contextual inquiries, then, have empirically rich grounds, making possible more comprehensive accounts of the circuits that link the science to technologies, culture, politics, money, and individual actors.

As for the matter of theory there are two notable points of congruence between social psychology’s enterprise and histories of the science. One convergence concerns an investment in assessing contemporary theory. Whereas canonical histories strive to provide a linear chronicle of scientific progress that points to contemporary work, contextual studies recover the variations and fates of theories; among the findings of the latter approach is a once capacious and creative theorizing about the social psychological world that was directed and ultimately diminished through incorporation of core precepts from psychology proper (and more recently from that master discipline’s interest in neuroscience, biology, and economics). Contextual histories thereby better inform us about social psychology’s once indigenous theorizing that, in turn, enables fuller knowledge about possibilities for future as well as current theories. Contextual history’s attention to the creativity,

variations, and fate of “native” theories signals yet another matter. Reliance on a confined or monolithic theoretical perspective, say information-processing models, inevitably compels a narrowing of hypothesis testing and a tendency to take that theory perspective as not just *descriptive* but *normative*, as not just a possible way for describing a phenomenon but as compelling (the world must be this way). Awareness of the once vivacious and plural theorizing returns to us the potentials and challenges of different theory choices. As legal theorist Janet Halley notes, “Sustaining competing theories for describing the same social arrangements can expand our sense of the stakes at stake when we make our choices about what to see as a social good and a social bad, how to understand their distribution, what to think of as normatively bad, and what to aspire to” (2006, p. 8). Recovering the dynamics of theory construction in social psychology can enrich the scientific enterprise, reintroduce comparison of theories, and uncover normative assumptions. Additionally, it might well refresh social psychology’s integrity.

These three commonalities—facts, methods, and theory—lay grounds for social psychologists to appreciate histories inspired by recent developments in the history of science. Yet even with these common grounds, thinking historically about social psychology is methodologically and conceptually complex; accordingly, the chapter will ask much of its readers. It bids readers to bracket conventional scientific self-conceptions, including some basic tenets about what is “social,” and to regard scientific undertakings as normative (as well as descriptive) acts in the making and sustaining of the modern social world. The gains might be modest, as in exposing implicit cultural premises of research, or they might be more far-reaching, intimating how a historically enriched social psychology ultimately benefits historical scholarship. My own stance allies with this latter potential for developing a historically grounded social psychology. Our discipline and history both could benefit from heeding each other’s work. In the messy project of understanding social life, all hands are needed on deck: Just as social psychology needs history, so history needs the skills and insights of social psychology. One potential project for forging this alliance is taken up at the chapter’s end.

The chapter proceeds first with brief review of advances in the historiography of the natural sciences, developments that register limitations of canonical, internal histories and suggest fertile partnerings of social psychology and history. Given notable advances in the study of reflexivity and language, the next two sections take up histories that relate these subjects to social psychology. They are followed by introduction to histories that examine scientific practices: These studies move from asking *what* knowledge is generated to asking *how* social psychological knowledge is produced. Studies of scientific practice (whether they entail microanalysis of a project or far-reaching mappings of material connections between the science and other realms) can yield a broad, empirically grounded purchase on social psychology. They expose significant if often unnoticed ingredients of research that are not explicated in

research programs or reported in scientific articles. The concluding section is forward looking, offering one generative possibility for an alliance of history and social psychology by borrowing from an unrealized late 19th century project. The conclusion thus is conjectural if promissory, intimating how what starts as an apparent perplex can open the way for innovation.

In the end, fuller historical understanding is important for all social psychologists, whether they want greater accuracy in their ongoing research programs or seek innovative, interdisciplinary models. Calling for fuller historical knowledge presumes no single theoretical or philosophical perspective. In this sense, appealing for greater awareness the field’s history is analogous to calls for representative sampling of research participants. The authors of a recent review of psychology’s biased sampling marshal evidence indicating that psychology research needs better representation of the world population. While one might readily presume that demands for diversity in subject selection would be made by researchers committed to a “cultural” or “nurture” perspective, the authors are, in fact, inclined toward more biological perspectives. For them, reliable knowledge is paramount and it is necessary to place any knowledge of “universal features of psychology on a firmer footing by empirically addressing, rather than *a priori* dismissing or ignoring, questions of population variability” (Henrich, Heine, & Norenzayan, 2010, p. 3). Similar logic holds for histories that are incomplete or partial. By ignoring, for instance, the varied aspirations underlying Floyd Allport’s mandate for “methodological individualism”—a mandate that dramatically altered the course of North American social psychology—grounds for continuing that now nearly hegemonic presupposition remain empirically (and philosophically) underdetermined, and its entanglement in behaviorist, populist, and psychoanalytic assumptions goes unquestioned (Graumann, 1996; Greenwood, 2004; Parkovnick, 2000). History thus can importantly inform science.

Histories of a science

During the past half-century, a transition generally marked by Thomas Kuhn’s thesis on scientific revolutions transported the historiography of science beyond formulaic narratives of scientific progress and great scientists, and beyond debates over whether scientific change is better explained by “internal” laboratory conditions or “external” ones of funding, politics, and *Zeitgeist*. Teleological chronicles of the march of scientific progress have since been replaced by a historicist or contextualist perspective that understands past science in the context of its unfolding. Historians now scrutinize details of the entire scientific field: conceptualizations of objects, variables, training, instruments, financial supports, culture, and language. They chart the vibrancy of scientific practices—the back-and-forth relays of information, materials, techniques, and customs between what is conventionally demarcated as “science” and “culture”—tracing the “connectivity of science” (Galison, 1997).

Until the past three decades, histories of the social sciences were considered apart from the history of the natural sciences; for the social sciences, psychology exemplary among them, historical narratives were crafted mostly by practicing social scientists themselves. These presentist (in portraying progressive paths to present science) canonical testimonies have served laudatory ends. They do disciplinary boundary work in claiming certain methods, excluding certain events, and defining the discipline's principal domain of inquiry in relation to other social sciences (Good, 2000). E. G. Boring's 1929 history text, long serving as psychology's official chronicle, exemplifies such systematic emphasis on certain subjects and methods while excluding or sidelining others. Among the discipline's unsung accomplishments in Boring's history are applied, developmental, and social psychology (O'Donnell, 1979).

Two intellectual movements came to challenge these presentist histories "without the past" (Leahey, 1986, p. 649). In the 1970s, many social scientists found history a fruitful means to better understand their discipline's trajectory, priorities, and exclusions. Around the same time, professional historians became more interested in the impact that the social sciences had on 20th-century culture and society (Porter and Ross, 2003). Among them, historians of science found in psychology, a near science, abundant opportunities for interrogating the extension of scientific ethos to traditionally humanist subjects such as rationality, sociality, and mind. Together, these scholars amended inaccuracies in presentist histories, excavated cultural impetuses behind social scientific undertakings, and promoted historiographic perspectives being forged in the history of science (Lubek, 2000; Samelson, 1974; Stocking, 1965).

These histories, alternatives to the canonical textbook chronicles, are retrieving a legacy of social psychology that is far more extensive, culturally consequential, and intriguingly complex. Contrary to accounts locating North American social psychology's inception in a specific discipline-based event such as the first experiment (Triplet), textbook (McDougall or Ross), or individual-centered theory (Floyd Allport), they discover other impulses motivating modern social psychology. An exemplary case is found in late 19th-century social psychology. Historian Jeffrey Sklansky (2002) has linked the mid-19th-century emergence of social-psychological theorizing with classical economics' failure to adequately explain ongoing transformations in economic behaviors and institutions. Economic thinkers in the antebellum era confronted problems in explaining structural changes in market behavior: Substantive economic transformations challenged classic economics' base assumptions about market actors and their interrelationships. In brief, they turned to understandings of the market "conceived not as a federation of autonomous agents but as an indivisible union of interdependent parts" (p. 228). Until then, American conceptions of society and political economy assumed an agent's independence or autonomy, taking his competitive self-interest as the basis for commerce. But these premises about individual actors and the market faltered as explanatory models after the Civil War, failing to account for changes in

manufacturing, labor and property ownership. No longer sufficient was the idea of independent "economic man"—"The self-governing individual, endowed with the natural faculties of rational will and productive labor, entitled to the natural rights of property and popular sovereignty . . ." (p. 3). New conceptions of interdependence and social-psychological cohesion gleaned from social psychological scholars better explained economic activity. Responding to economic changes and instructed by social-psychological writings, late 19th-century social scientists "reconceived market society as a fast-moving mainstream of culturally created desires, habits and mores, instead of an unchanging arena of contract and competition among independent proprietors" (p. 3). They forwarded a social psychology to describe and explain the "seamless relation between self and society" and a "newly socialized psyche freed from the confines of class and individual self-interest." According to Sklansky, social psychology of that era "represented a progressive challenge to the reigning ethic of competition and accumulation, which had become loosely identified with the old science of wealth" (pp. 9–10).

Sklansky's excavation of the dependence of economic models on social psychological knowledge illuminates how crucial was modern social psychology's overarching mission to decipher the shifting arrangements between the individual and the social world. His study shows, too, how only by situating social psychology in its historical context can we fully understand how its ideas (notably about individual/society relations) were directly connected with material events in the social world. Detailed historical exposition offered in this history provides insights into the material and social events that influence contemporary work just as it illuminates theories that differ from our contemporary individual-centered social psychology.

Historiographic emphasis on science-in-culture also brings to the fore economic or political motives underlying supposedly neutral scientific values. Close examination of social psychology's lean toward natural scientific inquiry does not confirm canonical lore that attributes the rapid ascent of experimental techniques to a natural maturation of the science after the Second World War. To the contrary, American social science in the postwar era met significant governmental pressures to adopt scientific modes of operation (Solovey, 2004). The social sciences were charged to become a certain kind of science in order to maintain funding and professional regard. Social sciences, including social psychology, implemented a natural science model for reasons other than strict logical reasoning or empirical evidence alone; they were fueled by and comported with political and cultural interests. If such histories seem to tarnish a discipline's image, what warrants appreciation is how they recover a more vibrant, intellectual, and culturally complex science.

Another advance in historiography of science is particularly relevant to social psychology, present as well as past. A number of philosophers of science, psychologists, and historians have independently argued for a category distinction between the natural and human sciences, one acknowledging the unique

difference between the objects of study in these two scientific domains. This distinction has crucial implications for social science. First, objects in the natural sciences are indifferent to the claims made about them; they do not change as a result of new scientific claims made about them. By contrast, humans, the objects of social science, can react and respond to claims posed about them. Second, in the human sciences the observer and object of observation are one and the same kind, and this reflexivity manifests in an array of empirical and theory implications. Finally, social knowledge and its making depend on the social world in important and inescapable ways that do not occur in the natural sciences. In other words, social knowledge is intrinsically culture-bound, as evidenced by the fact that it is unimaginable how we can know “even ourselves, except in terms that we acquire through living in a particular time and place. To conceive otherwise would be to conceive of having knowledge without language or symbols” (Smith, 1997, p. 17). These essential conditions of the human sciences are not entirely unknown to social psychologists. In fact, social psychologists have been pre-eminent in responding, however tactically, to some of them by creating techniques for their management in research situations. Perhaps because of such masterful laboratory management of reflexivity, researchers have neglected its fuller manifestations. We can turn to historical studies to better comprehend these intrinsic conditions of studying the human social life.

Reflexivity

In the opening pages of an exquisitely compiled history of the human sciences, Roger Smith states a core paradox of the human sciences, asking “How are we to stand back from being human in order to observe what it is to be human? Even to attempt this standing back—and there are many ways in which it has been undertaken in pursuit of scientific truth—is a way of being human that, in turn some other person will be able to study.” He continues questioning, “are we then condemned to travel in self-reflecting circles, to create knowledge of human beings only to find that what has been done is to create another mode of life rather than a lasting truth?” (1997, p. 13). Smith broaches what Steve Woolgar (1988) calls the “horrors of reflexivity,” and anticipates the dynamic, even generative consequence of reflexivity. These extensions of reflexivity are taken up in the next section but it is first necessary to corral and identify the several meanings of this essential property of the human sciences.

In its barest meaning, reflexivity is the inescapable, self-referential attribute of theory. Reflexivity inheres in “all social sciences since any statement that holds that humans act or believe in particular ways in particular circumstances refers as much to the social scientists as to anyone else” (Gruenberg, 1978, p. 322). To be valid, social-psychology theory should be predictive of or explain the relevant thoughts and actions of the scientist as well as those of his or her subjects. A second meaning of reflexivity implicates all sciences and scientists

who observe objects in the world insofar as their observations are necessarily guided by pre-existing understanding of those objects (Woolgar, 1988). Reflexivity here refers to the back-and-forth process through which an account of the world depends in fundamental ways on pre-existing knowledge of what that account refers to and vice versa. Modern psychology’s conceptions of “motivation,” for instance, depended on pre-existing understandings of “will” and residuals of that understanding of will persist in scientific studies of motivation. Both meanings posit reflexivity as at once unavoidable and integral to generating claims about the world. They take as fundamental the complex binding of research and the rest of the world. However, investigators differ appreciably in how they heed or engage reflexivity. Depending on their conscious awareness (self-conscious regard) and motives, reflexivity can be intended or inadvertent, engaged or eschewed, appreciated or scorned. When reflexivity entails a self-conscious regard, a turning back upon oneself, it allows “those who do science to better understand the social mechanisms orienting scientific practice, thus enabling them to become ‘masters and possessors’ not only of ‘nature,’ according to the old Cartesian ambition, but . . . of the social world in which knowledge about nature is produced” (Pierre Bourdieu, quoted in Mialet, 2003, p. 614). Psychologists of the social world have not been entirely blind to or dismissive of such reflection. A century ago William James observed the inescapable reflexive process and advised, “The knower is not simply a mirror floating with no foot-hold anywhere, and passively reflecting an order that he comes upon and finds simply existing. The knower is an actor, and co-efficient of the truth on one side, whilst on the other he registers the truth, which he helps create” (quoted in Leary, 1995, p. 93).

Twentieth-century researchers who practised such self-conscious reflection often targeted psychologists’ very blindness to reflexivity—their seeming unawareness of how newly honed, rigorously prescribed methods actually constrain or distort their purportedly objective observations of social life. Closely inspecting what Bourdieu would later call the “social mechanisms orienting scientific practice,” Horace Mann Bond (1927) delineated the tacit rules of the scientific “game” that governed avowedly objective, empirical studies of race differences in intelligence. He found that seemingly neutral methodological decisions actually skewed both observation and measurement, yielding results that complemented racial stereotypes. Bond’s list of these rules is extensive, including that the experimenters/examiners are white (nonwhite researchers either were not considered or barred access from research with white students, as Bond was); the tests are standardized on white children and by white researchers; subjects’ social status is ignored, being deemed irrelevant; nativist assumptions about race differences are assumed, as evidenced in the then commonly held notion that high-scoring whites come from families of high social status because the family is intelligent.

Saul Rosenzweig (1933) similarly explicated the social psychological processes transpiring underneath the actions being observed in experiments. He described how both subjects

and experimenters proceed with intentions and perceptions that are neither acknowledged nor represented in the experimental hypothesis, yet these intentions and perceptions shape experimental outcomes (Morawski, 2005). The psychology experiment, Rosenzweig concluded, has an additional, largely unobserved, social psychology. Likewise concerned with researchers' unacknowledged intentions, Gordon Allport devoted much of his APA Presidential address (1940) to this matter; for instance, psychologists' privileging of quantification as their "frame of reference"—a framing that constricts and distorts the phenomena being observed. (In a playful visualization of researchers' unawareness, Allport graphed this escalating quantification complete with a psychologist jauntily rocketing up the graph line.) Twenty years later, Donald Oliver and Alvin Langfield pronounced reflexivity an "unfaced" issue of psychology. It is, they held, a real property of human nature that must be figured in psychological research. Oliver and Langfield forcefully claimed that disregard of reflexivity undermined scientific psychology because "any psychology venture is a failure if in its accounting it fails, or refuses, to take into account its own accounting" (1962, p. 117). "Unfaced" rather aptly describes researchers' regard for reflexivity, for despite the uncertainty principle in physics, new self-interrogatory methods of literary theory, and a "linguistic turn" in many kindred social sciences in the 1970s, few psychologists—social psychologists included—have systematically interrogated the reflexive properties of research (Gadlin & Ingle, 1975; Morawski, 1992, 2005; Richards, 1987, 2002; Unger, 1983).

If the psychological sciences largely disregarded substantive forms of reflexivity, there was a brief period when it did come under sustained scientific scrutiny. Beginning with the innovative social-psychological experiments of Martin Orne and Robert Rosenthal, some researchers began to assess the ways that experimenters' expectations and biases (as well as those of subjects) influenced experimental outcomes. These sometimes alarming findings spurred creation of techniques for reducing or eliminating such contamination. Although the methodological correctives were adopted by many, they target only readily detectable biases present at the observational stage of research. The correctives, for the most part, leave unaddressed the first two forms of reflexivity discussed above. Important and potentially transformative research on these two other forms of reflexivity—the self-referential axiom of any human science and the double hermeneutics of human science—can be informed by historical case studies. Analyses of pre-existing understandings of psychological phenomena circulating through research serve as distinctive means to identify core precepts (as in the case of will lingering in constructs of motivation). Histories of reflexivity afford means for interrogating not only past conceptions but also contemporary ones. They offer methods as well as analytic frameworks, for example, to locate the moral and religious values underlying current concepts of "happiness" or clinical and cultural conceptions of the unconscious undergirding current research on implicit attitudes.

Although it is sometimes difficult or impossible to ascertain whether researchers' engagement with reflexivity is intended or unintended, the consequences of reflexivity can be detected through contextual or textual analyses. Several studies illumine the back-and-forth processes of reflexivity by chronicling vectors of self-regard/other regard that are constitutive of psychological knowledge. Investigating postwar conceptions of personhood, particularly researchers' thinking about thinking, Jamie Cohen-Cole (2003, 2005, in press) located a constellation of psychological attributes that captivated a group of psychologists in the 1950s. Through extensive examination of institutional and personal archives, debates among researchers, laboratory records, informal exchanges, think-tanks, and publications, Cohen-Cole traced social scientists' promotion of a new set of human attributes that constitutes an "open-minded character" and that soon became bedrock presuppositions for cognitive psychology. Reacting against the mechanistic, rather passive personhood presupposed in then-dominant learning theory and behaviorism more generally, and simultaneously desiring a model of personhood that bespoke a more promising sociopolitical life, researchers who were to become the vanguard of the cognitive perspective extended their own self-concepts as scientists—flexible, creative, autonomous, and rational—to their subject matter, other human beings. Consequently, work on authoritarian personality intimated the existence of and need for a different, more democratic, flexible character. Studies of closed-mindedness similarly inspired studies of cognitive openness; at the same time research on creativity boomed. Soon there emerged a new psychological type whose attributes of flexibility, reason, openness, and creativity mirrored the ideal attributes of a scientist. As George Miller remarked in 1963, "the scientist is Everyman, looking just as you and I. We go and look for the things we want, and when we find them we find part of ourselves" (quoted in Cohen-Cole, in press, p. 186). In keeping with their dissatisfaction with closed, mechanistic models (like those of behaviorism) researchers embraced an antipositivist philosophy of science that stressed the importance of "good thinking." They ultimately extended that desideratum to describe human nature generally. In so doing, they refashioned the human psyche in the likeness of the creative scientist, making it possible to observe a scientific object who was a thinking, creative, and cognitively complex actor, not a passive, rigidly socialized one. Self-imagining, then, begat a scientific model of personhood.

The reflexive play of self-regard/other regard at times has been influenced by scientists' direct experiences in research. In an insightful microanalysis of postwar psychologists' self-regard, Gerd Gigerenzer (1991, 1996) found that researchers' concerns about their own cognitive capacities—namely the accuracy of their judgments—led them to adopt techniques of statistical inference that they presumed to be superior to human decision making. The psychologists creatively drew upon these investigative tools to confect models of thinking, figuring the mind as statistical processor. Research tools, then, shaped a theory of mind.

Reflexive practices are not always consensual, even in a highly regulated discipline. In fact, despite a near hegemonic methodology, psychology in general and social psychology more specifically, have exhibited (although not necessarily admitted) conceptual pluralism (Richards, 2002; Smith, 2005). Researchers have harvested conceptions of persons from a culture rich in prototypes of human character. One particular conception that appealed to social psychologists is found in extensions of recent experiences of war, combat, and brainwashing to design models of the social self as vulnerable, fragile, and defensive. With a consequential awareness of human vulnerability along with defensive attitudes of Cold War American politics, some postwar researchers moved toward vesting persons with (and needing) a certain protective self-psychology, and they described that self in terms that Catherine Lutz (1997) has called an “epistemology of the bunker.” Cultural politics of permanent war and covert warfare along with psychologists’ war-related work experiences promoted construal of “a new more vigilant self, a self not so much explicitly disciplined as suspicious of itself” (p. 245). Given apprehension of a dangerous world, the mind was reconceived as having mechanisms to be protective and subversive, even self-subverting. This defensive yet vulnerable self made its appearance in empirical studies of compliance, coercion, defensiveness, and self-deception. It even prompted researchers to worry about subjects’ possible subversions of experimental situations. World events—or more accurately, perceptions of these events—engendered assumptions about the self. Cultural preoccupations, then, inspired models of human nature.

In addition to conceiving a self radically and defensively separate from others, a conception that notably influenced research on person perception and social interaction, other notions of human subjects resonated with postwar culture, thereby bearing imprints of inadvertent reflexivity. Both a cognitively complex, resilient type of human and a vulnerable one traveled through social-psychological thinking of the 1950s and 1960s. In an archival study of the ethical debates over the use of deception, Laura Stark (2010) corroborated the coexistence and liveliness of these two types of person. By the 1960s experimental deception had become arguably the most heated concern of research ethics; its prominence is evidenced by the attention given it by the APA committee mandated to develop a code of ethical practices for research with human subjects. Apprehending the possibility that regulations would be imposed on psychology research, the APA sought to pre-empt that undesirable outcome by producing its own regulations. The committee’s deliberations over whether it was appropriate to deceive subjects hinged on rudimentary assumptions about human nature. On one hand, taking subjects as vulnerable and susceptible to influence implied the need to restrict the use of deception techniques. On the other hand, seeing them as resilient and autonomous suggested that a modicum of deception would have no adverse or lasting effects. Legal, economic, and political contingencies, then, charged debates over basic assumptions of personhood and ultimate resolution of conflicting points of view.

Inadvertent reflexivity, the relay of self-conceptions and cultural conceptions (in and through social science) is ubiquitous. Rarer are self-conscious accounts of reflexivity such as forwarded by James, Bond, Rosenzweig, and Allport. Abeyance of reflexivity is understandable, especially in a science that seeks to differentiate its knowledge from common cultural understandings of social action. For enterprises like social psychology, reflexivity evokes a “horror” that distinctions between social psychology and everyday social knowledge might collapse. That is, if research encompasses or refashions pre-existing understandings of the world, then how does one distinguish scientific knowledge from other knowledge about the social world? What then privileges empirical knowledge from common-sense knowledge? (Perhaps that worrisome apprehensive helps explain why surprising and counterintuitive research findings are valorized in social psychology, for it is precisely such findings that dramatically depart from everyday accountings of social life.) Whatever the reason, evading reflexivity entails neglecting core human properties and capacities—to understand social life, iteratively interpret, and alter unfolding social actions. We neglect to consider if and how reflexivity is a social-psychological phenomenon in our objects of inquiry as well as in our own inquiries. The alternative, admitting reflexivity, permits researchers to identify how, when, and with what consequences their foreknowledge preconditions scientific knowing. We can see better the connectivity of scientific conceptions and worldly ones; such investigations turn feared collapse of scientific and everyday accountings of social life into an opportunity to observe their very interdependence. And acknowledging this connectivity makes visible the ingenious translations from one domain to the other. To recognize reflexivity, therefore, expands the entire investigative vista, granting opportunities to observe previously elided processes and track the dynamic relays between social psychological knowledge and ways of being persons.

Making the social

A provocative history of the human sciences by Bruce Mazlish (1998) aims to show how the human sciences can be at once “scientific” and also attentive to the diachronics of consciousness, including reflexivity. Mazlish proposes that the human sciences are emergent knowledge forms in the sense that significant intellectual advances ensue from transformations in those sciences’ objects—both societies and social actors. The human sciences, therefore, are dynamic (uncertain) sciences. He dismisses simplistic edicts that prediction, control, and determinism are grounds for social scientific knowledge. These classic social science aspirations are complicated if not crippled by a capacity of social actors that social psychologists observed and recorded nearly half a century ago: “if humans knew how they are supposed to behave, they may behave otherwise” (p. 17). In lieu of these canonical goals of social science, Mazlish argues for a cultural evolutionary conception of consciousness wherein large-scale changes in cultures (technologies, language, symbolism, social

relations) compel new social understandings or new consciousness that includes the human sciences themselves. He draws a strong cocausal relation between human life forms and the human sciences, a relation indicating that “there can be no economic science until a market economy has emerged. There can be no sociology until the concept of society emerges out of the changing forms of human co-habitation, as in the shift from the feudal to the industrial” (p. 75).

For Mazlish (1998) the intrinsic relationships among practices, consciousness, and scientific interrogations mean that the human sciences inevitably involve degrees of uncertainty. They are “uncertain sciences,” yet they are sciences nevertheless. The prescriptive and contingent characteristics of social knowledge assures uncertainty because, as he observes, “any law in the social sciences is part of a process including prescriptions that foster change, which then creates new conditions in which that law no longer effectively applies” (p. 189). In other words, the very scientific shortcomings of the human sciences are at least partially produced by their success. Transformations in human behavior result at least in part from the assimilation of social scientific knowledge, and these transformations can invalidate the knowledge that motivated them. Put otherwise, Mazlish proposes that the “social” of psychological and societal life is different kind of object than the objects studied in the natural sciences. The social is more mutable and generative than quartz; its evolution is far more rapid and diverse than genes. Implicit here is the need to retire the “methodological individualism” that undergirds social psychology for the individual is always social and this sociality changes over time.

Perspectives like Mazlish’s challenge the conventional boundaries between social life and formal scientific accounts of that reality; between knowledge and culture; and between the real and the nominal. In so doing they accord with Anthony Giddens’ (1984) concept of social sciences “double hermeneutic.” As the first part of this hermeneutic, Giddens posits that social scientists initially draw upon cultural interpretations to initially locate and define their object of inquiry—for instance to formulate working definitions of “frustration” or “romantic attraction.” This is the first hermeneutic or interpretive task. The second hermeneutic step transpires upon completion of the scientific analysis, when scientific interpretations of that object are returned to the rest of society. A growing number of scholars hailing from various disciplines are forging similarly historically grounded models of the relations of social/human science and social actions. In acknowledging reflexivity, these models articulate dynamic feedbacks—what Ian Hacking (1995a, 2002) calls “looping” of human kinds and Graham Richards (1987, 2002) terms the “circuitry” of the psychological. Scientific (and other authoritative) representation of social life changes the very ways of being social, of being persons; these changes in turn inform subsequent scientific investigations. As Hacking describes this relay, “To create new ways of classifying people is also to change how we can think of ourselves, to change our sense of self-worth, even how we remember our

own past. These, in turn, generate a looping effect because people of the kind behave differently and so are different. That is to say the kid changes, and so there is new causal knowledge to be gained perhaps, old causal knowledge to be jettisoned” (1995a, p. 369). Hacking claims not that self-worth is necessarily a relative phenomenon but, rather, that its form changes in significant ways and now does so in part because of the scientific knowledge found about it.

Apprehending these looping effects, philosopher Alasdair MacIntyre (1985) rather boldly asserts that people are not the same as they were before psychology was established, even proposing that “Psychologists have had varying (sometimes striking) success in interpreting the human world; but they have been systematically successful in changing it” (p. 897). According to MacIntyre, it could not be otherwise. To illustrate how psychology’s empirical descriptions inevitably make their way to ever more robust prescriptions and new actions, he draws upon social psychological studies of balance, dissonance, and attention—a theory mode he calls “the invisibility of rationality in other people.” Social psychologists’ demonstration of humans’ failure to be fully rational is “culturally influential” in conditions where individuals aim to affect others. He argues, “advertisers, political campaign managers, and others succeed, not only in acting on the hypothesis that the rest of us are by and large nonrational sheep, but also in communicating to us that is how they think about us” (p. 901). MacIntyre emphasizes researchers’ own cultural practices, such as the experimental arrangements that structure interactions between so-called rational actors (experimenters) and irrational ones (subjects), arguing that these very arrangements are constitutive of rationality and irrationality. Whenever other human practices (such as education and commerce) incorporate similar social arrangements, they tend to import such binaries (rational and irrational) as well as prescriptions for action.

The social sciences are hardly unique in effecting such changes in human thought and action, and social psychologists who consider the transformative effects of their science can be informed by other transformative actions and entities. Outside the realm of scientific research there is now considerable evidence of how technologies and technical practices alter human actions and self-conceptions. Invention of the internet and related computer technologies that made virtual reality possible also have made possible perceptions of self as plastic and alterable, perceptions that were previously merely theoretical conjectures about the self’s mutability, partiality, and fragmentation (Turkle, 1995). Inventions in robotics and cybernetics have, according to some, brought into being the “posthuman” who resigns material embodiment and consciousness in favor of privileging information flows and a human being “seamlessly articulated with intelligent machines” (Hayles, 1999, p. 3). The subjectivity of the posthuman is not the liberal humanist subject with will, body, and a “natural” self. The psychological sciences, charged with microscopic inspection of human nature and assessment of human action, occupy an important, but not the only, place in the making of forms of personhood.

The feedbacks through which scientific practices (methods, theory, and epistemology) engage and co-constitute ontology (the nature of human nature) is clearly evidenced in survey research. Sarah Igo's *The Averaged American* traces the rise of survey techniques that have aimed to calibrate the normative—the collective whole or “us” as well as the individual or “me”—of American society in the 20th century. Survey research designates the ordinary person as the subject and ultimately also consumer of the resultant knowledge. Survey data influence both the public sphere and individuals, providing the latter with ever changing indices of themselves. Survey findings at once describe the public and individuals' patterns of living, and they have come to serve as prescriptions for living. They entice people to ask questions and seek answers—about what is important about them, what is changing about them, and what specific ways they approximate or diverge from the norm. Survey information thus “enabled individuals to filter their experiences through tables and percentages, to fit themselves into social scientific categories and to identify with strangers” (Igo, 2007, p. 20). As Igo found, individuals were not always readily compliant participants or recipients of the findings: Just as they came to assess themselves and alter their consciousness of self and others, so they sometimes spoke up, contesting what was measured or researchers' interpretations of the measurements collected. If researchers cherished reformist or emancipatory aims—aspiring, as did Alfred Kinsey, to unleash individuals from social myths and conventions—the massive, nearly ubiquitous uses of survey techniques to probe virtually every aspect of life had the effect of making “statistical citizens” who reflect upon data about themselves and others in newspapers, magazines, television, textbooks, and the internet. Surveys soon became part of everyday thinking about society and a pertinent source of self-rumination, part of understanding the “public” and oneself. They served to delineate a “mass public” and “also shaped the selves who would inhabit it, affecting everything from beliefs about morality and individuals to visions of democracy and the nation” (Igo, 2007, p. 282).

These cases intimate historical ontology, the dynamic looping of self and knowledge about selves, but do not presume it to be a matter of social construction, some top-down scientific invention of human objects (Hacking, 1999). Instead, they detail the co-constitution of scientific knowledge and human forms (ontology), showing such co-constituting to involve cultural imaginings, economic arrangements, social structures, material conditions, and even abstract presuppositions of the science (such as determinism, reductionism, ameliorism). This circuit of social knowing and being “is flexible, often indeterminate in its effects, at some level ‘natural’ as opposed to ‘artificial’ and emanates . . . from its own reflexive struggles to know itself” (Richards, 2002, p. 30). The intricate, multiple connections between what Graham Richards has distinguished as “Psychology” (the science) and “psychology” (the psychological world) can and do sometimes yield unexpected, counterintuitive, and even inadvertent consequences.

The past half-century's investigations of menstrual experiences illustrate inadvertent outcomes of investigative projects

(Parlee, 1994). Aiming to promote unbiased research and challenge sexist assumptions about women's psychology—specifically notions of the psychological deficits imposed by female reproductive physiology—a number of feminist researchers, many of whom were social psychologists, called for more rigorous empirical research, including physiological measures, of women's experience throughout the menstrual cycle. They specifically urged research that distinguishes between cultural and biological influences on women's experience. At the time, the late 1960s, extant research on the possible effects of hormonal cycling was scant, although there existed popular conceptions of women's impaired psychological functioning at certain times in the menstrual cycle along with accompanying arguments that this impairment affects women's performance. With researchers' call for better-designed studies, the matter of women's periodicity soon garnered attention, yet this rising interest came not only from the feminist researchers themselves but also from differently interested groups, including medical researchers, physicians, clinical psychologists, pharmaceutical companies, and consumers. Further, researchers' demand for studies that distinguish between cultural and biological influences had the inadvertent effect of bifurcating the research program: Medical and life scientists seized the biological domain, leaving psychologists to investigate cultural and psychological ones. The research agenda thus was divided and feminist psychologists sidelined from important discussions. The extensive if fractured interrogations along with revived “hormone talk” ultimately if inadvertently magnified notions (and quite possibly experiences) of physiological gender differences. The very scientific focus raised public visibility of the matter. Also, the concomitant dramatic growth in pharmaceutical industry and emergence of biomedical notions of body and mind (understandings of individuals as products of biological processes) incited beliefs that the female hormonal cycle *must* have psychological effects. And as Parlee observed, these beliefs circulated during a period when gender relationships were particularly fraught and when men and women sought expert explanations for personal situations. The ensuing rhetoric of fluctuating hormones, emphasis on negative symptoms, and the new nomenclature of PMS (premenstrual syndrome) trumped contravening social-psychological studies that reported considerable cultural factors. Biologically inflected rhetoric likewise obscured new empirical work that indicated no significant effects of hormonal cycles, reported situational (not biological) influences on women's (and men's) moods, and detected methodological contaminants in some research (Parlee, 1994). Thus, heightened attention promoted the idea of premenstrual syndrome as a reasonable explanation of women's experiences.

Language

Psychologists take their scientific language very seriously. Cleaving precise statements about the human world and sculpting operative terms with impressive skill, they have

achieved a rigorous regulation of language, producing a manual of style that surpasses other disciplines in its comprehensiveness (Bazerman, 1988; Budge & Katz, 1995). The language of psychology is taken to be referential; that is, words are taken to refer more or less accurately to some aspect of reality or to denote some natural entity or event. While serving important epistemic and methodological commitments, this view of language—with “its virtual obliteration of personal opinion stated as such” (Brown, 1991, p. 135)—also under-appreciates the genealogies of the words, narratives, and rhetoric. Philosophers and linguists have long argued that there exists no neutral, acontextual description of persons or actions and proposed, instead, that statements, however assiduously constructed, acquire their meaning within the context of their appearances. They have demonstrated how conventions of terminology, style, and argumentation bring with them a rich linguistic and practical legacy: Language conventions are footprints of a usage journey that more often than not travels back and forth from everyday discourse to laboratory talk. By continuing to subscribe to a referential theory of language—and by aiming thereby toward transcending the vernacular—social psychologists (much like other social scientists) “tend to overlook the inherently rhetorical nature of the topics which they investigate as well as the rhetorical nature of the enterprise of social psychology itself” (Billig, 1990, p. 49). For example, while emotion research foregrounds the body, it nevertheless has relied on vernacular terms and storied tellings of emotion; as Theodore Sarbin argues, “Common emotion terms, such as anger, love, grief, and jealousy, are names for cultural narratives rather than bodily perturbations” (Sarbin, 1998, p. 302).

The names given to psychological phenomena and the narrative accounts through which knowledge is relayed, therefore, carry meanings beside or beyond proximal and technical ones. Historians of the social sciences have paid more attention to language as it became apparent how the “discourse of science and enlightenment were the principal medium in which social scientists formed their purposes, and in which they represented those purposes as true and legitimate” (Ross, 2003, p. 207). The “linguistic turn” taken by many working in the human and social sciences (but not psychology) prompted attention to the work that words actually do and the essential yet flexible ways that narratives are deployed to structure explanations (White, 1973). Heeding the linguistic turn, some historians excavate the language of social psychology, and their investigations illumine the field’s uses of metaphor, narrative, and rhetoric.

Metaphor

Perhaps no literary device in science has been subject to more inspection than that of metaphor. Interrogations undertaken by scientists themselves as well as philosophers, linguists, and historians suggest “that metaphor is not only a form of *speech* but more fundamentally a form of *thought*, having basic epistemological functions” (Leary, 1990, p. 1; emphasis in original).

Metaphors are ubiquitous in all the sciences, and the more potent ones form the bases of our conceptual vision, inviting us to comprehend something in terms of (qualities of) something else, to recognize some previously unseen yet revelatory similarity between the two things. Metaphors are at once cognitive constructions and powerful devices of communication: “By defining a particular object metaphorically we arouse certain expectations, focus attention on certain features, and thereby indicate certain priorities for practical action” (Danziger, 1990b, p. 351).

Theories of mental life have significantly relied upon extended metaphors. Nineteenth-century psychiatrists, neurologists, and psychologists drew on metaphors of energy from physics to describe mental functions; they eventually took the mind to be a system of nervous energy, which then would produce mental dysfunctions such as neurasthenia and neurosis if disrupted or interrupted. The idea of mental energy with its limits and discharges remains, framing everyday notions of stress and fatigue as well as scientific models of cognition.

Examining psychology’s language of affect, James Averill (1990) identified five major metaphors in modern research on emotion, i.e., theories of emotions drawn on metaphors of physiological process and responses; animal nature; driving forces or vital energies; diseases of the mind; and inner experiences. Each of these metaphors calls attention to particular features of psychic or social life; each disregards or denies other features. Locating these central metaphors provided the basis for imagining a sixth—emotions as social roles—that Averill developed as one that more accurately registers emotion’s cultural and moral functions. While the other prevailing metaphors take emotions as inward and internal, the “role” metaphor admits the sociality of emotions, highlighting “the role that emotions play within the social system, as well as any biological and psychological functions they might have” (p. 127). Although not a sole means of linguistic analysis, historical studies can complement or elucidate studies of contemporary language analysis and vice versa, as is the case with the language of emotions adopted in psychology. Historian Otneil Dror’s (1998) examination of late 19th-century laboratory experiments on emotion documents how emotion came to be taken as internal, physiological (determined) states. He found that those empirical studies of emotion were undertaken with the aim of creating laboratory and clinical conditions in which the subject is not in an emotional state and hence could serve as a superior object for experimentation or patient for physical examination. Rendering emotions as bodily states greatly advanced their measurement and strengthened belief in the laboratory as a controlled space for more objective observations. Dror’s history of laboratory work on emotion thus helps explain the specific metaphors that Averill identified in contemporary emotion research.

The metaphor of theater that underlies role theory and dramaturgical perspectives has extensively shaped social psychology’s understanding of social interaction. The theatrical or dramaturgic metaphor notably circumscribes what can be understood about the social self and social action, particularly

through an implicit assumption that scripts impose limits on persons' roles and, therefore, on their behavior. Assuming that behavior follows scripts and transpires on a managed stage eclipses certain aspects of the social self, notably the vicissitudes of self-reflection, innovative behaviors, and intersubjective relations. The limits imposed by the theater metaphor, argues Richard Walsh-Bowers, signal a need to rethink the social self and social roles as more fluid, fundamentally intersubjective, and involving "the social actor's experience of reflective consciousness, the development of selves within interpersonal relations, and the person's executive function or choice-making and self-monitoring capacities" (2006, p. 682).

Psychology's metaphors and analogies have often been drawn from natural science, engineering, and mathematics. These scientific domains are rich in imagery that imparts tacit certainty, validity, and precision to the less esteemed sciences of mental life (Brown, 1991). The metaphoric use of statistics to model mental processes illustrates such importations of imagery as well as technique. Only after the institution of inferential statistics were statistics taken seriously as an analogy of cognition, culminating in the idea of mind as intuitive statistician (Gigerenzer, 1991, 1996). The usage of a technical tool to fashion new theory shows how what has been called the "logic of justification" is not independent from the "logic of discovery"; it indicates how metaphors are importantly connected with creative development of theory. The statistics metaphor of mind has generated a massive body of psychological research, significantly shaping the architecture of social psychology. Gerd Gigerenzer named this metaphor case a "tools-to-theories heuristic" because it originates with researchers' practical utilization of statistics to resolve perceived problems of their own subjective judgments about experimental outcomes (judgments about hypothesis testing). By excavating the analogy's origins, Gigerenzer located its base assumptions, including its tacit premise that there is a singular statistics and only one interpretation of probability.

Another case where research tools come to metaphorically transform researchers' understanding of a phenomenon is the use of game theory to better understand the strategies of war. Introduced during the postwar era, game theory was especially appealing for its emphasis on rationality and mathematical formulations. As economic historian Mary Morgan (2007) found, this investigative tool soon transformed thinking about war: "Whereas game theory initially provided a mathematically formulated theory of rational action in certain situations that might be *applied* to the Cold War world, the process of using game theory to think about that world turned the relationship around: the Cold War came to be seen as a set of game situations" (p. 159; emphasis in original). Researchers in the 1950s and 1960s did not simply advance game theoretic models and methods, they also played the games such that "game theory was associated not so much with the civilization of war as the reduction of war studies to the playroom" (p. 160).

As these studies suggest, the power of metaphors and analogies extends well beyond that of literary device and can

engender literal descriptions of the social world. They foster not only novel ideas about psychological objects but also new features of those objects, even new entities. They kindle innovations and "in engineering such creations, psychology creates new capacities for itself—new ways of thinking, experiencing and behaving" (Richards, 2002, p. 20). Semantic innovations, then, eventually come to be taken as expressions of human nature, even ushering transformations of the object (Maasen, 1995).

Rhetoric

Although "rhetoric" is sometimes used to imply empty or florid language, the term also refers to the discipline, art, and methods of persuasive communication. Studies of rhetoric examine the production and use of argumentative or persuasive discourse, aiming to decipher discourse in the context of its enunciation. They address both past and contemporary social psychology, affording a broad perspective on its linguistic forms and inviting comparisons of those forms across time. Investigating the rhetorical strategies in recent social psychology, Michael Billig isolated claims about "common sense," comparing social psychologists' aims to understand and transcend (the confusions of) common sense of ordinary actors with their own internal, scientific arguments. He discovered that "the uncommon sense of social psychology resembles the more ordinary common sense, which it seeks to replace" (1990, p. 59). Both versions of common sense, social psychologists' and common persons', share similar structures of argumentation, vetting of conflicting claims, confusions in communication, and residual counterclaims.

Textbooks incorporate persuasive means to distinguish the discipline's knowledge claims from common knowledge, but early introductory psychology textbooks faced yet another challenge: how to persuade readers that they were distinct from the rather faulty human beings textually depicted as nonrational or moved by forces beyond their own volition. In other words, authors had to address the very subjects whose own experiences were being reinterpreted, sometimes radically, by the science. To persuade readers and establish an alliance with them, authors deployed rhetorical strategies to signal to readers that they (the readers) stood apart from ordinary beings, or that they could stand apart once they acquired the psychologists' perspective (Morawski, 1992, 1996, 1997). Their strategy resulted in a dual representation of persons. The texts contain a double discourse "of persons as at once organic forms and social entities. Just as they describe the determined, biomechanical body, so they present readers as social agents who could will and ultimately determine the direction of their lives" (Morawski, 1997, pp. 220–221). Social psychology textbook authors faced the same dilemma, only heightened by a greater need to secure the field's scientific legitimacy.

Textbooks provide exceptionally rich material for discursive and rhetorical analysis. Crafted without the stringent oversight of journal editors, and written for learners rather than professionals, these texts are more open, permitting their authors

latitude in claim making and justification. Clare MacMartin and Andrew Winston (2000) utilized discourse analysis to identify forms of talk about experimentation in social psychology texts published between 1930 and 1960. In their and related studies, language statements are understandable as forms of action. Following J. L. Austin's theory of performative speech acts, language can be seen as constitutive of practice rather than simply representational. Accordingly, MacMartin and Winston examined discussions of experimentation "in terms of the grammatical and rhetorical features with which it was constructed, variations in the way in which it was constructed and used within and across texts, and the possible functions of the way in which it was deployed" (p. 352). Comparison of Gardner Murphy and Lois Murphy's and Kurt Lewin's writings on research reveals how the latter enthusiastically and persuasively promoted the experiment as the central method in the discipline. Among the rhetorical tactics, Lewin foregrounded himself as exemplar; assiduously avoided examining grand assertions made about experimentation; invoked feelings of a social psychological community; and offered tantalizing analogies between the objects of social psychology and those of the physical sciences. Lewin's selective rhetoric compels a specific science, thereby providing substantively more directive than mere description or representation of actual scientific practices (see also Danziger, 1993; Korn, 1997).

Research methods textbooks have also played an important pedagogical role in promoting experimentation. A series of historical studies have found them to contain a justificatory rhetoric along with defensive arguments seeming to anticipate criticisms. In the 1950s, textbook authors began to actively defend experimentation and denigrate other conceptions of social psychology research (Stam, Radtke, & Lubek, 2000). They proclaimed a monolithic, systematic method in a field in which there existed at the time no certainty or consensus about either the objects of study or appropriate methods of inquiry. Not surprisingly, these accounts contain inconsistencies and what Stam et al. call "rhetorical strains." Prominent are tensions between the idea of rational experimenter and intuitive scientist; between insecurities about the experiment as ideal method and the exoneration of that method; between the nature of subjects as passively submissive and as subversively threatening; and between a fun-and-games attitude toward experimentation and a sober, scientific one. In another historical project, Stam, Lubek, and Radke (1998) examined the specific language used to distinguish subjects (as docile, cooperative, and machine-like) and the experimenter (conscious, ethics, and intuitive). Identification of rhetorical tropes like these depictions of subject and experimenter exposes inconsistencies in scientific practice that, in turn, invites further investigation into why such inconsistencies or contradictions arose and whether similar ones persist in current research practices. In this case, the mixed portrayals of subject and experimenter complicated explanations of subjects' behavior in experiments (Morawski, 2010) and decisions about what constituted ethical research (Stark, 2010). Additionally, they contrast with the

"automated, cookbook approach to research" that was being promoted in methods texts and chapters (Stam et al., 1998, p. 159).

One function of rhetorical slippage is particularly discernible in apparently incompatible prototypes of the social psychologist. One prototype represents a "scientific experimenter" who dutifully adheres to the logic and rules of method while the other depicts a fun-and-games researcher whose "ludicro-experimental" approach involves theatrical, creative, and high-impact techniques (Lubek & Stam, 1995). Despite the textual inconsistency, engaging *both* types of the scientist permitted representation of social psychology as a rigorous science *and* the seductive proselytizing of its performances. Championed in many methods texts, the ludicro-experimental icon eventually faced criticism, but for a period it served to promote the discipline and expand its investigative purview.

Narratives

Considerable scholarship has explored the narratives scientists have used to represent the world, sometimes producing provocative theses about the nature of science itself. One such thesis argues that experimental activities exceed any singular historical narrativity, and that "an experimental system has *more stories* to tell than the experimenter at a given moment is trying to tell with it" (Rheinberger, 1999, p. 425; emphasis in original). Another proposition suggests how narratives do not merely describe but also organize scientific thinking, specifically how narratives structure scientific actions, decisions, representations, and the science's official histories (their retrospective accounts of scientific advance). Whereas it was once assumed that narratives are compelled by the facts (whether the facts are historical, social, or scientific), they are now understood to be more flexible: Narratives entail selection, ordering, and interpretation as well as deletion and omission (White, 1973). Narrative analysis yields insights into writers' interests and commitments as well as their disavowals and aporias. Like empirical evidence, narratives do crucial work in forging explanations, dismissing alternative hypotheses, and delimiting the empirical field (Haraway, 1991).

Social psychology's self-produced narrative histories, the "official" histories, have received much attention. These histories are found to be not only laudatory and authoritative, but also mythic: They feature "origin myths" that claim precursors and founding events with exceptional partiality (Harris, 1979; Lubek & Apfelbaum, 2000; Samelson, 1974). More recently these mythic stories have been augmented with or supplanted by "production statistics" about the field whereby quantification tells the "story" of social psychology (Samelson, 2000). In order to craft a linear and progressive story, official histories omit much, including contestations over the very conception of the "social," methodological problems, political interests, and abandoned theories. Crafted as stories of success, for example, official histories fail to acknowledge that some of the field's heroes, including Floyd Allport and Leon Festinger, "later in

life found their old approaches wanting and forswore them totally, at the same time that novices in the field were being taught to follow in the old (abandoned) footsteps” (Samelson, 2000, p. 505). While truncated and partial, official chronicles of social psychology nevertheless often gesture toward counterstories or larger forces behind that life. Thus, the more prominent or “classic” histories, from F. B. Karpf’s 1932 monographic history to E. E. Jones’ 1998 *Handbook of Social Psychology* chapter, acknowledge social psychology’s organic connectedness with turbulent social conditions—crises, catastrophes, dysfunctions, tensions—whether these be the Civil War’s disruption of the social world or the post-Second World War global struggles and intergroup conflicts (Morawski & Bayer, 2003). Yet these accounts stop short of examining the substantive implications of the field’s cultural beginnings and do not investigate the political and economic interests that have substantially motivated the field. Had they attended to political and social conditions, a quite different narrative would have ensued. For instance, canonical histories often cite the influence of the Second World War and the Cold War that immediately followed, yet they do not ask how these events nurtured researchers’ preoccupations with defense, security, conformity, and trust and, more importantly, how these concerns found their way into social psychology laboratories. Nor do they consider how this culture prompted the abovementioned turn toward cognitive conceptions of creative, flexible, and rational human actors and away from dominant behavioral notions of passive, mechanistic ones. They similarly overlook economic factors, including how funding agencies encouraged defense-related research and certain scientific methods.

With an aspiration to compose a coherent, singular and linear account, canonical histories underplay contradictions and controversies (such as the “crisis in social psychology”), and omit alternative theories and perspectives (such as narrative psychology, feminist research, and non-experimental methods). Conventional accounts stop short of presenting the full story, for instance, of the immigrant social psychologists (such as Lewin and Asch) whose later “mainstream” researches are championed. Canonical chronicles likewise tend to erase family discords and errant relatives with the effect of constricting the social world that actually made social psychology as it is.

As one form of contextual inquiry, studies of social psychology’s language expose these ways that words, figures of speech and narrative structures meaningfully shape how objects come to be known. They illuminate researchers’ ambitions, ambivalences, and conceptions of themselves and the social world—motives and affects that fueled particular ventures. In so doing, they suggest the import of scientific language and offer means for considering the roots and implications of current linguistic practices. Attention to language serves not only historical insights but also contemporary work; for instance, showing how contemporary research on stereotypes and attitudes adopts the language (and deep concepts) of virtual reality with its aspirational ideal types, computer technology with its automaticity, and the economy with its equivalence of exchanges.

A note of caution is warranted here: Examinations of language do not presume that social psychology and its objects are thereby linguistic constructions—that they are simply worlds made up with words. On the contrary, language analysis can appreciate the real as it is discursively given form and meaning and how, in turn, semantic forms are constitutive of that real. Critically appraising rhetorical conventions, tensions, conflicts and elisions, for instance, permits better seeing how “rationality” and “irrationality” came to be affixed to certain forms of decision making and decision makers (Lopes, 1991). Examining these terms, Lopes discovered that prior to the 1970s, psychological and social psychological research recorded fairly competent decision-making among experimental subjects whereas later research found nonrational, even befuddled decision-making. The bias and heuristics literature arising in the 1980s abounded with clever and engaging experimental designs, intimations of researchers’ superior judgment, and ambiguous slippage from description of “heuristics” to that of “bias.” That research also incorporated a repetitive, evaluative language regarding what is “rational” and what “irrational.” Not surprisingly, the popular press echoed researchers’ descriptions of subjects’ judgments as “ludicrous,” “indefensible,” and “self-defeating” (Lopes quoting Tversky and Kahneman, p. 80). The reality of that irrationality, Lopes concluded, “is mostly in the rhetoric” (p. 80).

Histories of language use show how categories such as “attitude” were revised and refined over the past century to foster certain assumptions about morality, the relation of thoughts to the real, and the irrelevance of ideology (Danziger, 1997). Although initially described as observable phenomena, attitudes eventually came to be understood as internal, dispositional, and not directly observable. Through this transformation attitudes came to be defined as hypothetical internal states that the subjects themselves could only partially and inadequately report. The term came to refer to social, albeit strictly individual, attributes; to “an actually existing state of affairs inside the individual.” Discarding a conception of attitudes as an observable physical stance or as a conscious awareness, social psychology bestowed upon attitude “a whole metaphysics of unobservable but nevertheless real and distinct entities that push and direct the person from within” (Danziger, 1997, p. 145). This example of changing discourses acknowledges the reality of social psychology’s entities yet demonstrates how “psychological properties are intelligible features of the world only by virtue of their display within a discursive context” (p. 190). That is, entities are real (and lively) yet cannot be simply and directly observed as such; they are registered in language, including quantitative language. Words and discourse generally are pointing fingers that contribute to more accurate genealogies of our scientific practices. Language analyses thus serve contemporary researchers as well, providing background and techniques for examining how our scientific language functions and what world assumptions it brings along and conveys via empirical research. Beyond its presumed representational function, scientific language does do work, and recent language

studies advance a larger project to better understand scientific practices beyond abstract philosophical notions of them.

Practices

Historians and science studies scholars are exploring questions about *how* science happens, augmenting studies of *what* happens in science. In lieu of documenting scientific achievements and presuming certain epistemological precepts, they ask, *how* is science produced? The question is direct, yet the answers are proving to be complex and multiple. Seeing science as practice impels fuller appreciation of the myriad tools, actors, social relations, resources, languages, and material conditions through which knowledge is imagined, generated, validated, and shared. (Many of the studies of scientific language and reflexivity, although treated separately in this chapter, fit within the purview of scientific practices.) The move from asking the *why* to the *how* of science shifts analytic emphasis from theory to method, foregrounding instruments, experiments, cognitive styles or personas, marketing, and social exchanges. For some scholars, understanding scientific practice ultimately entails a “decentering” of the human subject (Pickering, 1995), acknowledging plural realities (Law, 2004), or mapping affinities among actors and actants or networks (Latour, 2005). Investigations of practice sometimes trouble cherished tenets finding, for example, that against beliefs that science is unified and orderly, it can be disunified (Galison, 1996). Studies of scientific practice are especially promising for social psychology with its complex and rich connections with private as well as social life.

Attending to scientific practices need presume neither debunking nor celebratory ambitions. As philosopher Joseph Rouse proposes, “critical engagement with scientific practices might variously articulate and reinforce dominant epistemic and political alignments, contribute to or extend oppositional discourses and practices, or shift the field to envision new possibilities” (1999, p. 453). Engaging scientific practices typically entails bracketing epistemology and its dualisms (theory and methods, objects and representations of objects, human and nonhuman agents) in order to investigate how these cognitive tools are produced. In so doing, these projects assess science in the making—the techniques, skills, reasoning styles, and professional cultures of scientists as well as the material conditions (machines, bodies, spaces, objects and economics).

Just as these investigations yield insights into what procedures, machines, reasoning styles, space, economics, objects, and interactions make science, so they cast light upon what is backgrounded or unexplicated. Analyses of practice can bring to the fore tacit moral and political impulses accompanying technical choices. The history of statistics in the social sciences reveals how their wholesale adoption by researchers was tied to the rise of a democratic ethos of fairness, the replacement of an elite group of decision-makers with experts, and a culture of suspiciousness about truth claims (Porter, 1986, 1994, 1995). Statistics thereby promised “truths” that transcended those based on privilege, tradition, character, or brute political power.

As evidenced with statistics, political alliances and social imperatives influenced seemingly unlikely projects of instrument making, editorial duties, national crisis, or recording devices. In other words, even presumed stable epistemic matters such as quantification convey political and moral commitments and change over time: They emerge, grow, and sometimes pass away. “Objectivity” too is illustrative of this liveliness of epistemology: Its appearance and alterations over the past two centuries are closely tied to changes in conceptions of human subjectivity, machine technologies, gender, and professional struggles (Bernstein, 1983; Bordo, 1987; Daston and Galison, 2007; Keller, 1985, 1994; Porter, 1994).

Attention to investigative practices ultimately exposes the extensiveness of science, revealing how knowledge—its inception, execution, and reception—involves commerce, personal lives, governments, economics, technologies, material cultures, and social arrangements. Such studies chart the vibrancy of scientific practices: the back and forth relays of information, materials, techniques, and customs between what demarcated as “science” and “culture.” These capacious relays can be productive of new events and objects, generating novel events and entities and indicating that scientific objects can change. For instance, shifts in modern economics are fully explicable only by considering its tools, theories, changes in the economic world, and the interaction between these: Modern economics influences the economic world as it lends models and instruments to explain it. Taking account of these extended practices explains how “the economic science of the 20th century has, by means of its engineering interventions in the economy, engendered new economic ‘events,’ to be reckoned with by new generations of economists” (Morgan, 2003, p. 305). Similarly, the classification of multiple personality changed over the 20th century in response to new evidence, imported assessment techniques and theories, and changes in the phenomenon itself—in those individuals classified as such (Hacking, 1995b). Histories of scientific objects thus can reveal them to be real and historical, weighty with biographical detail, and traveling across delineated boundaries of science and culture.

There are several distinctive features of social psychology that make the field at once an enticing and also a challenging candidate for study of scientific practices. First, the discipline’s intimate relatedness to social life—its direct relevance to nearly all social interaction—and its long championed utility for social policy signals its extensive connections outside the discipline (Good, 2000). Perceiving the extended and extensive circuits prepares us to ask, what exchanges are made between the laboratory and the street? How do trades of resources and ideas transpire? With what consequences? Apprehending this circuitry likewise invites asking how might social psychology alter its scientific objects? How might persons act or think differently upon learning of social psychology’s discoveries or living in environments designed according to them? How, for instance, are people changed by knowledge of bystander intervention dynamics or heuristic bias?

The second feature of social psychology that distinguishes its practices is its methods, specifically the harnessing of methods of its master discipline (psychology) to study social, exterior actions and things. In other words, modern social psychology has adopted and adapted methods fabricated for studying interior states and deploying them to investigate what might be very different sorts of worldly phenomena—social ones. How was this transposition of intrapsychic methods onto external social affairs effected? With what consequences? How has what we take to be “social” changed through these investigative transpositions? What, if any, aspects of the social world have consequently been laid aside? Social psychology’s distinctive practices thus are most evident in two arenas: its methods and objects of inquiry. Examining the field’s methods exposes some of the ways that everyday sociality was translated to render it amenable to precision research techniques. And close analysis of the objects of research uncovers overlooked moral and political premises that accompany empirical work.

Methods

The social psychology attached to (and located within) psychology has substantively utilized the principal methods of psychology, consequently privileging experimental techniques, quantification, and individual-centered procedures (despite the longstanding utilization of other methodologies). Curricula and textbooks’ nearly singular concentration on experimentation omits discussing that method’s philosophical and epistemological presuppositions. Yet the design and implementation of research methods are far more than an empirical matter. They involve extensive negotiations among researchers that are often guided by ideals of efficiency, expediency, and aesthetics as well as epistemic ones. Sometimes these negotiations are internal to the science, as is the case with the invention of “variables.” The history of “variables”—the coming into being of a uniform, mandatory technique in experimentation—shows how this methodological entity was crucial to the making of consensus among researchers. Identifying a laboratory entity or event as a “variable” greatly reduced observational variability. It also invoked a sense of observer neutrality and promoted an engineering attitude through which research was more readily translatable into social life outside the lab (Danziger, 1997; Winston, 1988, 1990, 2004; Winston & Blais, 1996). The emergence of variables occurred over a short period of time and entailed transfiguring 19th-century terminology and operations of statistics into signifiers of psychological constructs. Disbanding common talk of stimulus and response for variables began in the 1930s, and by the 1950s “variable” came to be used “to refer to anything that was the object of psychological investigation” (Danziger, 1997, p. 167). Events happening within the organism (initially described as “intervening variables”) thus could become part of observable reality. Introduction of variables sidelined matters of subjective meaning: As variables shifted from being a statistical concept to a natural, psychological force, researchers could more readily

translate common cultural understandings into precise, measurable ones; cultural importations, then, acquired scientific life. The birth of variables, adopted from statistics, enabled techniques that, in turn, could designate the reality of psychological phenomena. Winston and Blais (1996) note a certain irony in psychology’s adoption of variables as a scientific advance when, in fact, that technique departed from the scientific practices of the physical sciences.

Research methods have rarely been architected by scientists alone and often involve elaborate and extended negotiations with others, including public and private funding agencies, departmental colleagues, subjects, university officials, and interested third parties (educators, clinicians, government agencies, companies). Much can be learned about the design of tests and scales from archival evidence of the extensive negotiations between researchers and the military (Carson, 1993). Psychologists charged with creating a uniform intelligence test for First World War army recruits worked intensively with members of the military, heeding their conceptions of the desired soldier, ideal personal characteristics, and test validity. Similarly, the genealogy of survey research described above necessitated important, ongoing interactions between researchers and the public—exchanges that substantively shaped subsequent survey development as well as interpretation of findings (Igo, 2007). Even methods conceived and refined in laboratories have larger histories where other individuals outside the laboratory influenced the form and use of methods. And sometimes experimental techniques are moved outside the laboratory; the application of B. F. Skinner’s behavior analysis to “real world” situations attests to the transportability of laboratory technologies to everyday settings (Rutherford, 2009). While the psychology research community increasingly disregarded the methods and findings of radical behaviorism, its techniques were borrowed, re-engineered, and sometimes flourished in hospitals, clinics, and schools. Today, variants of behavior change techniques continue to be used in practical settings.

Connections between scientific methods and the world outside the laboratory are vibrantly evidenced in two studies of a classic social psychology experiment. Stanley Milgram’s experiments on obedience have been subject to a plethora of analyses, reflections, and judgments: Along with biographies and ethical reprises, they have been singled out to illustrate counterintuitive findings and the social impact of research. With assiduous attention to the cultural meanings circulating through Milgram’s project, however, much more can be learned. Anna McCarthy’s (2005) examination of Milgram’s affinity to early reality television documents his prescience about virtual reality as well as his vital interest in reforming citizenship. Pursuing commonalities between Milgram and reality television innovator Allen Funt, McCarthy found that they shared an investment in shaping a model citizen. For both men, “covertly filmed behavior was a tool for teaching responsible citizenship on multiple scales, from the interpersonal to the institutional to the national” (2005, p. 23). Both Funt’s and Milgram’s staged

accomplishments required extensive theatricality, a dramatic enactment or simulation of real life that was hailed as realism. Rooted in these projects were tensions between what was fictional and what was real, and that fictional/realist equivocalism still poses “persistent dilemmas” for social scientists and critics alike. Milgram himself appreciated (both directly and indirectly) the ideological affinities between real and fictional, sometimes blurring distinctions between them. He cowrote a paper promoting *Candid Camera* as social science, and while undertaking his own film project in 1965, he described his work as “an amalgam of science and art” (quoted in McCarthy, p. 32). It is not surprising, then, that Philip Zimbardo identified Funt, Milgram, and himself as intellectual grandsons of Kurt Lewin.

Seeing these connections, especially in a classic experiment, compels us to ask how researchers distinguish reality from pretense of reality, from actings and maskings? How do we come to decide what about the social stuff of everyday life—or laboratory life—is real? Common confusions between artifice and real, performance and authenticity, thus raise a central epistemic question: What about the social is “real”? And how do we know this real of the social? Apprehending how epistemic decisions are intricately tied to commercial culture and political ideals opens the way for potentially insightful, if complex, reflection on how these decisions are made in contemporary research. Are the recent virtual reality replications of Milgram’s experiments as real as Milgram’s experiment? Are the simulations of his experiment? And are any of these as real as everyday obedience? McCarthy’s study thus locates a scientific premise that social psychologists should not leave untouched if research is to accurately inform us about the social world. These questions are not only historical ones but remain concerns of contemporary research; in fact, they exemplify how historical reflection can provide background lighting for better seeing persistent if sometimes latent scientific problems in current research.

Ian Nicholson (in press), too, examines Milgram’s project in terms of cultural atmosphere of the era, namely the postwar thinking about masculinity. His detailed analysis of the experimental design, confederates, actors, subjects, and experimenter’s self-regard reveals the circulation of anxiety-inflected representations of masculinity. In those documents he found that Milgram’s experiments and their sensationalized reception signal postwar worries about the deterioration of manhood, disappearance of the inner-directed man, and arrival of the compliant organizational man. These worries were taken to be signs of larger changes, namely male emasculation and surrender to authority. Milgram’s studies addressed (and his results further indicated) these very concerns about masculinity by creating an experimental version of the broader cultural circumstance—fears of the fragility of human character. The research program’s central and dramatic representation of masculinity is further evidenced in Milgram’s persona. Nicholson drew upon archival materials and published accounts of Milgram’s behavior and personal style that describe him as

often harsh and interpersonally detached—fitting the cultural idea of masculinity in that period. Additional evidence of the masculine atmosphere of the project is found in the experimental setting fully furnished with machinery, shock, and danger. In its rudimentary theatric form, the experimental situation was gendered through featuring “a man using technology to do violence to another man at the behest of a ‘scientific’ male authority” (p. 22). Even the ethical criticisms of Milgram’s project contain a gendered subtext. Additionally, Milgram heeded masculine attributes in selecting confederates, and seemed uninterested in female performances. Of the 1,000 subjects who participated in the experiments, 960 were male; men alone served in 17 of the 18 experimental conditions; and only in a 1974 publication did Milgram comment on the female participants. He never explained their near absence among the ranks of experimenters, confederates, and subjects. His analysis informed by recent work on what previously stood as an unmarked category, “masculinity,” Nicholson detected how masculinity, as a stereotype and a social way of being, actually permeated a scientific project. Here again historical investigations excavate and bring to the fore practices and premises that are not readily observed or examined in otherwise precise scientific work. Common or tacit ways of being, like masculine ideals in the postwar era, transpire within research projects; historical inquiry makes them visible to us. A bevy of analysts of social psychology’s most renowned experiment failed to observe this significant feature (an exception is Stam et al., 1998).

These two studies of Milgram’s classic experiment find a place among contemporary histories of science that aim to “understand science of the past in its own terms, reconstructing the reasonableness if not timeless rationality of the arguments on all sides” (Daston, 2009, p. 802). McCarthy and Nicholson’s studies demonstrate such relations, and they perceptibly accord with philosopher of science Joseph Rouse’s observation that “Laboratory practices guide a massive continuing effort to reconstruct the world in the image of the laboratory” (2006, p. 11).

Social psychology’s enthusiastic adoption of experimental techniques introduced major challenges to isolate, define, and measure “social” phenomena without altering those often elusive entities. And the multifold transportation of psychology’s experimental techniques to the study of social phenomena testifies to the field’s ingenuity in doing so. “Debriefing,” “confederates,” and “deception” (all techniques refined and commonly employed by social psychologists) intimate the challenges of applying scientific techniques designed for studying intrapsychic, individual events to investigate social ones. One important technique invented to make the psychology laboratory more amenable to investigating the social is the confederate. The role of the confederate, once called “phantom” or “stooge,” relies on pre-existing social types such as the confidence man or the earlier manifestation, the trickster. Yet at the same time these individuals introduced into the laboratory a modern, technological equivalent of ghosts. The confederate is at once a named

social actor and also a nonsocial entity, taken to be a stimulus relaying information and whose nonrecognized presence (by the subjects) takes scientific precedence (by the experimenter). Betty Bayer's (1998) comprehensive historical survey located an array of social-psychology phantoms that have taken nominal forms of confederates, stooges, paid participants, bogus pipelines, false feedback, accomplices, cover stories, cyanoids, and the like. These phantoms "denaturalize the 'nature' of scientific relations and investigative practices" thereby representing subjectivity as human information processing (not any social materiality or relationships) and endowing mechanization with phantasmic (invisible) powers (Bayer, 1998, p. 189). Vested with this special subjectivity *and* scientific essence, "Phantoms make seeable not only how scientific discourse constructs the boundaries between subjectivity and objectivity, but also how scientific discourse trades guise and disguise off one another to bring these very entities into being" (p. 195). At once made visible and invisible, phantoms help realize new understandings of persons in which internal or information processing can become foregrounded and the concrete social relations of laboratory actions are discounted or elided.

Debriefing practices have a similarly rich life involving the trade of cultural practices and scientific ones. Ben Harris' (1988) detailed chronicle of "debriefing" found the word's origins in military warfare practices: It has a paradoxical history in "the adoption of a military metaphor for an ethical *desideratum*" (p. 189). The practice was variously reconfigured over the course of experimental use, ambiguously changing who was to be debriefed and who was debriefing. Throughout these shifts, debriefing practices apparently served to satisfy two research problems: one ethical (to achieve honesty in research) and the other methodological (to reduce contamination of experiments). Perhaps as significant, however, was how its technical, bureaucratic origins bestowed added legitimacy on social psychologists' experimental roles and authority.

Deception is another investigative tool important to social psychologists who, like other psychologists, sought to guarantee subjects' naiveté during experimentation but who also needed means to recreate experimental conditions as similar to the social world as possible. James Korn's (1997) expansive history shows how deception techniques were formulated and legitimated on cultural as well as academic and scientific grounds. Attaining the desired experimental realism involved creating "illusions of reality" that would engage subjects and persuade colleagues of the validity of experiments. Deception importantly served the goal of realism yet it was made possible by more than scientific precepts of control and realism: Longstanding cultural ideas inflected these understandings. The acceptability of deception depended on individualist values with its logic "that it was all right to break the law because, as individuals, they concluded that their action would produce more benefit than harm" and those others would concur because of the positive outcomes (p. 163). Deceptive practices were enabled, too, by a cultural conception of experts as beneficent along with experts' paternalist "view that sometimes people

should be helped even if they don't ask for it because what they are made to do is for their own good and is something they later will appreciate" (p. 163).

Histories of social psychology's practices, distinctive in their multivalent connection with the world outside the laboratory and resolute honing of scientific definitions of ordinary social phenomena, are in an inaugural stage. But we already have enticing prolegomena to guide the historical work ahead. More needs to be learned about social psychology's manifest focus on methods and its relative abeyance of theory (Kruglanski, 2001) including theories of the "social" (Stam, 2006); the transport of folk understandings to laboratory entities (Graumann, 1993); the colonization of North American models (Van Strien, 1996); and the variegated yet largely unacknowledged accomplishments of applied social psychology (Hill, 2006). While the project is young, extant histories afford a better understanding of the field's objects of analysis and the political currents flowing through its investigations.

Objects and their politics

Social psychology's origins and subsequent missions are marked by a double play of political and moral commitments. Although these commitments are perceived to lie outside scientific work (serving only as aspirations or consequences), they actually impress the entire project. From its inception, the discipline has promoted itself through ameliorative (if not Utopian) aspirations. Researchers have routinely asserted that their field's knowledge would guide projects to realize social adjustment, justice, and remediation of social problems generally (Finison, 1976, 1986). Echoed in its canonical histories, social psychology has claimed a notable place among 20th-century social sciences as a discipline especially equipped to make knowledge that benefits human welfare. And the field has retained its position within psychology as a leading arena for producing socially beneficial knowledge. Thus, social psychology's political and moral aim is its foundational and ongoing desire to produce scientifically validated means for realizing social order, democratic justice, sound social relations, and cooperation.

A second political and moral axis inheres in prime object of study, in the very conception of the "social." The North American social psychology that flourished within psychology departments meticulously if perhaps unselfconsciously aligned its definition of the social with (psychology's) assumptions about the individual as existing prior to and apart from the social. Adoption of this individualist perspective had several implications. First, it required abandoning conceptions that diverged, including conceptions of the self as socially constituted (e.g., G. H. Mead) and of social interaction as emergent, potentially transformative phenomena. Social psychology committed itself to understanding an essentially nonsocial individual as she thought and behaved in social situations such as work, intimate relationships, and public actions. This ontological presupposition—that the individual exists as an independent entity constituted prior to or outside any historical

and social influences—is both moral and political. That is, as an autonomous agent, the individual ultimately is (morally as well as causally) responsible for navigating, interpreting, and responding to social conditions. This presupposition is premised and dependent on democratic and economic ideas of the liberal citizen that were articulated in the 18th and 19th centuries (Smith, 1997). This vision of the individual (and his relations to the social world) has substantive implications for *what* is studied about social processes. And the field's concordance with psychology's autonomous individual, in turn, continues to compel assimilation of psychology's current models; thus has social psychology readily adopted cognitive perspectives after the fashion and, more recently, those of neuroscience and evolutionary psychology.

Political and moral stances inflect the very objects of social psychology—its ontology of social beings. These roots engender what are taken to be objects of inquiry and relay assumptions about the intrinsic qualities of those objects. Political assumptions about the individual as an independent being in society thus undergird practices of locating the social as something that acts on the individual and consequently exploring just how social “variables” affect (through internal processes) his actions and thoughts. Appreciating such assumptive roots makes more understandable the field's tendency to study violence and aggression in terms of internal psychological processes and not economic or political ones. This tendency to focus on internal dynamics of violence is apparent in social psychologists' scientific response to Kitty Genovese's murder. To explain why individuals hesitated or failed to assist the victim, researchers undertook experiments on bystander's behavior, studies that probed several immediate, individual, and local variables as the most likely determinants of those behaviors. They identified several bystander variables rather than investigating extra-individual ones, notably community relations, gendered forms of violence, or poverty (Cherry, 1995). In selecting certain variables to study, researchers subscribed to the precepts established by psychology's experimental paradigm “by moving in as closely as possible to the behavioural phenomenon and casting the event in terms of independent variables such as size of group that affect dependent variables such as intervening behaviour” (p. 19). Other salient social variables distinguishing that environment were left unexamined. However, the paradigm (with its intra-individual foci and experimental techniques) adopted from psychology was not the sole influence on the studies. The research also was structured through emphasis on certain social actors: It concentrated on bystanders, not on victims or perpetrators. This selectivity of objects for investigation resonates with then dominant cultural understandings of the period, including an attenuated fear and mystification of urban violence, certain stereotypic (gendered and class-based) notions of victims and perpetrators, and relative blindness to gendered forms of violence.

Popular models of self-control, including classic studies of locus of control, concord with a politics of individual responsibility and self-management. They, too, have backgrounded social

and economic conditions that differently enable or constrain control for different persons (Baistow, 2000; Furby, 1979). Armed with a political and epistemic conviction that “the path is set for empiricist science to intervene with methodologies which can constrain the individual from the non-rational” (Henriques, Hollway, Urwin, Venn, & Walkerdine, 1984, p. 90), researchers have typically proceeded with the assumption that their empiricism is without politics or morals or that any political or moral origins of their work are removed from empirical research. Additionally, certainty about the purity of scientific practices has fostered general abeyance of other cultural knowledge that is imported into research and sometimes becomes essential components in the staging of experiments.

Historical studies uncover just how moral and political matters enter into and circulate through research programs. In doing so, they also signal how our scientific objects are not pre-existing forms to be discovered in controlled, observational situations. Rather, our objects come to us as both embodied *and* imagined in their emergences. What is routinely taken as the scientific discovery or observation of objects thus needs to be supplemented with better understanding of how scientific objects take shape and can change. Such inquiries illumine the “ontological fecundity of the sciences” both inside and outside designated scientific spaces (Daston, 2000). In their liveliness, scientific objects can and often do exceed the properties formally ascribed to them either through particular hypotheses or the experimental roles they are assigned.

“Socialization,” a key object in social psychology's past that retains a recognized place in scientific (and popular) discourse exemplifies an object that was neither wholly “discovered” through empirical research nor simply borrowed from ordinary language or common understandings (Morawski & St. Martin, 2011). In its 19th-century usage, socialization explicitly referred to an economic arrangement of production: It referred to a social organization out there in the world. Shortly afterwards the term came to be used to describe coordinations of the social world and individuals: It was taken to be an interactive object, described as such in the writings of sociologist Georg Simmel. (Even the 1934 *Encyclopedia of the Social Sciences* defines and attributes the term “socialization” to Karl Marx's definition as a particular social organization of industry, and notes as well Simmel's definition of socialization as a process of group formation and association.) By the second decade of the 20th century, psychologists and budding social psychologists took up the word to describe a process whereby individuals learn and internalize social conventions. This definitional turn revised Simmel's sociological model by emphasizing a dynamic relation (an ontological link) between individual consciousness and the social world. By the third decade of the 20th century, researchers forged an unlikely amalgamation of psychoanalysis and learning theory (and eventually role theory) to present socialization as something that happens within the individual as a response or accommodation to social forces. By mid-century, socialization appeared as a bricolage: a concept variously combining notions of adaptation, learning, conformity,

identification, internalization, conditioning, assimilation, unconscious drives, attitudes, personality, culture, social norms, and the very idea of “human nature.” The chapter on socialization in the 1954 *Handbook of Social Psychology* commences by stating “socialization refers to a problem which is old and pervasive in human life—the problem of how to rear children so that they will become adequate members of the society to which they belong.” The author also noted that “the use of a fairly uniform label for socialization in scientific discussion is new, and there is not even yet complete agreement on the terminology” (Child, 1954, p. 655). This was a rare caveat, for socialization already had become a lively object, taken as a valid construct in textbooks as well as empirical studies. As Theodore Newcomb instructed student readers of his 1950 social psychology textbook, socialization was a *bona fide* social psychological object showing that “in common with other members of your group, you have interiorized many social norms so that they are now part of your own psychological make-up” (p. 5). Socialization came to indicate that personhood is, in Urie Bronfenbrenner’s words, “a matter of social rather than biological inheritance” (1970, p. xxv). By this time, however, nascent cognitive psychology’s conceptions of flexible, rational actors were already reshaping conceptions of socialization, enabling Lawrence Kohlberg to propose that socialization must be understood in terms of “active processes of attention, information-gathering strategies, motivated thinking, etc.” within the person (1969, p. 349).

The evolution of socialization as a scientific object, therefore, was not driven simply by empirical findings, nor was the concept itself submitted to much empirical testing. There ensued no systemic or comparative examination of the various mechanisms attributed to the socialization process, and its definition often remained an odd admixture of what were taken as discordant theories (combining behaviorism, psychoanalysis, role theory, and more). By the postwar period socialization stood as an established object, a thing done by or happening to individuals. Researchers continued to suppose it to be an essential human capacity. Even today, few researchers and laypersons question the object’s existence, and socialization endures as a common and effective explanation of behavior (Morawski and St. Martin, 2011).

The very idea of the “social” has an even more complex history. Given the ontological, political, and practical implications of any definition of the social, historians have investigated the life of this central concept. The individualistic definition of social psychology, a definition generally attributed to Floyd Allport’s 1924 text, successfully displaced theories about the ways individuals are socially constituted, their perceptions mediated through social arrangements, ongoing interactions, and agreements. For John Greenwood (2004), abandoning these theories in favor of an individual-centered orientation (and a concordant adoption of methodological individualism) literally affected a “disappearance of the social.” As discussed above, adherence to an individualist definition of the social has significant cultural, economic, and political dimensions. It has

permitted making social-psychological knowledge that is compatible with prevailing political beliefs about individual responsibility, sound citizenship, and work ethics (Rose, 1990, 1996; Sampson, 1977, 1981). The individualist grounding of social psychology’s modern enterprise accordingly fits with larger systems of competition and exchange: Its conceptualization of the social fixes the focus on experience at the level of individual (and subjective) life experiences. As a consequence, social psychological knowledge encourages people “to accept a change in their subjective experiences as a substitute for changes in their objective reality” (Cushman, 1990; Sampson, 1981, p. 735). Macroanalytic histories indicate how the presupposition of a certain kind of individualism has yielded knowledge that is well suited to the societal regulation and administration of individuals (Danziger, 1990a; Henriques et al., 1984; Rose, 1990).

The absolutism of such mainstream presuppositions is evidenced in failed if ingenious attempts to promote alternative conceptions of social psychological phenomena. One such contestation occurred in the 1920s and 1930s when a small group of social psychologists challenged presiding models of the individual and the version of empiricism forwarded in the emerging experimental program of research. Gardner Murphy, Lois Barclay Murphy, and Gordon Allport designed an alternative perspective based on William James’s radical empiricism and a social activist stance. These researchers, according to Katherine Pandora, “rejected the image of the laboratory as an ivory tower, contested the canons of objectivity that characterized current research practices, and argued against reducing nature and the social worlds to the lowest possible terms” (1997, p. 3). In turn, these researchers called for an understanding of persons as culturally situated and, therefore, as fully explicable only in terms of the cultural conditions of their lives. They also opposed psychology’s incorporation of culturally prevalent notions of democracy and its commitment to adjusting individuals to fit into society. As Allport stated, “To a large degree our division of labor is forced, not free; young people leaving our schools for a career of unemployment become victims of arrested emotional intellectual development; our civil liberties fall short of our expressed ideal” (quoted in Pandora, p. 1). For Allport and his colleagues these unfortunate conditions called for an expanded understanding of both democracy and the individual as socially situated. They also proposed what Pandora called “experimental modernism”: a pursuit of scientific knowing that would connect reason with morals and feeling. They beckoned the generation of scientific knowledge that would challenge conventional understandings of persons and the world that psychology had assumed.

Looking forward while looking back

Histories (beyond canonical chronicles) illumine vibrant scientific landscapes upon which social psychologists can more fully and accurately appraise the field’s fundamental aspirations and commitments. They show the vital connectivity of science,

including fertile connections between theory and methods, scientific objects and cultural life, social psychologists and creative entertainers, and psychological models and economic ones. Far from revisiting antiquated debates or quaint metaphysics of the social, histories make visible to us multiple forces and agencies (besides scientists' logical decisions) that have shaped the discipline. Their significance lies not in pondering how things might have been different, but rather in providing analytic lens for better appraising scientific choices and constraints. Historical and critical reflection thereby connects with the present by providing tools and ideas for present-day researchers to assess their own choices and constraints. Such appraisal also makes space for thought experiments. Imagine setting aside the compelling research programs afforded by cognitive psychology and asking, what would a social psychology be if its conception of the social were about networks and assemblages (Latour, 2005; Law, 2004)? Or cybernetics of the posthuman (Hayles, 1999)? How might social psychology proceed should researchers explore biomedicine's new conception of persons' vitality, capacities, experiences, and responsibilities (Rose, 2007)? What would come from considering performativity, a theoretical perspective that understands psychic experience in terms of the powers of speech and language to regulate action (Butler, 1997; Sedgwick, 2003)?

This chapter's conclusion, then, ends by looking forward and glancing backward; by asking what social psychology could accomplish by taking its reflexive circuitry as a working conceptualization. Consider for a moment how scientific investigations can make new entities (e.g., "cognitive dissonance," "stress," "Freudian slip") and how new entities can arise in the social world (e.g., "virtue reality," "transgender," "surrogate mothers"). These inventions—discoveries, in turn, create further opportunities and capacities for scientists (to study) and other persons (to incorporate or resist). The discoveries, makings and namings of our scientific objects always entail relays of understanding and acting between science and the rest of culture. They can involve ruptures or radical interventions. More often the lively circuits are reiterative, iterative, and reflexive.

What if social psychology took up the project of forging models of the circuitries of the psychological? What if the conceptual and technical resources of social psychology writ large were extended to explore the (spatial and temporal) transports, translations, and circulation of the social psychological? Such a research program would permit examining not only stasis but also change; not simply a phenomenon's mechanics and reductions but also its emergence and growth; not merely stability of social-psychological objects but also movements within and between them; not linear models alone but reflexive and reiterative ones as well; not merely empirical tests of theory but also theory-based appraisals of methods; and not just the participants who generously grant data but also other cultural actors, including the experimenters themselves. Empirically registering these social dynamics, however historical the methods, is an inescapably psychological undertaking. Any account of the dynamics of making social-psychological

knowledge and persons, according to Richards, "Must itself be a Psychological model" (2002, p. 8). Or, in Smith's terms: "the history of the human sciences is itself a human science" (1997, p. 870). This historical awareness enables us to reconsider Kenneth Gergen's (1973) famed critique of social psychology as history. That is, one benefit of more accurate and comprehensive historical knowledge is that we can extend his observations to consider the possibilities of a historical social psychology. Historical social psychology could map the circuitry linking the social world, ordinary actors, scientific practices, and the scientists themselves. It would require thinking anew about our objects, for despite stolid presuppositions that social psychological phenomena stand still—that they are fixed, unfluctuating processes or entities—histories of circuits and their loopings soundly suggest otherwise. History is significant, then, not only for providing tools for reappraising the language, techniques, moral bases, and practical consequences of contemporary research but also for suggesting opportunities for developing theory.

Should imagining the theoretical possibilities of historical thinking seem beyond the purview of social psychology, history itself offers insight. Wilhelm Wundt, a "founder" of modern scientific psychology, proposed that one main avenue of psychology be *volkerpsychologie* or folk psychology, and that this strand of scientific psychology would deploy historical methods to gather, record, and assess artifacts of social and cultural life (Kroger & Scheibe, 1990). Another founding figure, G. Stanley Hall, categorized psychology's structure as threefold, consisting of comparative, experimental, and historical approaches. The historical would trace the lives of all "finished systems," including those of comparative and experimental psychology (Leary, 2009). Indeed, in psychology's nascent decades journals published historical and cultural—historical articles, although this diversity was short-lived and its proponents are all but forgotten (Pettit, 2008).

The propaedeutics of Wundt and Hall along with their colleague William James's resonant calls for diversity in psychology remind us that no single method was advanced as sufficient for the challenge of understanding and explaining mental life. These founders from the outset apprehended the limits as well as advantages of any single method. They understood that the ontology (of mind, brain, behavior, and sociality) amounted to more than invariance and mechanics and that its constitution also involved accommodations, emergences, mutations, and evolution. This yet unrealized project affords social psychologists with capacious opportunities for investigating the dynamics of the social-psychological world.

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