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History of Social Psychology: Four Enduring Tensions

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It scarcely needs stating that one never writes *the* history of a field, only *a* history of a field. Even so, how does one write *a* history of a field that has been subject to historical analysis in this *Handbook* alone four times in the last 70 years? If historical accounts are valuable, it is not solely for the events they recount, but for the lens they use to analyze them. Previous history chapters in *The Handbook*, all excellent and well-worth reading, have each explicitly adopted a particular lens through which to view the history of the field. Gordon Allport's (1954) chapter in the first edition of *The Handbook* republished with minor updating in the second (1968) and third (1985) editions, focused on the historical roots of the discipline. Jones's (1985) chapter, appearing alongside Allport's in the third (1985) edition and reprinted in the fourth edition, focused on an especially formative 50-year period in the field's history. Taylor's chapter in the fourth edition (1998) focused on the history of a central concept in the field—the social being. Ross, Lepper and Ward's chapter in the fifth edition (2010) focused on the field's enduring themes and foundational insights.

What perspective on the field's history might be useful to readers of the sixth edition of *The Handbook*? Constraining the selection is not only the existence of the chapters in the previous editions of *The Handbook*, but also the emergence of several other excellent historical analyses (e.g., Collier, Minton, & Reynolds, 1991; Danziger, 2000; Farr, 1996; Goethals, 2003; Jackson, 1988; Kruglanski & Stroebe, 2012a). Readers now have access to numerous varied and informative histories of the discipline and even critiques of those histories (e.g., Lubek & Apfelbaum, 2000; Morawski & Bayer, 2013; Samelson, 2000). In choosing our lens we sought one that complemented those of our predecessors, illuminated important historical trends, identified driving forces not centrally featured in previous *Handbook* chapters, and spoke to the current state of social psychology and to those entering the field.

Our decision was to focus on intellectual disagreements, or as we term them here, tensions. By tensions, we refer to opposing positions on important issues that have both attracted proponents and adversaries and galvanized intense empirical and theoretical attention. Through the identification and characterization of what we see as the field's central tensions we seek to give the reader a unique historical perspective on the field of social psychology and its development. We make no claims that the tensions we discuss are the only ones with which the field has grappled. It

could be argued, and likely will, that they are not even the most important tensions. Our selections were guided by two considerations: (1) that social psychologists have taken different perspectives on the tension from the field's inception to the present, and (2) that the ebbs and flows of the tension are instructive, such that charting and analyzing them provides a window into the intellectual development and sociology of the field.

In writing this chapter we tried to avoid two pitfalls that await unwary historians, especially amateur ones like us. The first pitfall is presentism or whiggish history that describes the current state of the tension in triumphalist terms, with its current state representing a final resolution. A tension that has endured for a century or more is unlikely to disappear without a few reverberations going forward. The second pitfall is American centrism. Previous history chapters in *The Handbook* have been justifiably characterized as American-centric. Ours no doubt is as well, despite the two of us being Canadian, but we hope less egregiously so.

The four tensions we have chosen, knowing that they comprise an incomplete and surely arbitrary list, are as follows.

1. *Particularism versus universalism*: tensions regarding the existence, attainability, and desirability of universally applicable conclusions about human psychology.
2. *Social context versus the individual*: tensions regarding the extent to which researchers do and should consider individuals' embedding within broader social and cultural contexts.
3. *Rationality versus irrationality*: tensions regarding how thoughtful, logical, reasonable, and adaptive people are.
4. *The dispassionate scientist versus the value-driven advocate*: tensions regarding the role that social psychologists should play in society.

Each of these tensions has powerfully shaped the course and discourse of social psychology.

PARTICULARISM VERSUS UNIVERSALISM

A recurring tension in our field has been the degree to which we should aspire to uncover universal truths about human nature. We discuss the two different forms in which this tension has reverberated across our field's history, with the common link being the question of generalizability: Can one finding, or theory apply beyond the specific parameters, samples, or context within which it was demonstrated or developed? Debates around the answers to this question have propelled innumerable research agendas and help give the field its current shape.

The first and broadest form of the universalism-particularism tension concerns whether there exist universal social psychological processes that are a fundamental and invariant part of human nature. This question is central to the social psychological project, and an outsider would understandably be surprised to learn that it remains unsettled. This question also received little attention during our field's early decades. Instead, social psychologists were preoccupied with the second and narrower form of the universalism-particularism tension: What can the experimental method reveal of the world beyond its specific operationalizations of independent and dependent variables? For example, can an experiment about stereotyping in the laboratory tell us anything at all about stereotyping in the real world? To the degree that it can—to the extent that an experiment

about racial stereotyping set in Canada in 1995 can shed light on real-world racial stereotyping in Canada in 1995—we would say in today’s terminology that the experiment has external validity (Wilson et al., 2025).

The narrow methodological question about the external validity of experiments is different than, but intimately tied to, more ontological queries about generalizability (Ferguson, 2004; Murad et al., 2018). Both concerns address the extent to which findings from a particular context and sample can provide information that applies beyond these narrow bounds. The distinction lies in the breadth of what “beyond” means: Generalizability to contexts outside the experiment versus generalizability to psychological principles with universal scope.

The history of social psychology has entwined external validity with human universals. In early debates over the experimental method and its potential for general conclusions, many of the supporters of the experiment made arguments that, in our reading, presumed that human psychological processes could be universal. Thus, we begin the history of the universalism-particularism tension with an explanation of how the forces that elevated the status of the experimental method also reinforced an orientation toward and assumptions about human universals.

How The Experimental Method Came To Dominate

When social psychology was established as a discipline (see Allport, 1954), modernism and positivism—the idea that true statements can always be, at least in principle, verified empirically or through systematic logical derivation—had already consecrated the experiment as the principal vehicle of science, one that was well-suited to uncovering universal laws and causal explanations for physical reality. However, even then, some doubted whether this same vehicle could reasonably be used to illuminate *social* reality. Wilhelm Wundt (1907; 1908), an early giant in psychology, believed that the experimental method could reveal universal principles of cognition and perception and approved of its use in these non-social subfields of psychology, but he explicitly excluded social psychological principles from its remit. This choice was in keeping with other social sciences, none of which employed experimental methods at the time. So how did the field of experimental social psychology emerge? Instructive accounts of this abound (Danziger, 2000; Farr, 1996; Jackson, 1988; Stam, Radtke, & Lubek, 2000) but we suggest that, at least in North America, three big pushes won the debate for the experimentalists (and incidentally the universalists) in an enduring way.

Floyd Allport’s Push: Restricting The Definition Of Social Psychology

A crucial step toward the experiment’s ascendance was Floyd Allport’s (1924) seminal and influential book *Social Psychology*. Floyd Allport was the older brother of Gordon Allport and was appointed in 1921 as an associate editor of the *Journal of Abnormal and Social Psychology*, the predecessor of the *Journal of Personality and Social Psychology*. In this chapter, a reference to Allport refers to Floyd; references to Gordon Allport will include his first name. Allport restricted the definition of social behavior, and hence the purview of social psychology, to *individual* behavior directed toward social objects: “reactions to language, gestures, and other movements of our fellow men, in contrast with our reactions toward non-social objects, such as plants, minerals, tools, and inclement weather” (Allport, 1924, p. 3). John Dashiell in his 1935 chapter in the current *Handbook’s*

predecessor pushed even further the reductionist perspective on social behavior by claiming that the psychological principles that operate in social situations were no different than those that operate in non-social situations. The pronouncements of Allport and his followers moved the field away from the emphasis on broad societal structures adopted by the other social sciences, particularly by sociology which until then had seen social psychology as a joint endeavor (Danziger, 1979).

The restricted definitions of social reality aligned with an emphasis on experimentation (Danziger, 2000). If the forces on behavior we seek to identify are not large, amorphous, and multifaceted sociocultural phenomena (e.g., a country's language, religion, or political system), but rather small decomposable and isolable social elements (e.g., the presence or absence of other people), then clean experimental designs become more feasible and less subject to criticism, or at least they seemed so at the time. Allport provided paradigmatic examples for his vision of experimental investigation *via* his studies on what he termed social facilitation. As Kurt Danziger (2000) noted, Allport's definition and his methodological preference cannot be separated: "His methodological innovation would not have been plausible, or even conceivable, without a radical break with conceptions of the social that formed part of the foundations of existing social sciences" (p.334). Allport believed, more than Wundt, that experiments could illuminate social processes because he had a much narrower conception of social processes.

In pushing for experimental studies, Allport and his compatriots implied that an experiment's results applied beyond the confines of the experiment itself and quite possibly revealed invariant processes. Their restricted definition of social psychology's scope excluded the study of not easily decomposable phenomena (e.g., culture) that, if studied, might have reminded researchers of arguments against universalism and for particularism.

Kurt Lewin's Push: Emphasizing Underlying Principles And Subjective Perceptions

Much has been written about Kurt Lewin in previous history chapters, those appearing in this series among others (e.g., Collier et al., 1991; Danziger, 2000; Hilton, 2012; Kruglanski & Stroebe, 2012b). Many writers have emphasized how he elevated the importance of group dynamics to equal that of attitudes as the field's paradigmatic concerns (Goethals, 1983; Jones, 1985). Whereas Allport's (1924) definition of social psychology excluded the psychology of groups ("There is no psychology of groups that is not essentially and entirely a psychology of individuals" p. 4), Lewin's vision made it possible not only to study groups in the laboratory but also to study them in a dazzlingly broad array of manifestations and instantiations. In his chapter on Lewin's field theory in the 1968 edition of *The Handbook*, Morton Deutsch identified three field-shaping components of Lewin's metatheoretical approach.

Person and the environment

First, Lewin emphasized the interrelatedness of the person and the environment underlying psychological processes. He rejected explanations of behavior that focused on characteristics of the individual (e.g., personality traits) independent of the situation, or on the situation independent of the person. In Lewin's (1946, p. 338) words, "to understand or to predict behavior, the person and his environment have to be considered as one constellation of interdependent factors". Further, Lewin

assumed that this constellation was represented in the person's subjective experience which derived not only from the physical situation the person found themselves in but also from their perceptions of it. This assumption (now known as the power of construal [Ross & Nisbett, 1991; Wilson, 2022]) enabled researchers to use the experimental method to examine a wider range of variables than the physically specified ones conceived of by Allport.

Genotypic processes versus phenotypic observations

Second, Lewin followed his teacher Ernst Cassirer (1923), a German philosopher, in advocating a focus on what they called relational or Galilean concepts rather than entity or Aristotelian concepts. The objection was not to the historical writings of Aristotle, but rather to medieval natural science's interpretation of them, which emphasized entity categorization and actuarial prediction of observed phenomena (Martin & Sugarman, 1993; Smith, 1988). Lewin (like Cassirer) instead embraced the modern physical science heralded by Galileo, according to which the task of scientists is to establish underlying principles that transcend the inevitable irregularities of surface-level phenomena. In this view, observed events *must* vary over time and among cultures, and thus serve only to provide clues to the underlying dynamic processes. Scientists must represent these non-observable dynamic processes and their relation to observed events by explicit theoretical propositions.

In Lewin's terminology, the Galilean scientist's job is to discover (genotypic) constructs that underlie and find expression in observed (phenotypic) events. For example, Galileo did not discover the law of falling bodies by collecting and cataloguing arbitrary, empirical (phenotypic) observations of falling objects under natural conditions, but by defining theoretically the (genotypic) concept of gravitational acceleration whereby, in a hypothetical vacuum, objects of different masses would fall with the same acceleration. Lewin's assumption of genotypic lawfulness sanctioned the belief that the underlying processes that were presumed to explain behavior in one environment would apply similarly across a variety of types and levels of other environments (Sullivan, 2020). For example, the dynamics underlying differences in democratic vs. authoritarian classrooms would similarly underlie differences in democratic vs. authoritarian societies. Lewin held, then, that the social psychologist's task when encountering cultural differences in, say, moral imperatives, is to trace them back to a common deep structure or underlying principle of universal right and wrong (for a more modern version of this argument, see Gray, Schein & Ward, 2014).

Systems under tension

Lewin's third meta-theoretical contribution was his emphasis on systematic rather than historical concepts of causation. From physics, Lewin borrowed the concept of a force field, and posited a psychological field, or life space as the locus of a person's experiences and needs. The individual, viewed from the perspective of Lewin's field theory, is a system under tension that experiences competing pushes and pulls. Lewin insisted that past events are of only indirect causal significance in the system and can influence it only through their representation in the person's current life space. The implication is that whatever determines social behavior at any given moment is limited to the individual's subjective experience, which could be their experience of a past life event or their current sociocultural context, but which are located within the person's mind, and therefore, *could* presumably exist within the mind of any other human. This idea reinforces the broader claim of universality, while also contributing to the primacy of the experiment. For example, a researcher need not manipulate an early childhood experience, but merely the subjective state it might induce

later in adulthood. One reason this aspect of Lewin's meta-theoretical framework was compelling was its implication that once one understood all the forces operating in a situation one could, by changing them, modify existing social reality, thereby improving group functioning, intergroup relations, leadership, and so on.

Leon Festinger's Push: Entrenching The Experiment

Festinger, an early student of Lewin, took over the supervision of Lewin's cohort of graduate students when he died in 1947 at the age of 57. As Edward Jones (1985) observed, "Festinger became the creative executor of the Lewin's metatheoretical estate" (p. 76). John Darley, using a Biblical illusion, captured their relationship this way: "Lewin is something of a Christ figure, but certainly Festinger played the role of St. Paul who made the thing happen" (Patnoe, 1988, p. 209). When a survey asked members of the *Society for Experimental Social Psychology* in the late 1970s to list 3 people who had made the greatest contribution to the field (Lewicki, 1982), 70% listed Lewin, but 79% listed Festinger. No other person surpassed 50%. It is little wonder that Festinger was introduced as the "Pope of Social Psychology" at a mid-1960s meeting of the *European Association of Social Psychology* (Jaspars, 1980). He was *the* dominant figure in social psychology from 1950 to the early 1970s and the extent of his influence is difficult to appreciate by those not in the field at the time.

Demurring from laws or grand theories, Festinger advocated ideas that have been characterized as mini-theories (Smith, 1983) and one-motive-at-a-time theories (Kelley 2000). Two examples of these are his influential theories on social comparison (Festinger, 1954) and cognitive dissonance (Festinger, 1957) that offered more concrete (and in the case of dissonance theory, more counterintuitive) predictions than Lewin's formulations. The primary relevance of Festinger's theorizing to the universalism-particularism tension is that he defined theoretical conceptualizations as statements about causal relations between variables (Schiff, 2017), thereby elevating experiments and their capacity for establishing causal relations.

Although Festinger shared Lewin's commitment to high impact experimentation (i.e., psychologically engaging, or immersive), he advocated for stricter methodological rigor. His insistence that independent variables should be manipulated in a systematic and controlled way was more in keeping with traditional American experimental (non-social) psychology. Thus, whereas Lewin's experiments (Lewin et al., 1939) created different social "climates" (e.g., those associated with democracy and autocracy) with little concern for confounds, Festinger's experiments systematically manipulated single variables. Stanley Schachter, a student of Festinger, put it this way:

Social psychology experiments before he came along were essentially morasses. In the democracy-autocracy study, for example, they were attempting to create the experimental parallel to what is democracy and compare it with the autocratic. They were manipulating a million things (Patnoe, 1988, p. 193).

Lewin probably would have accepted this characterization, which aligned with his vision of the purpose of experiments. His goal for an experiment was that it should capture the essential structure of a phenomenon (e.g., democracy) that could then then be "transposed" to other situations. That achieving this goal required manipulating "a million things" was not a problem if the genotypic structure was revealed. Conversely, Festinger was concerned with "empirical purity" rather than "theoretical purity" (Danziger, 2000). Lewin aspired to focus experimental social

psychology more on universally generalizable and mathematically precise theories, like physics—but he failed. Festinger aspired to focus experimental social psychology more on meticulously designed and executed tests of relationships among conceptual variables, like other areas of experimental psychology—and he succeeded.

The Social Psychological Experiment Comes Under Scrutiny

The universalist and experimentalist positions remained dominant in social psychology for decades, but both positions had their opponents. Numerous critics have rejected the idea that the results of experiments can illuminate *anything* meaningful about social behavior, and the intensity and scope of these criticisms have increased over time. One strain declared experiments externally invalid due to their artificiality. Others argued that there were other reasons the results of experiments cannot be generalized, including the experimenters' relationship to participants, and their overreliance on a constrained set of paradigmatic manipulations. Finally, in their most direct challenge to the broad assumption of universality, critics questioned the positivist assumptions of the experimental paradigm.

External Validity: Laboratory Experiments Are Artificial And Unrepresentative

Everyone agrees that experiments are artificial

The idea that laboratory experiments are artificial and unrepresentative of the everyday world is generally agreed upon by both sides of the universalism-particularism debate. Lewin himself saw artificiality in the laboratory as not only acceptable but desirable. Experiments, Lewin contended, should not be realistic in the sense of being like the everyday world (Danziger, 2000). In the same way that physical laws are based on idealized cases, such as frictionless planes, total vacuums, and warped space, so too Lewin envisioned that the laws of social behavior required creating ideal, and necessarily unrealistic, cases. In his words:

Whether the event described by the law occurs rarely or often has nothing to do with the law. Indeed, in a certain sense, the law refers only to cases that are never realized, or only approximately realized, in the actual course of events. Only in experiment, that is, under artificially constructed conditions, do cases occur which approximate the event with which the law is concerned (Lewin, 1931/1935, p. 12).

In making the case for his position Lewin invoked Galileo's law of falling bodies and argued that the "free and unimpeded fall" of a body does not occur regularly or even frequently in world history, and yet the law made a fundamental contribution to our understanding of physics (Lewin, 1931/1935, p. 12).

Not all early proponents of the experiment shared Lewin's conviction that social psychology *should* model its theories and methods after the physical sciences and that experiments *should* seek to discover mathematically expressible laws of social behavior (e.g., Murphy, Murphy & Newcomb, 1937). Nonetheless, most experimentalists believed that artificial manipulations of situations could be useful in testing causal relations between social behavior and intervening psychological mediators.

Some argue that artificiality undermines external validity

Starting in the 1970s, more scholars began to raise questions about the degree of artificiality that could be tolerated if an experiment was expected to pertain to people's behavior outside the experimental context (e.g., Babbie 1989; Pettigrew 1988; Sullivan 2001). Campbell and Stanley (1966) eventually introduced the label external validity, which inspired even more thinking about the concept and a proliferation of arguments that linked experiment artificiality with external invalidity (e.g., Bickman & Henchy, 1972; Weber & Cook, 1972; Willer & Willer, 1973). Harré and Secord (1972) revealed the flavor of these arguments:

Surely the actions of participants in experiments are in part a function of the laboratory structure. To the extent that this is radically different from the social structure outside the laboratory . . . we are unlikely to discover anything that can be transferred to life situations (p. 60)

Far from occurring only within the bounds of *social* psychology, similar criticisms were expressed by researchers in virtually all areas of psychology, including cognition (Neisser, 1985), perception (Gibson, 1979; Haber, 1983), child development (Bronfenbrenner, 1974; McCall, 1977), and clinical psychology (Gaylord-Ross, 1979).

The Lewinian Counterargument: The Goal Is Abstract Universal Theory Not Generalizing To The Messy Real World

Lewinians had little patience for arguments regarding external validity. For them, the purpose of experiments was to match the real world genotypically rather than phenotypically. They believed that the key issue was whether one's experiment engaged participants in the psychological processes of interest, and to do so did not require that it resemble the outside world. Lewinians sought what Aronson and Carlsmith (1968) termed "experimental realism" as opposed to "mundane realism" (see also Wilson et al., 2025). Festinger (1980) stated that "even though you may have to contrive an artificial or semi-barren situation, you want that situation to be real and important to the subject" (p. 225). For Lewinians, even those with goals more modest than discovering universal laws, it was not problematic that an experiment lacked mundane realism (i.e., if it put participants in situations unlike any they could realistically encounter outside of the lab); it was only problematic if it lacked experimental (psychological) realism.

This defense of the social psychological experiment hews to the position of the physical sciences, which distinguishes between the perfect laws of nature and their messy instantiation in the real world. Others articulated a similar position with different terms: The goal of experiments often is to test theories, not to generate predictions that will accurately and precisely be borne out in real-world situations (Berkowitz & Donnerstein, 1982; Henshel, 1980; Mitchell, 2017; Mook, 1983; Plott, 1991; Stroebe, Gadenn, & Nijstad, 2018; Zelditch, 1969).

Lewin undermined: the demise of the high impact experiment

The Lewinian defense largely rested on high impact experiments that proponents argued were, despite being artificial, psychologically real to participants, analogous to how physicists in a laboratory can artificially mimic the effects of earth's gravity on a falling object. Lewinians argued

that a high impact manipulation is usually necessary to engage research participants and capture the underlying psychology of the conceptual variable in question (Kimmell, 1998). For example, Melvin Lerner (2003) doubted how much low impact manipulations and low experimental realism could reveal about people's concern with justice. He contended that when people respond to anodyne descriptions of individuals who have experienced negative outcomes, they follow normative rules for assigning causality and blame. However, under higher impact, more emotionally arousing settings, they show less compassionate and more primitive responses to victims of injustice, such as derogating their character. In short, the Lewinian justification of artificial experiments method rested on those experiments fully engaging participants psychologically.

Nevertheless, the prevalence of high impact experiments diminished substantially by the 1970s and was replaced by what Aronson et al. (1998) called "judgment experiments". This style of research goes far back at least as far as Asch's (1946) impression formation research but gained momentum with the emergence of attribution research and further accelerated with the rise of social cognition (Devine, Hamilton & Ostrom, 1994; Fiske and Taylor, 1991; Leyens & Dardenne, 1996). It entailed experiments that rarely required stage-crafting to manipulate the independent variables and generally required only pencil and paper or reaction time measures. Research on pro-social behavior, for example, shifted from high impact studies on behavioral responses to real people in need to low impact, hypothetical studies with the needs of people described in imagined scenarios (Batson, 2012). The "scenario-study" line of research, popularized by Kahneman and Tversky (1982), helped provide this methodology with further traction as such studies could provide high mundane realism: A manipulation wherein participants imagine a hypothetical scenario can easily approximate or draw on real-world events. At the same time, their reduced experimental realism would raise questions in Lewinians' minds about their generalizability outside of lab contexts (Goethals, 2003).

Experiments Cannot Generalize Because Of Their Disregard For The Humanity And Agency Of Research Participants

An increasingly common sentiment expressed during what Silverman (1971) described as the field's most self-critical decade (some would no doubt submit new contenders for that honor) was that researchers for too long had insufficiently recognized that their research participants were sentient human beings, and not rats, pigeons, or fruit flies. Some versions of this claim appealed to morals, chastising researchers for not giving research participants sufficient respect (Baumrind, 1964) and instead treating them as means to an end and deceiving them without reservation (Kelman, 1972; Ring, 1967). Others were more pragmatic and averred that the failure of researchers to appreciate the humanness and agency of research participants undercut the validity of the results produced (Harré, 1979; Shotter, 1984).

This more pragmatic claim about the (in)validity of experimental results is relevant to the questions of generalizability we pose here and was supported by at least two different arguments. The first was that researchers underestimated research participants' capacity for independent thought and volition. Participants had their own agendas, such as responding to what they thought was expected of them in experiments as communicated by subtle and not-so-subtle aspects of the experiment. One influential paper articulated the idea thus:

The fact that the experimenter controls the information available to the subject and that he never reveals completely what he is trying to discover and how he will judge what he observes—this feature gives the experiment much of its character as a game or contest. It leads to a set of inferential and interpretive activities on the part of the subject in an effort to penetrate the experimenter’s inscrutability (Ring, 1967, p. 31).

Some versions of this first argument focused on the agenda whereby participants cared how their behavior made them appear. Studies demonstrated that this concern could affect how they responded. The threats to internal validity produced by experimenter expectancy effects (Rosenthal, 1966), demand characteristics (Orne, 1962), and social evaluation apprehension (Rosenberg, 1969) were widely discussed, and changed the way research was conducted (Adair, 1973). These factors most directly threaten internal validity, but in so doing they indirectly threaten external validity as well: If an experiment’s results are influenced by experimenter effects, then even if those results are robustly replicable, their findings cannot provide information about any psychological process that generalizes to the real world. Researchers responded by designing their experiments to minimize these threats (Jones, 1985). What they did not do was engage in more serious epistemological consideration of the challenges that social psychologists confront from having the status of both the means of knowledge discovery and the focus of that discovery: They are human beings studying experience of which they are a part (Morawski, 2005; 2012; Stainton Rogers, 2019). William James (1978) characterized it this way, “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” (p. 21).

The second argument about how the underappreciation of human agency could undermine experimental generalizability was that the heavy use of deception created suspicion in research participant pools, thereby contaminating them and affecting future participants’ behavior (Kelman, 1967). Roger Brown (1965), asking “Do any of our subjects ever believe us?” (p. 580), suggested that perhaps the real dupes were researchers themselves when they believed that participants bought their deceptions. Regarding the issue of contaminated participant pools, Gergen (1973) asserted that by discovering “laws” of behavior (e.g., the diffusion of responsibility in emergency situations) and informing the public about them, we change them (the “enlightenment effect”). Having made their way into textbooks or the popular press, “laws” of human behavior may no longer apply.

The social psychological experiment changed some in response to this second argument (Jones, 1985). Deception decreased in subsequent decades, but it did not disappear (Hertwig & Ortmann, 2008). Ethical sensibilities too changed, as reflected in the strictures of boards formed to oversee research with human subjects. These boards, variously labeled *Institutional Review Boards*, *Independent Ethics Committees*, *Ethical Review Boards* and *Research Ethics Boards*, became increasingly uncomfortable with high impact research that put research participants, often through deception, under tension. Moreover, some still question whether social psychologists yet have embraced seriously the goal of treating their participants as fully realized humans—the terminological shift from “subjects” to “participants” notwithstanding (e.g., vPittman & Sheehan, 2016, but see Moss et al., 2023).

Experimental Results Cannot Be Generalized Because They Rely Too Heavily On Too Few Paradigms

Despite the natural science-like aspirations of Lewin, neither he nor his students adopted standard operationalizations for independent and dependent variables. Their different experiments typically used different operationalizations of the same conceptual variables. However, many areas of social psychology adopted a more standardized approach. Attitude research, for example, converged on a set of measurements that bore the names of their originators: Likert, Guttman, and Thurstone. Following the lead of Carl Hovland (e.g., Hovland, Janis, & Kelley, 1953), independent variables in attitude research were similarly standardized and organized into three bins (source, mode of communication, and characteristics of the audience), famously memorialized by Harold Lasswell (1948) as “who said what to whom”.

A reason the field moved to a set of standard measures and operationalizations was that it began to abandon what McGuire (1983) termed the divergent approach—an interest in a single psychological genotype that has multiple phenotypic expressions. One example of this divergent approach was Freud’s effort to link the unconscious (the genotypic process) to everyday experiences (phenotypic expressions) such as dreams, humor, and mistakes (Freudian slips). Another is the initial efforts of dissonance researchers to show the various manifestations (phenotypic expressions) of dissonance arousal and reduction (genotypic process) in everyday life.

Instead, the field increasingly favored what McGuire (1983) termed the convergent approach—a focus on a particular phenotypic behavior (e.g., aggression, conformity, attraction) rather than a particular genotypic process. When the central interest is a specific observable behavior, it makes sense to adopt a single paradigm to measure it, and a standard manipulation for varying the factors thought to influence it. This approach, if sometimes artificial, follows the natural science model:

The social sciences had two options: They could have sampled over environments and have selected a number that would be representative of all possible and conceivable environments. Or they could have settled on one or a few ‘standard’ environments that would be consensually agreed upon, that would serve for comparable replications of experiments, where standard measures and standard operations could be performed under very similar conditions. ... The basic laws of motion are also studied in standard environments -environments that are seldom found in everyday life - perfect spheres rolling over perfect surfaces that seek to approach zero friction. (Zajonc, 1989, p.360)

Nevertheless, the reliance on limited paradigms in psychology did not inspire theoretical or empirical confidence in the way it did in the physical sciences. If the physical sciences chose their paradigms based on epistemological consensus, psychology was more swayed by convenience and narrow concerns about feasibility. This gave rise to two lines of criticisms of the reliance on limited paradigms.

Too few operational paradigms to capture the fullness of the conceptual variable

Though these methodologies may mean little to the contemporary reader, there was a time when conformity was routinely measured by the Asch (1956) paradigm and the Crutchfield (1955) apparatus; conflict by the Prisoners Dilemma Game (Gallo & McClintock, 1965); group decision making by the Risky Shift questionnaire (Wallach, Kogan, & Bem, 1962); aggression by the Buss (1961) aggression machine; and so on. Paradigms that initially generated exciting findings were used repeatedly, and in time became the gold standard, generally without having been empirically validated. So, for example, it was not only that our understanding of interpersonal attraction came from observing behavior in artificial situations, but that it came from observing behavior in one

arbitrarily-selected artificial situation—the Byrne (1969) bogus (phantom) stranger attraction paradigm in which attitude similarity was manipulated and interpersonal liking was measured with respect to a to-be-encountered stranger (Harré & Secord, 1972; West & Wicklund, 1980). Certainly, it seems tenuous to compare the knowledge-generating potential of the risky shift paradigm or the Prisoners Dilemma game in psychology to a frictionless surface or a vacuum in physics.

The reliance on single paradigms, described by Giner-Sorrola (2019) as the paradigm monoculture, gave many the impression that we were learning more and more about less and less. As Brewster Smith (1972) noted, these paradigms became “functionally autonomous vehicles” in which we seemed to be investigating what underlies the actions of participants in these specific paradigms, rather than what underlies the general class of behaviors these context-specific actions were deemed to represent. The tendency for an extremely narrow operationalization to become an acceptable proxy for a much broader universe of admissible observations has not disappeared; familiar contemporary examples being the use of the trolley problem for studying moral judgment (Bauman et al., 2014) and the use of *Implicit Attitude Test* for studying implicit cognition (De Houwer et al., 2009). These paradigms certainly tell us something about moral judgment and implicit cognition, respectively, but they do not, nor were they intended to, capture these phenomena in their totality.

Too often, the few paradigms were not empirically validated

When multiple paradigms were used, it was not uncommon for them to yield inconsistent results. This may have been because the paradigms’ construct validity was assumed rather than tested and demonstrated (Harré, 1979). Theoretical constructs can never be directly observed, so attempts to measure them need to be empirically validated (Xiao & Vicente 2000). It is critical to know whether the situation an experiment employs adequately represents the concepts and relations specified in the theoretical hypotheses under test (Chester & Lasko, 2021; Fiedler, McGaughey & Prager, 2021; Kruglanski & Kroy, 1976). Psychometrics have not been as rigorously applied to the measurement of situations as they have to personality (but see Parrigon et al., 2017; Rauthman et al., 2014; Rauthman et al., 2015). But if an experimental paradigm does not capture what Lewin called the “essence” of the intended construct, then researchers will misinterpret whatever results it bears. Some have suggested that the field has moved little beyond face validity in its evaluation of experimental paradigms (Bem & Allen, 1974; Yarkoni, 2022). Yarkoni (2022) refers to this as the generalization crises:

The modal approach is to stop at a perfunctory demonstration of face validity—that is, to conclude that if a particular operationalization seems like it has something to do with the construct of interest, then it is an acceptable stand-in for that construct. Any measurement-level findings are then uncritically generalized to the construct level, leading researchers to conclude that they’ve learned something useful about broader phenomena ...

As an example, consider the experimental study of aggression in children featuring the famous bobo doll (Bandura, Ross & Ross, 1961). How much can we learn about the role imitation plays in children’s aggression by observing the effects that imitation plays in their kicking a bobo doll in a laboratory setting? If violence visited upon a bobo doll is a valid measure of aggression, then possibly a lot, but if such violence reflects some other construct, such as playfulness, then perhaps very little (Harré & Secord, 1972).

Some scholars have argued that the past decade's heightened attention to replicability (see Giner-Sorolla, 2025 for a review) should be expanded to include generalizability beyond a study's particular methodology. Even a phenomenon that is highly replicable given the same experimental parameters may not yield particularly useful or generalizable insights if small tweaks to the measurement paradigm produce highly variable results (Gelman, 2018). Little is gained from multiple replications of poorly validated experimental paradigms (Yarkoni, 2022). Certain journals have taken the positive step of requiring authors to submit a "Constraints on Generality" statement that explicitly defines the boundaries of the universe of observations to which the authors believe that their findings apply (Simons, Shoda, & Lindsay, 2017). However, there is typically no requirement that these statements specifically address construct validity, and they tend instead to focus on generalizability to other populations—an equally important but distinct question.

Anti-Positivists Question The Existence Of Universal Truths

Thus far, we have discussed criticisms of the experimental method that in some way focused on external validity, or whether a particular relationship observed in an experimental context might generalize to any group of people who did not take part in that experiment. A completely different level of attack was levied at the experiment in the 1970s in the form of a postmodern critique that questioned the very existence of the universal laws of social behavior that the modernist social scientific enterprise assumed were its quarry. Postmodernism rejected the idea of objective and knowable laws (Pluckrose & Lindsay, 2020) and saw the experiment as a paradigm whose elevated status was unjustified (St. Pierre, 2002). In a much-debated article, Gergen (1973) advocated that social psychology abandon its modernist assumption that social behavior is lawful and insisted that social psychological researchers were engaged in the study of historically contingent social and cultural practices (and their influences on groups and individuals) rather than the search for universal truths about human nature. Gergen (1985) proposed reimagining social psychology along postmodern lines, although with limited guidance as to the successive steps (Krueger, 2002).

Compared to the other criticisms levied at the experiment and its potential to produce more generalizable knowledge, the anti-positivist critique was alone in relating to broader questions of universality. Rather than criticizing specific features and implementation of experiments, the anti-positivist claim was that the experiment's very ambition was misplaced because universal truths did not exist. The impact of these anti-positivist arguments on the ontological focus or epistemological practice in social psychology was minimal (Faye, 2012; Jones, 1985; Stam, 2006). The modernist premise that human experience is in large part comprehensible and lawful, and that experimental research is a powerful way to attain some explanation through prediction, continued to characterize the field's mainstream. What the anti-positivist critique did, however, in combination with the other criticisms, was to help shift the focus of the field to social cognition and the study of presumably basic, uncontrolled mental processes that seemed, to many at least, less likely to be culturally contingent, or to change as people learned about them (Devine et al., 1994; Fiske & Taylor, 1991; North & Fiske, 2012).

The State Of The Experiment Today

Although the concerns expressed during this version of social psychology's "crisis" were forcefully articulated, collectively they generally failed to alter standard practice (Elms, 1975; Jones, 1985). The social cognition movement, with the scenario and reaction time studies it employed, addressed some of the issues that the critiques raised (for instance, deception was typically not required, and

scenarios could realistically describe real-world situations of interest), but at its core social cognition research was vulnerable to many of the same criticisms as any experiment (Hilton, 2012). Moreover, the potential of social cognition for assisting psychology in understanding meaningful social behavior, such as obedience to authority and bystander non-intervention, was increasingly challenged (e.g., Baumeister et al., 2007; Rozin, 2001). Finally, the general inattentiveness of the field to social context troubled social constructivists (more on this issue in the next section), and even some positivists, who argued that it went too far in assuming that social stimuli and non-social stimuli were processed identically (Brewer, 1988; Lingle et al., 1984; Zajonc, 1980).

Nevertheless, these criticisms were mostly not taken to heart. It is possible that researchers simply disagreed with these criticisms; alternatively, the costs required to address them may have been too high. Without a clear and feasible roadmap for an alternative empirical approach that would ensure greater validity and generalizability, the field may have preferred to downplay the problem. If the latter, we may soon see criticisms of the experiment more widely embraced, as the current era holds promises for providing more methodological alternatives. The proliferation of big data and tools to analyze it, along with readily available behavioral observations in the form of social media use and other digital footprints, provide paths to overcome most of the criticisms of the experiment (with one exception: the anti-positivist critique), though of course these cannot yet match the experiment's ability to test causality. Moreover, online behavior does not always correspond to offline behavior, so we must be cautious in interpreting it, but the fact that it is generated naturally rather than as part of an experiment, with standardized paradigms nowhere in sight, gives it some external validity. In opening the field's eyes to viable alternatives to the traditional experiment these methods may lead to greater consensus regarding its limitations and finally weaken the field's commitment to it (Diener, Northcott, Zyphur & West, 2022). However, it remains to be seen whether the experiment's reign as the gold standard method is over, and if so, what scientific life looks like under the rule of a broader methodological oligarchy (Brannigan, 2021).

Cultural Psychology Finally Tackles Assumptions About Broad Universality

As social psychologists remained methodologically committed to experiments, disappointingly little theory construction emerged, and most theories tended to be bottom-up and inductive (driven by experimental results) rather than top-down and hypothetico-deductive (driven by theoretical reasoning). The field was, to borrow a phrase from Bruner and Tagiuri (1954) "experimentally preoccupied and theoretically underoccupied". The inductive theory that emerged generally reflected efforts to synthesize atheoretical and inconsistent research. Zajonc's (1965) account regarding social facilitation findings is one example; dual process models of attitude change is another (Chaiken, 1980; Petty & Cacioppo, 1986). True, social psychology was a young field, but the scarcity of theory and its disjointed patchwork nature—which Harold Kelley (1999) likened to a department store catalogue—did not bode well for the emergence of grand theories about "psychic unity" (Murdock, 1935).

Moreover, theories constructed through interpretation of existing data can only apply as broadly as the data do. For example, if one builds a theory of social facilitation based solely on observing the behavior of White, college educated men in America, one has questionable grounds for claiming that the theory would explain the behavior of all people. And yet this is precisely the claim implicit in much writing of the time (and to be fair, today's—more on that later). Early social psychologists

sometimes acknowledged the importance of culture for fully understanding social behavior, but they remained confident in the existence of general psychology processes, and the capacity of the social psychological experiment to discover them. Asch (1952) did not write about the psychology of “White male college students”, or even of “Americans in the 1950s”. His “immediate object was to study the social and personal conditions that induce *individuals* to resist or to yield to group pressures when the latter are perceived to be contrary to fact” (emphasis altered). Milgram (1965), for his part, introduced his obedience experiment as a way of shedding light on genocidal acts carried out across an ocean over two decades earlier. And it is difficult to imagine a more universalist claim than Festinger’s (1957) that cognitive dissonance carries “useful lessons for *everyone* concerned with understanding *human* behavior in a *world* where everything is not black and white” (emphasis added).

The stiffest rebuke to experimental social psychology’s potential to discover and describe universal truths came in the 1980s. This was not the first time that the issue of culture, or its importance for understanding social behavior had been raised (e.g., Bartlett, 1932; Klineberg, 1940; Pepitone & Triandis, 1987; Vygotsky, 1934). In fact, Wilhelm Wundt emphasized the importance of culture in shaping human psychology (Wundt, 1897/1902) and his ten volumes on *Völkerpsychologie* (Wundt, 1900–1920) can be regarded as the earliest manifestation of a socio-cultural perspective in psychology. But the movement that began in the 1980s changed a field that had until then mostly set this perspective aside.

Psychologists’ First Stance On Cultural Differences: Sure, But Who Cares?

Although eminent social psychologists acknowledged the importance of culture, and a growing corpus of research showed cultural differences, cultural psychology had little impact on mainstream social psychology for many decades (Bond, 1988; Kashima & Gelfand, 2012). There were many reasons for this. One was that cultural psychology relied on research methods that mainstream social psychologists saw as questionable. As Michael Cole (1996) observed, many felt they could ignore cross-cultural psychology because the research did “not live up to the methodological requirements of the discipline” (p. 2). It was often observational rather than experimental and was often tailored to the cultural context rather than standardized for repeated use. But this was not the main reason for the marginality of cultural psychology.

The main reason that cultural differences were not worrisome to social psychologists pursuing universal truths lay in the meaning they attributed to these differences. Donald Campbell (1959, p. 165) pointed to the difficulty of ensuring that measures were understood similarly in different cultures: “I personally am convinced that many of the cultural differences reported by psychologists and others using questionnaires or tests come from failures of communication misreported as differences”. A later, less narrowly methodological version of this claim would point to differences in “meaning” as an important source of cultural variation (Bruner, 1990; Henrich, Heine, & Norenzayan, 2010). More commonly, culture was believed to influence not so much basic psychological principles as the behavioral expression of those principles. In Lewin’s terms, culture affected phenotypic expression but not genotypic dynamics. Certainly, this was not a trivial influence, but it was not one of a magnitude that required reconsidering the field’s goal of extrapolating generalizable social psychological principles or accumulating generalizable empirical data. This position persisted as the mainstream position for many decades (Gaertner et al., 2010; Manis, 1976). A clear articulation of the distinction between content and process is Baumeister’s (1998, p. 727) description in *The Handbook* of the impact of culture on the self:

Selves everywhere have these three aspects (the experience of reflexive consciousness, interpersonal relatedness, and the executive function of choice and control). This is not to say that there are no cultural variations in selfhood which would be patently false. Rather these are the building blocks that cultures and societies can work on to shape distinctive forms of selfhood. Thus, all selves have an interpersonal dimension, but culture and societies can have considerable influence on how selves belong, interact, and respond to other people.

Cultures varied, but not in terms of basic processes or building blocks of social psychology. Festinger himself was not opposed to studying culture—in fact, he acknowledged the value of doing so—but he did not believe that taking a social psychological phenomenon seriously required evidence of cultural generality. Zajonc (1989, p. 355), probably reflecting the dominant attitude in the field prior to the 1980s, similarly emphasized that the search for the cultural generality of experimental findings should not be the first order of business in the knowledge acquisition process:

It is a pointless procedure to rush with replications of a given finding on a variety of populations, just so that we can assess the generality of the finding. Take the Schachter and Singer study (1962), for example, which demonstrated that when the individual is in a state of arousal that cannot be readily explained, he or she will draw upon environmental cues to understand it. Once found, Schachter and Singer could have replicated the study on graduate students, bricklayers, and a sample of Kuwaiti oil executives. Or better, using the same population as in the original study, they could have gone deeper into the important phenomenon they discovered and vary some of the crucial variables of their ingenious experiment, such as the intensity and clarity of the arousal brought about by epinephrine, the source of environmental cues, attempt to induce not only happiness and anger, but other emotions as well, measure other indicators of emotion besides subjective report, etc. Which would be more useful, examining the effect over different populations or over different conditions? I believe the latter.

Zajonc undoubtedly would point to the history of dissonance research approvingly in this regard. The publication of Festinger and Carlsmith's (1959) research did not result in immediate attempts to determine if its findings had cross-cultural generality, rather the effort to better understand the forms, moderators, and effects of dissonance arousal and reduction, and continuing to rely on largely North American undergraduate populations for its participant pool. The generally accepted view was that was the appropriate response because it meant that eventual efforts made to test the cultural generality of dissonance (e.g., Heine & Lehman, 1997; Hoshino-Brown et al., 2005; Imada & Kitayama, 2010; Kitayama et al., 2004; Lee & Jeyaraj, 2014) could be more sophisticated and dispositive than they would have been earlier.

An alternative argument to focus on process without first testing cross-cultural generalizability could have been that regardless of whether dissonance is a universal human process, it was sufficiently important in the North American cultural context to merit attempts to better understand the concept there. That this alternative argument was rarely if ever made underscores the widely held, often unquestioned, assumption that human universals are inherently more worthy of study than phenomena that may be culturally bound and temporally in flux, even when the latter carry pressing social relevance. We return to this point in the fourth and final tension.

Given their believe in universal processes, most researchers were not greatly disturbed when research failed to replicate a North American pattern elsewhere. However, cross-cultural research that did demonstrate generality was warmly received, especially to the degree there was distance

between cultures for which a theoretical prediction held. In fact, in some circles research was maximally satisfying if it demonstrated evidence of human psychological processes or phenomena in non-human species. None of Festinger's dissonance studies was as gratifying to him as the one that demonstrated that rats exhibited a greater resistance to extinction for learned responses for which they were insufficiently rewarded (Lawrence & Festinger, 1962). Zajonc might have felt similarly gratified by his (1965) demonstration of social facilitation effects in cockroaches. These reactions to animal studies serve as another reminder of the long-presumed goal of the field: To uncover principles of human behavior that are maximally universal and invariant.

Three Factors Lead To A New Stance: Culture Matters More Than We Thought

The philosopher Imre Lakatos (1970) distinguished between what he called the *hard core* and the *protective belt* of a research program (Gawronski & Bodenhausen, 2015). The hard core denotes the central assumption of a given theory, whereas the protective belt encompasses a larger set of background assumptions that are needed to derive testable predictions from the theory (e.g., measurement theories and operationalizations). According to Lakatos, researchers are resistant to admitting deficiencies in the core's central assumption and protect it from challenges (i.e., disconfirmed predictions) by adjusting the background assumptions of the protective belt. A social psychological example of this notion is the way that dissonance researchers, in the words of Cooper and Fazio (1984), "twisted themselves into pretzels" to retain the core assumptions of dissonance theory in the face of empirical challenges. Historically, social psychologists committed to the assumption of universality had been able to address evidence of cultural differences at the protective belt level. By the 1980s, however, at least three factors forced their way past the protective belt, thus, compelling mainstream researchers to begin to acknowledge that universality might be more elusive than they had previously imagined.

Globalization within and outside of social psychology

The 1980s was a time of unprecedented globalization as China, Japan, and South Korea rose as economic powers. An increased interest in cultural variations inevitably followed this change. Institutions, government, business, and higher education increasingly found themselves operating in more global environments, and, therefore, wanted insights into cultural differences (Cole, 1996; Hofstede, 1980; Kashima & Gelfand, 2012). Nations, and communities within nations, also asserted their distinctiveness more forcefully, thus further increasing the need for knowledge about cultural differences (Smith & Bond, 1999). These macro-level changes also meant that more people who entered social psychology had an interest in cultural differences—in response both to this external demand, but also to personal experience. For instance, among the rapid influx of young researchers from East Asian cultures entering American universities and social psychology programs (Kashima & Gelfand, 2012), some noticed obvious points of contrast that would not have been so salient to their monocultural North American counterparts. As objective as science attempts to be, it cannot escape the subjectivity of its practitioners: A body of scholars made up almost exclusively of White North Americans, even if they agree that cultural differences likely exist, may still rarely choose to study them, as they may not have sophisticated intuitions about how psychological processes might unfold among unfamiliar groups. More theorizing about cultural differences was a predictable consequence of the diversification of researchers' life experiences and intuitions about how people work. It also led to increased attention to phenomena more central to non-North American cultures (e.g., face, honor, karma, and dialectics).

The age of globalization and the internet also made cross-cultural work easier to do. Globalization allowed researchers from various parts of the world to team up to conduct research across divergent regions and cultures simultaneously. The internet enabled communicating about these shared projects and collecting data for them. As we speculated earlier about how viable alternatives contribute to researchers' willingness to entertain criticisms of the experiment, we wonder whether the feasibility of cross-cultural research enabled researchers to see it as necessary.

Growing doubts about the modernist vision

As the world transformed in the 1980s, so did the academy. Although anti-positivist criticisms have never fully pierced through the field's confidence that it can discover stable truths about social thoughts, feelings, and behavior, the dents and scars they leave behind have had some effect. Smith (1978, p. 438) may have referred dismissively to social psychology's "metatheoretical hemming and hawing about history and science", but Gergen (1973; 1978) was not alone in questioning positivism and its empiricist, modernist epistemology—the belief that knowledge about phenomena (including social phenomena) could be formulated as general laws through theoretical insight and empirical observation. Poststructuralism, postmodernism, and critical theory joined in this social constructionist critique (Agger, 1991; Harré 1984; Kukla, 1989; Sampson, 1985).

Bolstering this anti-modernist turn was social psychology's renewed relationship with anthropology, a once strong and flourishing connection that had disappeared for many decades. Anthropologist Michael Cole (1999, p. 439) offered the following comment in the late 1990s:

It is my belief that we hear so much about cultural psychology these days because the intellectual impulses associated with positivism and the Eurocentric world order with which they coincided have proven inadequate to understanding the world in the late 20th century. Just as interest in creating an experimental psychology and the first ventures into cross-cultural psychology coincided with the high-water mark of European imperialism, so does interest in interpretive social sciences and cultural psychology coincide with the implosion of European imperialism as former colonial peoples flood Europe and the United States.

Two cultural anthropologists who received an especially sympathetic and attentive ear from social psychologists were Clifford Geertz and Richard Shweder. Both opposed the search for cross-cultural generality, especially by means of the laboratory experiment, as illustrated by the following cutting passage from Shweder (1991, p. 81):

The experimental lab is still treated as a privileged space, where, quite fantastically and against much evidence, it is conveniently assumed that we can physically enter a transcendent realm where the effects of context, content, and meaning can be eliminated, standardized, or kept under control, and the central processor observed in the raw.

Geertz and Shweder envisioned and promoted a postmodernist social science. As Geertz (1973, p. 53) saw it, "the road to the general, to the revelatory simplicities of science, lies through a concern with the particular, the circumstantial, the concrete." They both favored the inductive method of ethnography over the laboratory experiment and saw cultural psychology as "Not an experimental science in search of laws but an interpretive one in search of meaning" (Geertz, 1973, p.15), one whose "main goal is not to draw up lists of common denominators. It is to understand a particular way of life, from a psychological point of view" (Shweder, 1991, p. 86).

In summary, these anthropologists and their followers favored the kind of Aristotelian, phenomenological description that Lewin decried. They did not categorically reject the existence of Lewin's Galilean human universals; they merely saw them as a less valuable end to pursue. In terms of these thinkers' influence, it has been less broad than they might have hoped: Mainstream psychology has remained overall committed to the ideal of uncovering universal truths. And yet it seems to us that now, some decades later, there is more variability in this commitment than we have seen previously. We highlight again the emergence of journal requirements for authors to include explicit statements about cross-cultural generalizability as one indicator that the field has cultural bounds on its mind.

Mounting evidence linking culture to process

A final factor that contributed to the rise of culture as an important consideration in mainstream social psychology was increasing evidence that culture could affect not only the (phenotypic) content of social psychological phenomena, but also their (genotypic) underlying processes. More and more data suggested ways culture moderated experimental findings, even those emerging from the social cognition tradition that presumed it was studying basic psychological processes. The bare boniness of social cognitive methods made it easy to apply them in other cultures, and thus to discover cross-cultural differences. For instance, basic attribution paradigms that elicited people's explanations for their outcomes and those of others yielded different results in different societies. The fundamental attribution error (Ross, 1977)—the tendency to attribute causality to actors rather than their circumstances—was found to be weaker in collectivistic cultures (Miller, 1984). These initial discoveries spurred further research and more nuanced conclusions—for example, that it might not be that the inclination toward dispositional explanation was weaker in collectivistic cultures, but rather that in those cultures it was offset by a recognition of situational constraints as similarly powerful (Choi et al., 1999).

In an influential paper Markus and Kitayama (1991) articulated the psychological implications of one central distinction among cultures. Building on an extensive program of research conducted by Harry Triandis (1978; 1989; 2018), this article described the tendency of individualist and collectivist cultures to foster independent and interdependent selves. The idea also had strong resonances with Carol Gilligan's (1982) highly cited book *"In a Different Voice"* which claimed that men and women had different orientations towards independence and connection. Markus and Kitayama's paper highlighted the intellectual perils of writing about one's findings as if they applied to humans and to the world, and paved the way for subsequent work that continues to refine our understanding of many important ideas such as dissonance theory, attribution theory, and the study of inference, judgment, and decision making (Choi, Nisbett, & Norenzayan, 1999; Cole & Packer, 2019; Morris & Peng, 1994; Shimizu, Lee, & Uleman, 2017). Coinciding as it did with the emergence of the self as a major research topic (Baumeister, 1998), its impact was powerful.

Along with a series of other publications in the 1980s that documented the impact of culture on psychological processes (e.g., Ekman, 1989; Hofstede, 1980; Holland & Quinn, 1987; Shweder & Levine, 1984; Triandis, 1989), Markus and Kitayama's paper shifted cultural psychology much closer to the mainstream in social psychology (Smith & Bond, 2022). The decades that followed would see it move even closer as cultural influences became topics of increasing interest to social neuroscientists (Kim & Sasaki, 2014; Kitayama, Varnum, & Salvador, 2019) and evolutionary psychologists (Caporael, 2007; Mesoudi, 2009).

Culture In Contemporary Social Psychology And Beyond

Where does the field stand today on the questions with which we began this section: Are there universal social psychological processes and can the laboratory experiment identify them? First, we clearly have come a long way in the century since Allport's (1924) seminal book. The experiment remains the gold standard for identifying causality, but the field has long ago abandoned the reductionism advocated by Allport's early writings and their restricted definition of social behavior. Consequently, the potential of the social psychological experiment to fully capture important social psychological processes as they unfold outside the laboratory, even culturally contingent ones, is increasingly questioned. A growing number of social psychologists doubt that any process documented in a single culture should be presumed, even tentatively, to be universal. Moreover, if universal genotypic processes exist, the growing sentiment is that their formulation must take seriously the role culture plays in their universality (Fiske et al., 1998; Norenzayan & Heine, 2005). But accepting that culture is not just another variable (Fiske, 1993) and that it is possible to have universalism without uniformity (Shweder & Sullivan, 1990) will necessitate that basic processes be conceptualized at a higher level of abstraction than they have heretofore (Markus, Kitayama, & Heiman, 1996).

Second, the field has not developed into a discipline that parallels the natural science model as Lewin had hoped. Some have advocated for the opposite: That social psychology should reinvent itself as an interpretive social science that relies on qualitative methods, including narrative and discourse analysis, rather than the experimental method. The idea of such a complete transformation has not penetrated the mainstream, but few contemporary social psychologists hold onto Lewin's hope as a realistic or desirable one. Social psychologists continue to primarily conduct experiments on samples of convenience, although now more often on participants using platforms such as MTurk rather than undergraduate pools. Nevertheless, more researchers than before explicitly state the cultural boundedness of their findings, even when not required to by journals (Kitayama, 2017)—though many (we included!) still often write their introductions and discussions about “people” and “individuals” without qualifying which ones.

Despite what we sense as a momentum toward particularism, it is unclear how far the pendulum will swing. For example, will we see increased enthusiasm for indigenous psychology—a movement that advocates that research be designed specifically for, and in collaboration with, insiders to any given culture (Kim & Berry, 1993)? If so, will it go beyond its starting point of documenting and describing the variation in how different cultures conceive of human psychology, and use this variation as a source of psychological models that are formally tested (Barett, 2022; Hwang, 2010; Kim, Yang, & Hwang, 2006)? This movement's self-proclaimed goal is ultimately to discover human universals that emerge from research that is appropriately sensitive to and adapted to the cultures in which it is implemented. Echoing the anthropological perspective we covered earlier, this illustrates how universalism and particularism may not always need to be in tension.

THE SOCIAL CONTEXT AND SOCIAL PSYCHOLOGY

How should we conceive of social context—what fundamentally is it, within what boundaries should we study it, and how should we balance our attention to context versus to the individual in isolation? One element of social context is culture, a primary focus of the previous section, but the term could capture a nearly infinite array of other constructs; for example, the presence versus

absence of other people, whether one is in a library or at a party, membership within a particular social category, the time of day or of year, or even the surrounding geographical landscape.

Broadly speaking, the tensions we have identified surrounding social context raise several interrelated questions. For example, should we define individuals in isolation from the social groups to which they belong? Must we measure group behavior or is aggregating the behavior of isolated individuals sufficient to inform us how groups (or even societies) act? If we do focus on individuals' behavior, are the most relevant forces internal to those individuals or external (i.e., emanating from their social and cultural context)? We begin by discussing how the perspectives in play at the birth of social psychology answered such questions, and then explore in greater detail how the questions and their answers have varied over time.

Bisecting Social Psychology: The Individual Versus The Collective

Experimental social psychology was preceded by nonexperimental social psychology (Collier, Minton & Reynolds, 1991; Danziger, 1979; Farr, 1996). Around the birth of experimental psychology, there were contrasting schools of thought on the appropriate unit of analysis for social behavior, and on the degree to which individuals were separable from their social context. For Allport (1924) and his fellow reductionists, the appropriate unit was an individual's reaction to social stimuli. In their eyes, analyses at the individual level were the critically important ones, because these simply could be extrapolated to describe collective behavior. Indeed, in a 1919 address to the *American Psychological Association*, Allport claimed that "there can be no effect at large which is not exclusively an effect upon separate persons", and that "when social psychologists focus their attention upon the behavior of the individual under direct and incidental stimulation from the behavior of others, then the most vital questions of the social order will find their solution" (p. 305). The embrace of reductionism was one reason that early social psychologists envisioned their science as an experimental one, fitting comfortably alongside other subdivisions of psychology.

Not everyone participated in this embrace. Wilhelm Wundt, as noted earlier, proposed that psychology incorporated two subfields, each calling for a different methodological approach (see Farr, 1996; Haslam & McGarty, 2001; Hogg & Williams, 2000; Jahoda, 2007). He conceded that lower-level processes such as physical perception and emotion seem to unfold exclusively in the realm of individual consciousness and could be studied experimentally in isolated laboratory environments (Danziger, 1979, 2000). However, Wundt believed that higher-level processes—such as reasoning, collective beliefs, and language—should not be studied in the laboratory, because they could not be reduced to intra-individual processes (Danziger, 1983). Depending as they did on "the reciprocal action of many," these higher processes could not be separated from their sociocultural context, which could not be satisfactorily mimicked, let alone manipulated, in a laboratory experiment, even with a dozen co-acting participants (Wundt, 1912/1916, p. 3). Interpretive methods that recorded these products of social and cultural life rather than experimental methods were the appropriate methods for studying these social processes (Kroger & Scheibe, 1990).

Sociological Versus Psychological Social Psychology

Durkheim (1898) shared Wundt's antireductionist position on collective phenomena but rejected his proposal that they be studied by psychologists using nonexperimental methods. Instead, Durkheim believed that the study of collective behavior warranted an entirely new discipline:

Sociology, whose domain was the processes by which societal forces create collective beliefs. Psychology, he contended, was only suited to ascertain the processes by which individuals come to select and organize private beliefs. Farr (1996) argued that Durkheim's stark separation of the domains of sociology and psychology facilitated the subsequent development of two different forms of social psychology: one, a subdiscipline of psychology, the other, a subdiscipline of sociology (Stryker, 1977). This distinction has provoked commentary and controversy for over a century (Smith, 1978).

The distance between these two social psychologies continued to grow and is typically described in terms of both ontology and epistemology (Farr, 1996; Georgoudi & Rosnow, 1985; Greenwood, 2003; Rizzoli et al., 2019). Within sociology, the collectivist and antireductionist approaches to social behavior pioneered by Wundt and Durkheim were pursued further by others, most notably George Herbert Mead (see Collins et al., 1991; Farr, 1996). Mead, not satisfied with simply separating mind from society for the purpose of study, sought to understand how they were related (Mead & Schubert, 1934; Mead, 1962). His social theory of mind contended that society influences individuals through their self-conceptions which arise and are continually modified through interaction among people. This position exemplified the idea, taken up within psychology only decades later and only by some, that it was almost always unwise to study an individual in isolation, without considering their social context. Mead proposed that the societal-level consensual representations of the world that emerge from social interaction were heavily dependent on the medium of symbols, especially language. Consequently, Herbert Blumer (1937, 1969) labeled Mead's social psychology symbolic interactionism, which over time branched into "microsociology" and "ethnomethodology" (e.g., Garfinkel, 1967; Goffman, 1961).

As our previous section on universalism made clear, the psychological branch of social psychology was dominated for several decades by experimentalism. Had experimental social psychologists adopted Mead's emphasis on the ontological indivisibility of the self and the social world, the history of the field would have been very different. Certainly, it would have focused more on the social context and less on the individual. As it was, it was sociologists who embraced and extended the indivisibility perspective and "regarded themselves as the legitimate heirs and guardians of Mead's conception of social psychology" (Farr, 1996, p.123). Sociological social psychology and the more reductionistic psychological social psychology were effectively two different disciplines by the mid-1920s (Farr, 1996; House, 1977; Jahoda, 2007; Stryker, 1977).

The Missing Social Dimension: A Century-Long Lament

The debate over whether (the psychological branch of) social psychology sufficiently attends to the social dimension has been fought continuously throughout the field's history (Gergen, 2012; Smith, 1978). Experimental social psychology, especially its North American variety, has been particularly vulnerable to criticism in this regard, both internally from its own scholars (Moscovici, 1972; Tajfel, 1972) and externally from sociological social psychologists (Graumann, 1986; Greenwood, 2003; 2014). Interestingly, the main tension is often less about the different beliefs of the various sides and more about how, in practice, to implement beliefs that are in principle shared. That is, there is widespread acknowledgment that our field *should* attend to the broader social context of individuals, but there is disagreement over how that attention should shape research.

Each of the next four subsections will delineate one way in which this disagreement over the missing social dimension of psychology has arisen since the mid 1900s. To briefly summarize, the first two of these relate to influential critiques from within psychology that appeared in the early

1970s, or roughly at the halfway point between the publication of Allport's (1924) field-defining book and the writing of this chapter. The first critique was by an American social psychologist, Ivan Steiner (1974), in an article entitled "Whatever happened to the group in social psychology?" This article, along with others that followed it, accused social psychology of restricting itself to individual-level behavior and its individual-level causes. The second critique was by a group of European social psychologists in an edited volume (Israel and Tajfel, 1972) entitled "The social context of social psychology". This European critique took up the Meadian mantle to argue that individuals are in large part defined by their social groups, and that we cannot claim to understand psychology fully without accounting for social groups as powerful causal forces.

The third way the disagreement over social context arose echoed themes peripherally present in the group-centered critiques of Steiner and the European contingent: A growing chorus asserted that social psychologists needed to be concerned not only with group memberships, but with the wider social context, as this formed an intrinsic part of individuals. As in the case of the European critiques, these arguments attempted to establish that contexts external to the individual existed as psychological realities within them that could not merely be statistically or experimentally controlled.

Finally, the oft-cited personality versus situation debate has some relevance to the tensions around social context (e.g., Epstein & O'Brien, 1985). On the surface, the deliberation on whether personal or situational factors explain most variation in individual behavior appears to assume that individuals can be cleanly separated from their social contexts. Nevertheless, a more nuanced look reveals how the person-situation debate casts doubt on what appears to be this foundational assumption.

Steiner: What Happened To The Study Of Group-Level Outcomes?

In his 1974 article, Steiner lamented the disappearance of research on groups, and especially the disappearance of a particular approach to group research and theory, and to social behavior more generally. His point of comparison with the state of social psychology at the time he was writing was not the period of Allport (1910-1930), but the post-World War II period (1940-1960). In Steiner's view, although the decades following Allport had seen social psychology become more "groupy," to use his word, it had reverted to an individualistic focus in the 1960s. Steiner bemoaned the passing of the "groupy" period in which he believed the field properly conceived of the individual as an element inextricably embedded in a larger system, group, organization, or society.

Steiner's argument focused on what the shift he identified meant for the *outcomes* that researchers investigated, although he did not completely ignore the groupy view of causes of behavior. According to the groupy perspective, individual-level behaviors such as task performance or protest attendance were not the principal phenomena of interest. Rather, a groupy approach seeks to explain emergent group-level outcomes such as variation in the success of different teams, or the effects of collective action in different communities. Interestingly, in describing this lost orientation, Steiner did not refer to many empirical findings of the ostensibly groupy post-war period. Instead, beyond a couple of classic findings, such as those of Sherif's (1961) Robbers Cave study and Lewin's (1943) research on changing dietary preferences, he drew primarily on *ideas* floated in influential textbooks of those decades (Asch, 1952; Katz & Schanck, 1938; Klineberg, 1940; Murphy, Murphy, & Newcomb, 1937; Newcomb, 1950). One suspects that even at the height of its popularity, the case for the groupy point of view might have been easier to make speculatively or theoretically, in rhetorical prose, than with experimental or even observational data.

The Critique

Small groups: the case for group-level outcomes

Steiner primarily used research on small groups to illustrate the shift from groupy to individualistic approaches. He pointed out that social psychologists' initial and distinctly groupy approach had taught them much about group-level outcomes of *ad hoc* groups; for example, the quality of their collective decisions or the kinds of products they generated. Research in this vein typically varied the size or composition of the group and observed what happened to the group decision or product, without measuring the individual-level acts of participating members. For example, a researcher might investigate the effect of group size on decision quality, without attending to what individual group members did or said differently in groups of different sizes. If researchers discussed these individual level-acts, it was in post-hoc theorizing, as they speculated about how their group-level findings *might* be mediated through individual processes such as the group pressure on individual group members, leadership style, or social facilitation effects. These discussions were almost never empirically grounded; in Steiner's words, groupy psychologists focused on "describing the ecology of the forest" which they could do "without having observed the interlocking behaviors of the trees."

Nevertheless, Steiner believed that sometime toward the middle of the 20th century, interest in and research on these individual-level process mediators grew, and that this came at the expense of further investigation of group-level outcomes. In speculating about the cause for this shift, Steiner noted that the individualistic approach was compatible with new theories such as attribution theory and dissonance theory, and that tightly controlled and brief laboratory experiments could more easily isolate local, static, and decomposable processes than they could measure meaningful, holistic, group-level outcomes (Danziger, 2000).

Attitudes: the case for group-level influences

Steiner's observation about small groups applied to other areas of social psychology as well. By the end of the 1960s, attitude research had also become more individualistic (Greenwood, 2003). The study of group-level influences on attitudes was eclipsed by approaches that focused on individual cognition (Pepitone, 1981). In seeking to understand attitude change, these approaches manipulated characteristics of the message, its source, and its targets, such as cognitive structure and latitude of acceptance, and measured individual participants' responses. Attitude researchers did not deny that this paradigm had its limitations, but typically did not recognize its lack of social embeddedness as one of them. The celebrated attitude researcher Carl Hovland (1959) described the limitations he saw in his famous "who said what to whom" experimental paradigm for investigating the attitude change:

It is, of course, apparent that there are important communication problems which cannot be attacked directly by experimental methods. ...It is not feasible to assess the effects of communications over a very long span of time. For example, one cannot visualize experimental procedures for answering the question of what has been the impact of the reading of *Das Kapital* or *Uncle Tom's Cabin*. These are questions which can be illuminated by historical and sociological study but cannot be evaluated in any rigorous experimental

fashion. But the scope of problems which do lend themselves to experimental attack is very broad.

These limitations notably did not include what Steiner would call this tradition's major flaw: Where Hovland acknowledges the paradigm's inability to understand long-term effects, Steiner would use the same example to illustrate its insufficient attention to the sociocultural dimension of attitudes (see also Greenwood, 2003). As critics sympathetic to Steiner's position would assert, Hovland's tradition takes for granted the conception of attitudes promoted by Gordon Allport (1954) in which the individual is viewed as a "thinking machine," rather than a social being embedded in a historical and cultural context that play a role in shaping the individual's attitudes (Howarth, 2006). Critics would also decry how readily Hovland relinquished the study of the societal creation and dissemination of attitudes to other disciplines, preventing social psychologists studying attitudes from ever, for instance, unpacking Kurt Lewin's (1951) contention that it can be easier to change the attitudes of an entire group than to change the attitude of a single person (Moscovici, 1963). Still others would criticize the silence of the Hovland tradition on the relationship between attitudes and public discourse, by citing, for example, how measuring individuals' racial attitudes often misses the kind of racism that emerges in cultural products like rhetoric and humor (Billig, 1996; 2003; Bonilla-Silva & Forman, 2000; Condor, Tileaga, & Billig, 2013).

Those who wanted attitude research to focus more heavily on attitudes as socially embedded and socially driven would point to earlier classic attitude research as models to follow (Greenwood, 2003; 2014; Howarth, 2006). A favorite example was Thomas and Znaniecki's (1918/1996) study of the attitude change among rural Polish immigrants as they adjusted to urban life in America in the early 1890s, which attended specifically to how "new" attitudes, such as individualism and success-seeking, arose and became part of a particular culture's structure.

The Field Embraces A Groupy Approach In Theory, But Rarely Adopts It In Practice

In a commentary on the state of social psychology circa 1970, Brewster Smith (1972, p. 94) observed:

When pressed, most of us will agree that social behavior involves interactive systems in which interactive feedback loops are more characteristic than linear causation, but experimental designs inherently produce a unidirectional snapshot. But it didn't tell us much about how members of groups create situations for one another, how one situation leads to the next, or how lengthy sequences of collective action are patterned.

The reason for this neglect, according to Steiner, was that the tight control of the social psychological experiment "legislated mutual responsiveness of participants out of existence" (p. 100). He noted that social psychologists conducted laboratory experiments that almost exclusively focused on a single participant. He attributed this to the priority the field placed on ensuring each participant had an objectively uniform experience, which was impossible if they were allowed to interact with each other. Based on this reasoning, Steiner concludes that the field's priorities prevented it from studying dynamic processes of mutual responsiveness, or groups' emergent outcomes like overall performance or decision quality.

This tendency to neglect important group properties occurred even though in principle, social psychologists recognized the importance of dynamic processes and group-level outcomes. In fact, most were more sympathetic to Sherif's (1961, p. 5) claim that "We cannot extrapolate from

properties of individuals to characteristics of groups” than to Allport’s claim, early in his career and cited in the opening to this tension’s section, that group behavior could be reduced to aggregate individual behavior. Sherif’s (1936) celebrated work on emergent norms in judgments of the autokinetic effect was one classic vindication of the importance of groups. Whereas there is no systematic direction to the movement individuals erroneously perceive in a static point of light in an otherwise dark room, when a group sits in that same dark room, each of its members comes to perceive movement in the same direction. This example demonstrated that to assume a group’s norm could be predicted by measuring the isolated perceptions of its individual members would be to commit the nominal group fallacy (Hogg, 2003).

Despite evidence from Sherif, and the field’s better judgment, however, social psychology journals are filled with studies that purport to examine group-level phenomena but simply measure (and sometimes aggregate) single individuals’ responses without allowing for dynamism or mutual responsiveness (see Horowitz & Perlmutter, 1953 for an early critique). To the speculations of Steiner for why so few social psychologists focus on group-level outcomes, we once again invoke what is becoming a theme in this chapter: pragmatics. Accepting that decisions made by co-acting groups are different from those of individuals poses an enormous practical challenge. It is difficult to arrange for the presence of a sufficient number of participants to study emergent behavior, especially given the contemporary standards for acceptable statistical power. Therefore, most social psychologists made what they considered a simple tradeoff: If they wanted experimental rigor, they could not study dynamic interaction and if they wanted to study dynamic interaction, they had to use nonexperimental methods (Solomon, Brinberg, Bodie, Jones & Ram, 2021; Waller, Okhuysen & Saghafian, 2016) that many considered inferior. Ultimately, it was much easier to proceed on the assumption that one could learn about groups by studying individual people’s isolated reactions, even in the face of empirical evidence disputing that assumption.

There are exceptions to this pattern, of course. For instance, both theory and method in research on close relationships have made considerable strides toward capturing dynamic interaction in dyadic relationships (Eastwick, Finkel, & Simpson, 2019; Fitzsimons, Finkel, & Vandellen, 2015; Murray & Holmes, 2009). One hopes that the progress made in this field portends similar progress in group research more generally.

The European Chorus: Groups Are Powerful Causal Forces From Which Individuals Cannot Be Separated

Although some scholars who lamented the absence of a social dimension yearned, as Steiner did, for a return to an earlier time in North American social psychology, others yearned for a completely new world, a more comprehensive reimagining of the discipline. This latter contingent was concerned less specifically with the paucity of research on group-level *outcomes* than with the idea that groups both big and small were an intrinsic part of individuals’ identities, and as such, could be a powerful causal force, one that mainstream individualistic North American research had not explored. Among the proponents of this position were the European social psychologists who contributed to a volume edited by Israel and Tajfel (1972). What distinguished the European critique of America’s reductionistic social psychology from the previous half century’s jeremiads on this theme was its inclusion of constructive proposals for making the field less reductionistic.

Foremost among the European social psychologists who prosecuted the case against American reductionism and for the importance of groups were Henri Tajfel and Serge Moscovici. These two

scholars are important historical figures in the field for two main reasons. First, they both offered forceful critiques of the methodological individualism that Allport ushered in fifty years earlier (Israel & Tajfel, 1972; Jaspars, Moscovici, Schonbach, & Tajfel, 1974). They whole-heartedly agreed with Graumann's (1986) charge that contemporary social psychology was characterized by the "individualization of the social and the desocialization of the individual". Second, and most importantly, they sought to expand the theoretical and empirical scope of social psychology in a way that they thought reflected European cultural and historical complexity (Prislin & Crano, 2012).

Tajfel and Moscovici focused on social processes that had received scant attention from North American social psychologists. They did not believe that these blind spots existed because the two cultures were characterized by different psychologies, as Markus and Kitayama (1991) and others would later claim existed between Eastern versus Western cultures. Rather, they saw similar psychologies operating in the two contexts, but observed differences in which aspects of this roughly universal human psychology were most salient on the two continents.

Tajfel's Contribution

As Steiner's review revealed, group research in American experimental social psychology up to the 1970s primarily concentrated on intragroup processes. A reason for this emphasis was that much of American social life at the time took place within single isolated groups that had little contact or interaction with others; for example, consider military groups (the crew of a submarine) or work groups (an office's secretarial pool). That most of the funding for group research came from the Office of Naval Research (Jones, 1985) was another reason for the small group focus. The Navy wanted to understand how crews on ships, often isolated at sea for extended periods, would get along and work with each other. With a few notable exceptions such as Sherif's (1961) Robbers Cave studies, intergroup relations were not a focus of theory or research (Dovidio et al., 2012).

Henri Tajfel's (Tajfel & Turner, 1979) social identity theory (SIT) would change the focus of social psychology dramatically. According to SIT, people's identities, an important source of self-esteem, encompass not only their individual identity but also their social identities. This insight effectively placed groups ontologically inside the mind, as cultural anthropologists (e.g., Geertz, 1973) had reconceptualized culture as something that is represented in patterns of thought rather than as something "out there" that people walked around in.

In introducing the concept of social identity, SIT influenced both methodology and theory in major ways. Methodologically, many took the concept to mean that a group study no longer needed the co-presence of multiple people: It could now be defined as research that engaged participants' social identities, even if it did not do so *via* others who were physically present, as in Sherif's Robbers Cave field studies. Importantly, groups represented as social identities were not simply reference groups, which were conceptualized as frames of reference external to people that were processed as social information (Tajfel, 1972). Instead, a person's social identities were viewed as integral part of them. Consequently, a researcher could engage social identities of participants simply by reminding them of their membership in a particular group—that is, by triggering them to self-categorize. Doing so caused people to be influenced by group norms, to behave in line with those norms, and to experience the shared concerns, interests, and emotions of the group (Smith & Mackie, 2008). For example, reminding participants of their English identity led them to exhibit more ingroup bias favoring the English over the Scottish, compared to reminding them of their identity as students (Giannakakis & Fritzsche, 2011). Tajfel himself did not necessarily endorse this simplified method of activating group identities (Condor, 2003; Reicher et al., 2010); nonetheless, the emergence of that

methodological tack, by making studies of group identities more convenient and feasible, is one reason that his theory succeeded in gaining such a strong foothold.

Theoretically, SIT and its offspring, self-categorization theory, provided social psychology with the capacity to address the link between individual psychology and collective behavior. By proposing that social groups are not just external forces but incorporated into individual psychology as a central aspect of a person's sense of self and a psychologically real entity (Turner, Hogg, Oakes, Reicher, & Wetherell, 1987; Turner & Oakes, 1986), social identity theory provided a pivot between the individual and the social world (Reicher, 2004).

The insight that individuals act as group members with shared perceptions, goals, and identity allowed individual psychological processes to be linked to group-level phenomena such as collective frustration and protest (Abrams & Grant, 2012; Grant, Bennett, & Abrams, 2017). Tajfel (1972) rejected the claim that one could extrapolate from individual frustration to collective frustration: "Shared social conduct is not shared because we are all frustrated; it is shared by those who have all basically accepted the same theory of social causation" (p.105). By this account, the emergence of coordinated social protest requires more than many individuals experiencing frustration with what they see as injustice: It requires them linking that injustice to their collective identity.

By providing the group with a psychological existence that did not invoke the metaphysical concept of the group mind, SIT also offered novel accounts of phenomena such as the deindividuated behavior that occurs in crowds. Such deindividuated behavior had formerly been described as involving the emergence of a group mind or the loss of participants' individual identity. However, social identity researchers saw it as more psychologically patterned: Individuals were still acting under the control of their own individual minds, but these were minds that contained social identities that could promote shared understandings, goals, and reactions (Abrams, 1994; Reicher, 1984; Reicher, Spears, & Postmes, 1995). The section on rationality, our third tension, discusses this distinction in greater detail.

A few years after SIT's initial formulation, Dominic Abrams, and Michael Hogg (2004, p. 100), two students of Tajfel, described how it became a meta-theory:

Whereas social identity theory itself provided a set of hypotheses and predictions about intergroup behavior, the broader social identity approach spoke to a wide range of other group and identity-related phenomena, including areas such as gender, cohesion, social facilitation, leadership, stereotyping, and social influence. The basic insight, that behavior was strongly affected by the alignment of people in terms of social group and category memberships, had ramifications for large areas of social psychology.

A reason for the meta-theoretical reach of SIT is the many psychological processes that derive from sharing a social identity with others, including self-esteem (Jetten et al., 2015); control, agency, and efficacy (Greenaway et al., 2015; Muldoon et al., 2017); and purpose, direction, and meaning (Cruwys et al., 2014; Oyserman et al., 2017). Abrams and Hogg's (2004) case for SIT as a meta-theory is reminiscent of the description that John Thibaut, one of Lewin's students, gave of field theory: It is less a theory than "a set of concepts that enables you to deal flexibly in an analytic way with any kind of situation you want to study" (Patnoe, 1988, p. 51).

Moscovici's Contributions

Moscovici (1972) agreed with Tajfel that American social psychology, with its individualistic focus, had not shown sufficient interest in certain important aspects of social behavior, especially social change and innovation. He observed that American social psychology “took for its theme of research and the contents of its theories the issues of *its own society*” (p. 19). Rather than object to this inward focus, he contended that Europe should contribute to a more comprehensive accounting by similarly focusing on itself. If European social psychologists merely studied the issues that their Americans counterparts found interesting, they would “do no more than adopt the preoccupations and traditions of another society” in an effort “to solve the problems of another society” (p. 19).

Moscovici’s major contributions to the field are his work on minority influence and social representations. Both bodies of work reflect his attempt to make social psychology more social by challenging, and offering alternatives to, what he saw as the American-produced individualistic orientation to social behavior. Like Tajfel, Moscovici (1989, p. 409) wanted to understand the relationship between the individual and the collective and asserted, as had Steiner, that “individual reactions don’t produce collective’s reactions.”

Minority influence

Asch (1956, p. 3) had insisted that “a theory of social influence should not be a theory of submission to social pressure,” but the field had to wait for Moscovici to adopt this prescription and demonstrate its value. Moscovici became interested in minority influence in part because of his own experiences with social change, which included the student protest movement in Europe during the 1960s. Drawing on his observations, Moscovici argued that American history and its geographic expansiveness blinded its social psychologists to the importance of minority influence. According to Moscovici, when Americans found themselves in a minority position, and did not want to conform to the majority, they had historically been able to leave their social circumstance for another part of the country (*exit* in the language of economist Albert Hirschman 1970). This option was not as available to most Europeans in that circumstance who therefore had to pursue minority influence (*voice* in the language of Hirschman).

More broadly, Moscovici’s theory proposed conditions under which minority and majority influence would each be effective and boundaries on how that effectiveness would manifest (e.g., Moscovici, Lage, & Naffrechoux, 1969; Moscovici & Faucheux, 1972; for a review of his theory and challenges to it, see Prislín & Crano, 2012).

Social representations

Moscovici’s other contribution to the link between the individual and the social context was his theory of social representations (SRT). Moscovici’s (1983; 1984) conception of social representations bore similarities to Durkheim’s notion of “collective representations” (1895). However, rather than the vast belief systems—science, religion, myth—that interested Durkheim, Moscovici was interested in more delimited and dynamic sets of shared ideas (Moscovici, 1984). For Moscovici, a social representation is a “stock of common knowledge and information which people share in the form of common-sense theories about the social world, which enable members of a society to be able to construct a social reality” (Augoustinos & Walker, 1995, p. 116).

Social representations are held to be major components of social reality and of the social structure of a society; they “are social because they are shared by many individuals and as such constitute a social reality which can influence individual behaviour” (Jaspers & Fraser, 1984, cited in Howarth 2006, p. 695). Their formation is dependent not only on widespread interactions amongst group members and between members and institutions, such as the media, but also on the operation of basic psychological mechanisms that serve to transform the novel into the familiar and understandable. An example would be emergent collective thinking about climate change (Bauer & Gaskell, 2008).

Unlike attitudes, social representations cannot be formed by the individual in isolation. The representations take shape in interaction, dialogue, and practice with others (Markova & Wilkie, 1987; Moscovici, 1988). The study of social representations, then, offered the promise of being able to examine large-scale social processes on the one hand, fundamental psychological mechanisms on the other, and the relations between the two (Wagner et al., 2018). Researchers might study the social origins of the construct of climate change, the way its presence in individuals’ minds influences their perceptions, goals, and behavior, and how it enters the individual’s mind to begin with. As did SIT, SRT imagined a means of linking micro to macro-level processes that did not involve extrapolation (for a review of the development and criticisms of SRT, see Voelklein & Howarth, 2005, and Wagner, Kello, & Rammer, 2018).

The Challenge Of Studying Real Groups

One of the experimental approaches most associated with SIT is the minimal groups paradigm, in which researchers place participants into arbitrary groups, and observe how even random assignment—sometimes overtly determined by a coin flip—triggers classic intergroup phenomena (Tajfel, 1970). This paradigm held obvious appeal for researchers who wanted to isolate the causal effects of group membership in general separate from the confounds associated with membership in any specific real-world group. It is ironic, nonetheless, because this very goal contravenes a core empirical imperative emerging from the European critique of individualism: That social psychologists should study real existing social groups in action, whether as a unit or in interaction with other social groups.

Such a real-world approach had not previously been the field’s inclination. For example, Allport’s (1924) distinction between “co-acting” groups (those where people participate in one another’s presence) and “face-to-face” groups (those where people participate in joint activities) made no reference to either type of group as having a social identity. Creating these types of groups in the lab permitted the study of social influence in the narrow sense of dyadic effects of one person on another, but both deliberately set aside the broader influence that might result from a sense of shared belonging to a socially important group.

Some early social psychological experiments known as “reference group” studies (Charters & Newcomb, 1952; Kelley & Volkart, 1952; Siegal & Siegal, 1957) pre-selected participants who were members of genuine groups. They provided a glimpse into the social dimensions of cognition, emotion, and behavior. However, integrative research on reference groups was never seriously developed (Singer, 1981). A reason for this lack of development was the field’s increasing commitment to experimental and statistical rigor, a posture that viewed any form of “psychological connection” among participants based on their membership in similar social groups (e.g., Catholics, Democrats) to violate assumptions about statistical independence and constitute a source of confounding (Greenwood, 2003).

Person-Context Interdependence

The importance of groups and social identity, highlighted by the European critiques we have just discussed, are both related to a broader argument whose refrain grew in intensity during the later decades of the 20th century (Cartwright, 1979; Collier et al., 1991; House 1977; Pepitone, 1981; Taylor & Brown, 1979; Smith, 1972). The argument was that any serious attempt to understand *social* psychology must account for the *social* context within which individual behavior takes place, including group memberships but extending far beyond them. This view led to further criticism of the field's individualism: Its emphasis was on intrapsychic processes (Rizzoli et al., 2019) that were usually treated as independent of the objective social environment (Katz & Kahn, 1978; Pepitone, 1981). Many agreed with this criticism; even social psychologists who specialized in laboratory research acknowledged the field's blind spot in this regard. Morton Deutsch (1999, p. 29), for example, said the following of America's individualist ethos: "I think this image has influenced American social psychology, which has been too focused on what goes on in the head of the subject with a corresponding neglect of the social reality in which the subject is participating".

One must remember the Lewinian position, however, that the challenge for social psychology was representing relevant social contexts in the life space of the individual. For example, many Lewinians would have acknowledged that social class was a variable that powerfully influences social behavior but would insist it was not a psychological variable (i.e., something that could be represented in a person's life space) and, therefore, not an appropriate topic for social psychological investigation (Krauss, 1998). It was appropriate to study the impact of an individual's perception of their social class, but not the impact of objective markers such as their parents' income or educational level.

Two Versions Of Interdependence

Despite considerable consensus regarding the interdependence of person and context, social psychologists have disagreed about precisely how to conceptualize it. The most common rendering treats individuals and contexts as separate constructs, connected by sometimes bidirectional causal pathways. Dorwin Cartwright (1949) defined the field's mission thus: "Social psychology is that branch of the social sciences which attempts to explain how society influences the cognition, motivation, development, and behavior of individuals and, in turn, is influenced by them" (p. 91). Similarly, Gordon Allport (1954) echoing Comte (1855), called social psychology's master question: "How are we both shaped by and shaping of our social world?" Although these quotes acknowledge the bidirectionality in the relationship between the individual and society, most contemporary social psychologists likely concur with Moscovici's (1972) observation that the interrogation of this relationship has been decidedly one-sided: We know a lot more about the question of who socializes the individual than the question of "who socializes society?" (p. 59).

In any case, the definitions of interdependence cited above imply that individuals and society are separate, if mutually co-created, entities. They offer a straightforward vision of person-situation interdependence, and one that lends itself well to the idea of experimental manipulation, and diagrams of boxes and arrows. These features of this view of interdependence may help explain its popularity. Greenway et al. (2015) embrace this perspective in their discussion of the contextual dependence of emotion:

We adopt Burke, Joseph, Pasick, and Barker's (2009) definition of context as a collection of sociocultural forces that shape experience, as this definition highlights the diverse nature of context. These sociocultural forces range from micro-level (intra- and inter-) personal factors that differentiate individuals and groups to macro-level political and historical factors that differentiate cultures. ...We have organized these contextual features into a framework presented in Figure 1, with levels ranging from micro-level (person) to macro-level (culture).

This perspective on emotion defines context on multiple and progressively expanding levels, each of which contains possible causal factors that might influence a person's behavior, but nonetheless remain conceptually separate from the person. It also allows for individuals to influence their context, for example through situation selection (e.g., choosing with whom to be friends or moving to a neighborhood where residents' politics align with one's own [Webb et al., 2018]).

A second, less popular but nonetheless recurring, view of person-context interdependence has more in common with the spirit of Tajfel's argument and insists that individuals simply cannot be studied as distinct from the groups to which they belong. We can trace this more complex view of person-context interdependence at least as far back as Lewin (1946; 1951), who in formulating his famous formula $B=f(PE)$ did not intend for P(erson) and E(nvironment) to be separable, additive factors, but rather "one constellation of interdependent factors" (p. 240, emphasis in the original). As he expressed it (Lewin, 1946), $E= f(P)$ and $P= f(E)$. In Lewin's conceptualization, behavior is a function of the life space, which is a unitary representation of all the environmental, biological, social, and psychological influences that define the person's unique reality at a given moment in time (see Oishi, 2014; Reis, 2008, 2012).

Similarly, in proposing what he deemed a more productive version of individualism, Sampson (1988, p. 17) argued that we needed to understand how "society constitutes and inhabits the very core of whatever passes for personhood: Each is interpenetrated by the other." Gantt and Williams (2002) advocated for a "more relational conceptualization of social behavior according to which neither individual human action nor social structure is reducible to each other but are intricately interwoven and codetermined" (see also Georgoudi & Rosnow, 1985). According to this view, it is insufficient to accept that individuals live in a social world that may influence the way the individuals behave, and that individuals may influence themselves. Rather, if we accept that the individual and the social context are mutually constitutive realities (Batel & Castro, 2018; Greenwood, 2003; Moscovici, 1988), historical, cultural, and institutional factors must be integrated into what we consider psychology, because they are essential to what it means to be a person (Doise, 1986; Gergen, 1973; Harré & Secord, 1972). The mutually constitutive view asserts that defining an abstract hypothetical acontextual person would be like defining an island without reference to the body of water it is surrounded by: Perhaps one could but, they would argue, to what end?

This more integrative view of person-context interdependence has been less common, even rhetorically, than the separate entity view, and empirically, it is almost nowhere to be found. Once again, this may be due to pragmatics, and the reality of our discipline's methodological training. If the context and the person are separate entities that can influence each other, the most straightforward methodological prescription is to manipulate one and measure its effects on the other, using basic inferential statistics to test for differences between groups. In other words, the separate entity view calls for exactly what social psychologists have traditionally been trained to do, offering them a clear direction forward—even if some broader contexts (e.g., overarching

political structures) can be challenging to manipulate. By contrast, the mutually constitutive view offers little in the way of testable, falsifiable hypotheses: If everything we study is a function of social context, and if social context is constantly changing, then the very meaning of manipulations and measures, let alone their expected associations, will also be in constant flux and impossible to pin down with our field's standard tools.

An Ironically Individualistic Solution To The Criticism Of Excessive Individualism

A convenient solution to the question of how to manipulate situations and methodologically represent mutual constitution is the same trend toward subjectivism and its emphasis on mental representations we discussed in the first section on universality. As Ross and colleagues (2010) described in an earlier edition of this *Handbook*, there was a growing consensus toward the end of the 20th century that what matters is not solely a person's objective reality, but also, and perhaps more primarily, their interpretation or construal of what they experience. This assumption makes it simple to manipulate even long-standing or complex sociopolitical contexts. If a researcher can induce participants to perceive—or perhaps even imagine—the environment that the experiment aims to mimic, the researcher can analyze the outcomes of interest regarding the social context. This assumption turned researchers away from the social in a different way, as they became increasingly interested in understanding where (internal) subjective interpretations originated. For example, what individual differences (Nasby, Hayden, & DePaulo, 1980) or cognitive activations (Srull & Wyer, 1979) would cause people to interpret another person's ambiguous behavior as hostile?

The assumption of subjectivity vastly simplifies the question of mutual constitution. If the social context is only meaningful as it exists within an individual's mind, then persons and contexts cannot be separated, as one literally contains the other. Ironically, it is also a deeply individualistic way of defining mutual constitution. It is difficult to imagine Tajfel or Moscovici endorsing the idea that society and the individual are inseparable because the former is operative only within the mind of the latter. Likewise, the strongest advocates of mutual constitution surely would resist the claim that we can learn something meaningful about how membership in a disadvantaged group shapes behavior solely by inducing objectively privileged people to perceive one of their social groups as occupying a lower status or minority status position (Condor, 2003; Reicher, 2004).

The Person–Situation Debate

The final version of the debate over the relevance of social context that we discuss is generally better known today than the other three: the person-situation debate. The history of this debate is covered in detail elsewhere (e.g., Funder, 2009, Roberts, 2009; Snyder & Cantor, 1998), and in most introductory psychology textbooks, so we outline it only briefly before narrowing in on its relevance to this section's central concern.

A Summary

Until the 1960s, many psychologists drew inspiration from those such as Gordon Allport (1937) and Henry Murray (1938), who had argued that there was at least a degree of consistency in behavioral differences among people. This idea provides the foundation for personality psychology: Person A is consistently more friendly than Person B because they vary in trait friendliness. A major challenge

came in 1968 with Walter Mischel's *Personality and Assessment*, in which he pointed out that a single individual's behavior across diverse situations is in fact surprisingly *inconsistent* and argued that the social context has a far stronger influence on an individual's behavior than their so-called personality. This latter idea—often termed situationism—gained traction in mainstream social psychology (Ross & Nisbett, 1991; Ross et al., 2010), and persisted over decades. Stanley Milgram, one of the field's most distinguished situationists, captured it this way, "Often it is not so much the kind of person a man is as the kind of situation in which he finds himself that determines how he will act" (1974, p. 205.) John Darley (1992), another situationist par excellence, offered a similar rendering that "when one probes behind evil actions, one normally finds not an evil individual viciously forwarding diabolical schemes but instead ordinary individuals who have done acts of evil because they are caught up in complex social forces" (p. 204). This idea—that Person A and Person B will be friendly only insofar as the situation prompts them to—is often represented as the core of social psychology (Taylor, 1998).

The debate between personality psychologists and situationists was fierce, with the former offering numerous justifications for the validity of traits. For instance, they argued that researchers needed to measure the consistency of a constellation of related behaviors, as opposed to a single behavior (e.g., Epstein, 1979): Person A might not consistently strike up conversations with strangers, but they do consistently *act friendly* if we include within that definition other behaviors as well, such as smiling at others, helping struggling acquaintances, etc. Personality psychologists also argued that even though situations could influence behavior, there were strong individual differences in the situations that individuals subjected themselves to (e.g., Snyder & Cantor, 1998): Person A is more likely to attend social functions than Person B, which gives them more opportunities to act friendly. Consequently, a lab study that deliberately manipulates the social context across participants may be controlling the very source of the consistent difference in behavior between Person A and Person B. If the person-situation debate is over (Fleeson & Nofhle, 2008), most social psychologists have landed on a position of interactionism whereby both traits and situations likely matter to important degrees, and in combination with each other.

Implications For The Role Of The Social

On its surface, this debate is between people who think that the social context matters more (situationists) versus less (personality psychologists). The field's resolution to this debate, interactionism, is typically conceived of in statistical terms (situations affect people differently depending on their personality), but this is not the same as interdependence, either in the Lewinian or mutually constituted sense in which persons do not exist separately from situations. Many of the advocates for the social, whom we have already covered, would thus be dismayed by the assumptions shared by both sides in this debate. First, the debate focuses narrowly on the behavior of individuals, which Steiner might point out ignores important group-level outcomes. Second, most of what has been written on either side, even on the interactionist compromise, contradicts the idea of mutual constitution: It assumes that individuals and their social contexts can be treated as separate variables, albeit ones that might mutually influence each other. But the mutually constitutive view argues that this is theoretically wrong: There is no abstract person who exists independently of any situation.

A closer look at the person-situation debate serves to highlight some additional nuance in the tension over how to conceive of the social. One reason the debate has been able to separate so cleanly individuals from their situations is that it focused on a limited range of short-term

situations. This emphasis arises because social psychologists traditionally have examined proximal rather than distal situational causes of behavior (Taylor, 1998). They have been most captivated by the sometimes powerful influence of the immediate social environment because this influence often seems so counterintuitive: Who would guess that finding a dime in a phone booth makes it more likely that someone will help a stranger in need of assistance (Isen, 1970), that the presence of other people would discourage someone from intervening in an emergency (Latané & Darley, 1970), or that the experimenter being in versus out of the room would influence whether someone follows their seemingly sadistic instructions (Milgram, 1965; 1974)?

Proponents of situationism in the person-situation debate maintained this focus on proximal situations. It is easy to contrast the person with the situation if we operationalize them, respectively, as a participant's self-report of how outgoing they generally are vs. whether they are currently in a library or there are other people present. However, if we broaden the scope of what we mean by "situation" to extend beyond the immediate environment and include longer-term and more distal social contexts, the distinction between the person and the situations becomes fuzzier. For example, imagine that Person A grew up in a social context that rewarded extroverted, sociable behavior, whereas Person B did so in a milieu that emphasized the value of reading and self-sufficiency. Suppose we find that Person A, in the aggregate, is more likely to behave in outgoing ways than Person B—would we count this as a win for traits, as it appears to show a consistent between-person difference across short-term situations? Or a win for situations, because clearly the social context of origin powerfully shaped (and perhaps continues to shape, through the psychological constraints of construal, Gilbert, 1998), these two people's behavior? If chronic exposure to a situation can result in a pattern of behavior that persists over time even once that situation changes, the traditional lines drawn by the person-situation debate become blurry indeed.

Where Are We Now?

Are There Still Two Social Psychologies?

In short, yes. Psychological social psychology (PSP) mostly continues to embrace the positivist epistemology of the natural sciences and assumes that there is an objective reality that can be understood by applying the tools of empirical method. Central to this paradigm is the hypothetico-deductive approach and the belief in internal causal mechanisms that can be identified through rigorous empirical research. Despite the criticisms that we have reviewed, the field remains mostly committed to the assumption that understanding individual experiences can tell us a lot about social behavior. Thus, it sees the task of research as identifying individual-level processes, often discussed as universal, such as fundamental needs, core motivations, traits, and cognitive capacities (Rijsman and Stroebe 1989). Few contemporary social psychologists would explicitly subscribe to the early views of Allport, the most obvious forefather of PSP, and especially his view that groups have no psychological reality, an idea that even he moved away from late in his career (Allport, 1955). Nevertheless, Allport's methodological individualism still dominates the field, and not only in North America (Markova, 2012).

Alongside PSP exists the admittedly much smaller field of sociological social psychology (SSP), which sees the social world and the self as indivisible and mutually constituted with no sharp divide between the two (Greenwood, 2014; Moscovici 1988). This social psychology views its task as being to consider how social and historical facts help constitute psychological subjects, relations,

and contexts (Gergen 1973; Harré & Secord 1972). In pursuit of this task, SSP adopts interpretivism, which holds that reality can never be objectively observed from the outside but must be inferred from inside through the direct experience of the people (Mack, 2010). The researcher's role in this enterprise is to "understand, explain, and demystify social reality through the eyes of different participants" (Cohen, Manion, & Morrison, 2011, p. 19) using qualitative as opposed to quantitative methods. There are many flavors of SSP (see Kruglanski & Stroebe, 2012b), including social constructionism (Gergen, 1973), humanistic psychology (Shotter, 1984), ethogenics (Harré, 1979), critical realism and discourse analysis (Billig, 2003; Edwards & Potter, 1992).

The relation of SSP to PSP is reminiscent of the separate cultures that C. P. Snow (1959) described as forming around science on the one hand, and the arts and humanities on the other. The two approaches now share only a tenuous bond. In an attempt at reconciliation, the *European Association of Social Psychology* dropped the word *Experimental* from its title and its flagship journal expressed a commitment to publishing articles from both traditions, but over time the number of articles reflecting the epistemological and methodological orientations of SSP have diminished (Rizzoli et al., 2018). According to Gergen (2012, p. 151), one reason for the lack for rapprochement between the two camps is that "there is a certain satisfaction in the status quo on both sides of the continuum." Another reason is that although some consider it possible to merge the two (Fiske & Molm, 2010; Jost & Kruglanski, 2002), most agree with Smith (1978), who observed that "you cannot split the difference" between them (Rizzoli et al., 2018).

In Psychological Social Psychology, Experiments Have Not Become Less Individualistic

Within PSP, the *experimental* work being done today shows little evidence of having been influenced by the spirit of the critiques of Steiner or the European social psychologists (Schruijer, 2021). One reason for this may be, ironically, the influence of SIT. Contra to the intent of the theory's developers (see Condor, 2003), SIT legitimated the study of groups in which group members never interact, thus making assessment of their behavior unnecessary (Levine and Moreland, 2012). There are multiple reasons SIT has served as the conceptual framework for much small group and intergroup research in the last 50 years, but part of its appeal has been the ability to conduct group research without having to study individuals interacting with each other.

Another factor that may (at least initially) have stood in the way of experiments becoming less individualistic is the rise of internet platforms such as MTurk. These allow for cheap, expedient, and large samples, which has compelled a great many researchers to run most of their experiments online rather than in laboratories. Setting aside questions regarding the representativeness of samples drawn from online pools (Anderson et al., 2019), and participants' attentiveness (Fleischer et al., 2015), most of these studies rely on low impact manipulations, and study individuals in isolation rather than in interaction with each other. However, applications are being developed that allow participants in online surveys to communicate with each other, using video or instant messaging. Further, internet-mediated experimental social contexts increasingly reflect dominant social contexts in many people's real lives. Thus, online platforms could be poised to deliver practical and convenient methods for the study of dynamic unfolding processes between groups of people, in social contexts that have some degree of ecological validity.

Psychological Social Psychology Has Become More Interested In Broader Social Contexts

If experiments *per se* have not become less individualistic, PSP has nevertheless been gradually shifting how it conceives of social context.

A long era of proximal focus

We noted earlier that for most of the field's history, the attention it paid to context was to proximal situations: an actor's immediate surroundings. Criticisms of this narrow focus are not new, as illustrated by this quote from one of the most influential textbooks of the 1930's (Murphy, Murphy, & Newcomb, 1937, p. 10):

It has become very evident in recent years that the social psychologist has thrust many of his problems into the laboratory without adequate consideration of the matrix in which his most certain and valuable data lie. He has simplified his phenomena in such a way as to exclude essential facts necessary to the understanding of social behavior, and has succeeded in experimental and quantitative control by leaving out most of the variables about which we really need to know.

Yet, this omission persisted—and not because social psychologists discounted the importance of distal contexts, such as political structures (see Lewin, 1951; Steiner, 1974; Tajfel, 1972). Rather, it is because of the dominance of the experimental method that distal contexts have tended to be experimentally operationalized as psychologically proximal variables (e.g., Pettigrew, Wagner, & Christ, 2010). An example of the conversion from proximal to distal variables is choosing to study subjective perceptions (Wilson, 2022): Asking participants to report how they see their country's political structure—or even manipulating this perception by highlighting counterfactuals in opposite directions (e.g., providing examples of more versus less democratic nations). Another example is instantiating different political structures in the laboratory, as Lewin and his colleagues (Lewin, 1951) did in their experiments on leadership styles—although more laborious and therefore, rarer, this approach is nonetheless a manipulation of a proximal (psychological) variable, or what the research participant is currently experiencing, rather than a distal (structural) one.

Another reason that social psychologists have focused on the proximal rather than the distal context of social behavior is that proximity is baked into their traditional conceptualization of “influence”. Consider Gordon Allport's (1954) famous definition of the task of social psychology as seeking to understand “how the thought, feeling, and behavior of individuals is shaped by the actual, imagined or implied presence of others.” Specifically, to use Lewin's terms, the “shaping” influence of the proximal context takes the form of pushing or pulling the individual. Most experimental social psychologists hold views like those of Milgram (1992) who described his own work as “a social psychology of the reactive individual, the recipient of forces and pressures emanating from outside oneself” (p. xix). A question worth pondering is whether experimental evidence showing the power of the situation reflects more the field's commitment to a meta-theoretical view of social behavior or its commitment to the experiment, the strongest suit of which is showing the power of the (proximal) situation (Branigan, 2021).

Whatever influence more distal social contexts, including culture, have on individuals, they are not typically described as pushes and pulls or forces and pressures. Rather, scholars interested in distal

contexts describe people as being “interwoven” with their social context or “embedded and psychologically subscribed” in it (Georgoudi & Rosnow 1985; Pepitone, 1981). Similarly, those focused on the broader social context describe behavior with terms such as “flexible across differing contexts” (Reicher, 2004) rather than with more deterministic language such as “contextually contingent.”

A more recent expansion

In recent years, the field has grown more interested in broad societal contexts and how these distal variables influence behavior. A well-known example of this is Nisbett and Cohen’s (1996) study of differences in contemporaneous violence observed in the U.S. Their undertaking was motivated not by the goal of finding a universal process underlying violence but rather explaining a regional difference in violence. Their account of the honor culture in the U.S. South described how in the 19th century, southern herders needed to protect their livestock by maintaining a reputation for toughness, partially due to the absence of a reliable police force in rural areas. Nisbett and Cohen’s analysis of the culture of honor proposed that distal factors such as herding, combined with immigration history (e.g., Scottish Highlanders) and social conditions (e.g., lack of police force), gave rise to the creation and maintenance of social norms of toughness, self-defense, and violence in the U.S. South.

Another example of researchers seeking to explain regional behavioral differences by reference to macro variables is the quest of Penic and colleagues (2020) to understand why reconciliation and justice following civil conflict are only sometimes seen as compatible. They found that these views appeared to follow a specific historical aspect of these conflicts. Reconciliation and justice were viewed as compatible when both sides had inflicted and suffered harm (i.e., symmetric violence) but not when one side predominantly inflicted harm while the other largely suffered (i.e., asymmetric violence). Similar efforts to tie micro-outcomes to macro causes are found in research examining how contact effects racial prejudice (Pettigrew, 1997) and how the prevalence of historical pathogens affects cultural values and personality traits of extraversion (Fincher et al., 2008; Schaller & Murray, 2008). Many socioecological factors cut across different levels of the environment (Oishi, 2025; Oishi & Graham, 2010): economic conditions (e.g., unemployment at the individual level, recession at the macrolevel), residential mobility (e.g., frequent moves at the individual level, mobile versus stable neighborhoods at the macrolevel), dominant economic or agricultural activities (Henrich et al., 2005; Talhelm et al., 2014), legal systems (Eisner et al., 2021) and political systems (Inglehart & Welzel, 2005).

Multi-level accounts of behavior are increasingly proposed as the promise of the field’s future. Sometimes this proposal takes the form of recommending closer links with other macro-focused social sciences such as sociology (Cook, Fine & House, 1995; Eagly & Fine, 2010; Oishi et al., 2009; Salee & Brossard, 2019) and political science (McDonald et al., 2017; Scuzzarello & Kinnvall, 2013). Cikara et al., 2022; Caccioppo & Bernston, 1992; Doise, 1986; Pettigrew, 2018) and ranges from the evolutionary to the neuroscience levels. An illustrative recent product of the multi-level approach is the finding that community-level racial prejudice potentiates the neural responses of Whites to outgroup faces (Hatzenbuehler et al., 2022).

Potential Causes Of The Shift Toward Broader Social Contexts

Methodological innovations

An important factor that helped shift the field's attention toward distal variables may have been developments in non-experimental methods and analytic tools. These developments occurred in part because of growing disenchantment with the experiment as an epistemological tool. Analytic tools now exist that enable researchers to make stronger causal inferences about distal variables with non-experimental data (L. Miller et al., 2019). These tools include multilevel structural equation modeling (Pettigrew, 2018) and multiple programs that analyze multi-level data (e.g., HLM, LISREL, Mplus, MLWIN, SAS, SPSS, and SYSTAT). With the barrier to causal inference lowered, multi-level analyses that consider the macro-level of institutions and societies should flourish in the future. Agent-based modeling (Miller, 2015; Smith & Conrey, 2007; Smith & Beasley, 2015) is another valuable tool. An agent-based model simulates a system made of agents who have individual-level attributes such as traits, goals, and strategies and allows researchers to investigate emergent macro-population-level phenomena from the repeated interactions of these agents. Artificial though this method and its researcher-controlled environment may be, it offers a practical way of testing for the causal influence of variables that lab experiments could never hope to realistically manipulate. Research using agent-based modeling has, for example, shown that complete segregation of neighborhoods may result merely from individual's desire to avoid being in the minority in a neighborhood (Schelling, 1980). In other words, segregation does not require that people hate each other or want to escape from members of other races, only that they prefer being in the majority (Smith & Conrey, 2007).

Shift from processes to phenomena

In our view, a second and possibly important factor in the increased interest in broader contexts is the field being increasingly organized around phenomena or dependent measures (e.g., health, inequality), rather than processes (e.g., dissonance reduction). This convergent rather than divergent approach (McGuire, 1983)—this interest in phenotypes more than genotypes—encourages researchers to use a wider lens in their work. The situating of micro phenomena in broader macro contexts seem more necessary when the goal of studying the phenomena is to understand them in and of themselves. For example, the existence of online platforms is a macro variable that complicates established social psychological phenomena such as self-presentation and social mobilization (Bayer, Triêu, & Ellison, 2020). If these phenomena are one's focus, then studying how they unfold online versus offline is increasingly important. When the goal is instead to understand a process (e.g., cognitive dissonance) of which any given behavioral manifestation is only one of many that the process might produce, the broader context for the manifestation might seem less relevant. The increased interest in the broader context seems progressively to be shared by granting/funding agencies (Giner-Sorolla, 2019; Major, 2006; Markus, 2004).

One might wonder what diminished the field's interest in abstraction—or what changed from the time when the Lewinian influence would have led social psychologists to deride a proposal, for example, to study how hate speech or bullying appears differently online versus offline? We can think of at least three reasons. One is that the Lewinian focus on genotypes requires a reductionism that may now strike people as not only theoretically unjustified but morally offensive. Consider religious experience as an example. It seems quite likely that some in the Lewinian circle would have believed that they could identify the “essence” of religious experience or religious commitment and capture its genotype in the laboratory. In contrast, now some might find such a project to be morally problematic reductionism, as it fails to account for and respect differences

among diverse religious traditions. Similarly, regarding research on the experience of membership in a disadvantaged group, it is now more apparent than in the past that we must examine, both theoretically and empirically, the ways and degree to which the experience of being a woman is and is not comparable to that of belonging to a marginalized racial group, coming from a family with lower socioeconomic status, or to being an elderly person. This is one reason the current *Handbook* has separate chapters for each of these social category dimensions. If the concept of intersectionality continues to gain in importance in our field, future *Handbooks* will have to decide whether this means further subdividing chapters, re-integrating them into a single mammoth one, or asking authors of all chapters to incorporate demographic and group-status considerations into what they write.

A second reason for the shift from process to phenomenon may be the target audiences. One of Steiner's (1974) laments was that social psychologists seemed more interested in doing research for other social psychologists than for broader audiences. For example, did the public really care about adding versus averaging debates in impression formation (Anderson, 1965), multiple causes of group polarization (Pruitt & Kimmel, 1977), or self-perception versus cognitive dissonance accounts (Zanna & Cooper, 1974)? Social psychologists certainly have not lost the capacity to become obsessed with journal-focused topics, but they are now more likely than they have been to focus on topics about which inquiring minds want to know. For instance, Nisbett and Cohen's (1996) work on the culture of honor in the U.S. South was motivated by what Rozin (2009) approvingly termed "informed curiosity".

Other trends indicate a greater orientation toward topics that a general audience would find interesting. Social psychologists submit more papers to high-impact, pan-disciplinary journals like *Science* and *Nature* that require appeal beyond researchers' own field. New journals and preconference research meetings focus on specific topics of public interest. Blogs and listservs easily identify social psychological "wow" research that speaks to topical interests such as intimacy with robots, intersectionality, and the costs and benefits of virtual work (Ellemers, 2013; Strack, 2012). Previously, social psychologists wrote monographs for their peers; now, following the success of authors such as Malcom Gladwell, they write books almost exclusively for general audiences.

A third, and related, reason for the shift from process to phenomenon may be social psychologists' increasing presence in academic units like business schools and public policy schools, as well as in industry. These areas are organized more around real-world phenomena than around their underlying psychological mechanisms. They also have a more applied focus, a topic we will return to when we reach our fourth tension.

THE RATIONALITY-IRRATIONALITY TENSION

The first two recurring tensions we discussed were each related to breadth: How broadly can we generalize the conclusions of our research, can they ever be universal, and how wide a lens should we adopt to study the spectrum from narrow intra-individual processes to broad sociocultural environments (and whether it is even useful to separate the two). The third tension assumes a narrow answer to both questions, as its protagonists have largely assumed that the psychological processes that they are interested in are universal, and that the context is not particularly relevant in understanding them. The crux of this third tension, which we label here the rationality tension, is whether we should be optimistic or pessimistic about human psychology (Zajonc, 1999). What we are calling the rationality tension is in fact a collection of related tensions as conceptions of

rationality have varied across time and domains. What unifies them is their focus on the capacity of humans to meet standards of thought, cognitive performance, or behavior that societal authorities have deemed superior to others. Examples include questions of whether one's behavior is guided by reason or by passion, whether reactions to others reflect critical evaluation or suggestibility, whether conclusions about facts derive from logical principles or from personal wishes, and whether perceptions are more or less accurate. Sometimes, researchers attempting to answer these questions have taken a prescriptive approach, explicitly using the term (ir)rationality, or writing in measurably optimistic or pessimistic terms (depending on their theories and findings). Other times they have attempted to remain more neutral, even cautioning readers to avoid prescriptive judgments, although often these cautions are ignored.

We begin by tracing the history of three longstanding rationality debates regarding the behavior of individuals: Crowd dynamics, suggestion and conformity, and reasoning biases. We then cover three more recent attempts to resolve rationality debates and unify the field: dual process models, reconceptualization of the appropriate standards for evaluation, and evolutionary approaches.

Crowds And Irrationality

Crowds and their apparent irrationality have fascinated observers since at least the time of the ancient Greeks. James Madison (Federalist 55, 1788) referred to the apparently inexplicable behavior of crowds when he famously observed that “Had every Athenian been a Socrates, every Athenian assembly would still have been a mob.” What attracted scholars' attention to the behavior of crowds, and what drove them to label it irrational, was that individuals in crowds seemed to behave in ways that differed from how they behaved alone, and from how society would want its members to behave. This idea has long been tied to the intuitive contrast between passions—what Kant (2006) called “illnesses of mind”—and reason, or what society cultivates within us. By arousing passions—our primitive animal nature—crowds were assumed to diminish reason.

One tradition of ideas that sought to describe and explain the effect of crowds on behavior traces back to a French theorist, Gustav Le Bon (1895/1960), who argued that crowds cause people to regress to a primitive, thoughtless, and uncivilized state. A subsequent tradition asserted that crowds influence people by inducing in them a state of “deindividuation” that renders them open to external influence which often will be anti-social but can also be pro-social.

Crowds As Agents Of Decivilization

Le Bon's (1895/1960) book *The Crowd: The Study of the Popular Mind* went through almost 50 editions and was translated into 16 languages (Nye, 1975). Gordon Allport (1954) described it as the most influential book in social psychology. Its immediate and enduring success may be owed to the intuitive resonance of Le Bon's thesis—that when in crowds, and especially under stress, people lose all capacity for reason. In Le Bon's native France, crowds had played a seemingly large role in the revolutions of the late 1700s and early 1800s, but his analysis extended beyond riots to consider also panic behavior more generally, including that occurring in emergencies and battle. Le Bon's account of the irrationality of crowds entailed the metaphysical concept of the group mind, and interwove elements of popular ideas at the time, including Tarde's imitation theory (Borch, 2013), Charcot's theory of hypnotic suggestion (Bogousslavsky, 2004; Goetz; 2016) and Durkheim's (1953) emphasis on the dominance of social or collective influences over the individual (Martindale, 1981).

Moreover, in addition to explanations of crowd behavior, his book offered a prescription for how to manage crowds, the essence of which was to assert discipline, authority, and control, and to persuade through affirmation, repetition, and contagion rather than reason (Bendersky, 2007). Moscovici (1981, cited in Reicher, 2004b) drew a link between Le Bon's theories and the mass politics of the 20th Century. Whether this point has merit or not, we know that Hitler, Mussolini, and Theodore Roosevelt, among many others, studied Le Bon (Nye, 1975).

Le Bon's account, as well as that of others who expressed distrust of the crowd, reflected long-standing assumptions that the most basic, primitive, ancestral component of the human psyche is irrational, but that it can be overridden by a civilized and rational, though precarious, and easily undermined, component. Numerous theorists argued that crowds exploit the precarity of the rational component and induce people to revert to their irrational primitive self, thus dissolving the existing social bonds that help reinforce civility (Freud, 1921/1985) and causing base impulses to supersede socialized group norms (Quarantelli, 1954, 2001). Le Bon (1895/1903) declared that "by the mere fact that he forms part of an organized crowd, a man descends several rungs in the ladder of civilization" (p. 36).

In addition to a general de-civilizing effect, Le Bon (1895/1960) thought that crowds produced a highly suggestible hypnotic-like state in people. Consequently, irrational behavior and ideas could spread through crowds as a disease would: "The first suggestion formulated implants itself immediately by a process of contagion in the brains of all assembled, and the identical bent of the crowd is immediately an accomplished fact" (39-40). The idea that crowds were a particularly susceptible channel for the uncritical transmission of irrational thoughts and fears was shared by other social psychologists of Le Bon's time (e.g., McDougall, 1920; Ross, 1920).

A curious aspect of Le Bon's legacy is that although his theories have been widely rejected—some would say debunked—his early critics rarely contested the underlying premise that people act irrationally in crowds. Notably, for instance, Allport (1924) mocked Le Bon's metaphysical concept of the group mind, but nonetheless shared his view that individuals' behavior regressed to a more primitive state in crowds and mobs. Rather than appeal to a group mind to explain this regression, Allport (1924) assumed that crowd members were simply expressing their inner truths: "The individual in the crowd behaves just as he would behave alone only more so" (p. 295). He put forward the behaviorist argument that crowds elicit the enduring response tendencies that people learned through their conditioning histories. Ergo, irrational collective behavior arises when a crowd is comprised of individuals who "owing to similarities of constitution, training, and common situations, are possessed of a similar character" (1924, p. 6). Allport argued that under such conditions, otherwise ordinary people could become prone to obeying verbal commands, automatically and without thinking, due to a history of learning the association between certain words and bodily movements.

If Le Bon's theories have fallen out of favor with academics, they still have considerable resonance with other segments of society such as the military (Bendersky, 2007) and the media. A recent example is the media's account of the so-called panic buying behavior that occurred in the early days of the COVID 19 pandemic (e.g., Ntontis et al., 2022). Despite the absence of empirical data, media reports regularly referred to such "panic buying" as due to "contagion of fear" and "herd behavior".

Deindividuation And Contextual Influences

Approximately fifty years after Le Bon's book, experimental social psychology, shorn of his and Allport's theories, found a new lens through which to explain apparently irrational individual behavior in crowds. In the paper that introduced the term for this new lens—deindividuation—Festinger, Pepitone, and Newcomb (1952) proposed that the internal controls of individuals are loosened in crowds due to feelings of anonymity. These authors supported this conjecture by demonstrating that male participants' willingness to express anti-social behavior, such as hostility toward their parents, increased with their feelings of anonymity. Subsequent studies also found that anonymity, particularly anonymity within a group, enhanced anti-social behavior (Cannavale et al., 1970; Singer, Brush, & Lublin, 1965). According to Zimbardo's (1969) influential analysis of deindividuation, anonymity leads to diminished self-observation and self-evaluation and, hence, to the weakening of controls based on guilt, shame, fear, and commitment, all of which lowered thresholds for exhibiting anti-social behavior. The concept of deindividuation shares with Le Bon and Allport's conceptions the assumption that crowds weaken the sway of internalized social standards for behavior (i.e., civilized standards for behavior). However deindividuation researchers posited very different (and better specified) mechanisms for this transformation.

True to the social psychological tradition of contrarianism (or even-handedness, depending on one's point of view), researchers soon demonstrated that deindividuated conditions could lead not only to anti-social behaviors, but also to prosocial ones (Diener, 1979; Johnson & Downing, 1979). Equally predictably, they began to dig deeper into potential mediating processes, such as the diminished self-awareness that anonymity supposedly produces (Diener et al., 1975; Diener, 1976; Duval & Wicklund, 1972). In combination, these findings led to the conclusion that crowds increase people's outward focus and decrease their inner focus (Prentice-Dunn & Rogers, 1982). Consequently, people are less able to retrieve both personal and internalized societal standards and become increasingly influenced by environmental stimuli. When these stimuli are anti-social people become more anti-social; when these stimuli are pro-social people become more pro-social. This notion also bears a resemblance to Le Bon's and Allport's ideas that crowds increase people's vulnerability to suggestion. One distinguishing feature of the deindividuation account, relative to those earlier ideas, is that it does not rely on the assumption of a regression to a primitive mind or self; rather, it explains crowd behavior through cognitive mechanisms in the modern mind.

Rationalist Counterarguments

Despite their more modern cast on crowd behavior, deindividuation researchers largely shared Le Bon's position that crowds produce irrational behavior. Both traditions agree that members of crowds do not *choose* their actions thoughtfully and deliberately, but rather impulsively and automatically. In terms of the actions crowd members ultimately choose, Le Bon linked the irrationality of crowds with anti-social behavior; deindividuation researchers linked it with greater sensitivity to contextual influence which could increase pro-social as well as anti-social behavior. In any case, these claims eventually provoked two separate forms of the kind of not-so-fast backlash that often occurs in response to theories positing human irrationality.

Challenges to irrational outcomes: the irrationality of crowds' behavior is vastly exaggerated

The first challenge focused on the claim of outcome-level irrationality, and disputed the idea that chaos and antisocial behavior was truly ubiquitous in groups. For example, reviews of available

data indicated that emergencies do not lead to panics, or crowds to mobs nearly as frequently as Le Bon and his successors claimed (Aguirre, Torres, Gill, & Hotchkiss, 2011; Donald & Canter, 1990; Drury, 2018; Quarantelli, 2001).

Moreover, rationalists pointed out that stories about irrational crowds were often exaggerated before becoming immortalized in public discourse. For example, consider the famous "Tulip Bubble" that supposedly occurred in Holland in 1600s, in which tulips purportedly sold for as much as five times the average person's annual salary. The historian Goldgar (2008) argues that the "Tulip Bubble" was not at all a mania: A few people paid very high prices for a few very rare bulbs, and some lost substantial amounts of money as well, but these buyers and sellers were a small group of people connected in ways other than their tulip passions, and the market was quite like other markets (e.g., the market for art). According to Goldgar (2008), the Tulip Bubble was incorporated into the public discourse as a moral lesson that greed is undesirable and chasing prices can be dangerous. Apocryphal though it may be, it has become a cautionary tale about markets, invoked as a reminder that what goes up must go down.

Similarly, some scholars have speculated that the belief in the psychology of mass panics and crowds has persisted without strong empirical evidence of actual panic behavior because the irrationality-of-crowds discourse serves ulterior societal motives or political agendas. For example, portraying crowds as susceptible to antisocial violence delegitimizes forms of social disorder such as protests and demonstrations and justifies oppressive crowd management strategies that protect society from the supposed excesses of "panicking" crowds (Drury, 2002; Van Dijk, 1993).

Challenges to irrational outcomes and processes

The second challenge to the claim of crowds' irrationality targeted both the outcome and the process levels. Those levying this challenge accepted the premise that crowds often behaved in ways that contravened some abstract and generally applicable *societal* standard for correctness but argued that this was not the relevant standard for rationality. They argued that crowd members' behavior might in fact be quite rational if we consider its alignment with a more relevant subjective standard. This argument took three main forms.

The first source was game theorists, who frequently seek to show that seemingly irrational behavior is in fact utility-matching—that is, it resulted from a systematic tendency for actors to choose actions that will increase their well-being. The case of crowd irrationality offered them such an opportunity. For example, Berk (1974) proposed a "rational calculus" model of crowd action that involved five steps whereby crowd members (1) collect information, (2) use this information to predict possible events, (3) generate a list of possible actions, (4) order these possible actions as a function of their probable outcomes, and finally (5) choose the top-ranked action—that is, the one that will minimize their costs and maximize their rewards. The role of crowds in this model is to influence the probable outcomes: Actions that the actor thinks the crowd will reward should become more likely. A similar perspective is the idea that observing the behavior of the majority is an adaptive guide for how one should behave (Boyd & Richerson, 2005; Gigerenzer, 2015). Taken together, these perspectives suggest that 1) individuals use similarly logical and perhaps even deliberative processes in and out of crowds, 2) crowds serve as one of many inputs to these processes, and 3) the outcomes of these processes ultimately help individuals maximize utility or even survive.

A second and related source of ideas that challenged the irrationality of the processes and outcomes of crowd behavior were sociological theories that emphasized social norms. The most influential of these was Turner and Killian's (1957) emergent norm theory, which held that crowd behavior was structured by shared understandings of appropriate conduct that specified limits to behavior. It disputed the idea that anti-social or any other behavior spread through crowds in a "spiral of contagion." Rather, it suggested that crowds influence behavior through a shared sense-making process that develops through interpersonal interaction (see also Sherif, 1936). The thrust of this analysis, like that of the game theory analysis, is that although crowds may influence their members, it is misguided to call the form of this influence irrational, because it operates through ordinary channels that encourage rather than discourage adherence to social norms.

As a final and related example, social psychologist Stephen Reicher and colleagues challenged the irrational account of crowd behavior by means of a social identity-based analysis (e.g., Drury et al., 2020, Hopkins et al., 2019, Reicher 1996). Whereas deindividuation theorists proposed that people *lost* their identities in the anonymity afforded by crowds, these theorists proposed that people *switched* from an individual to a group identity, or from one group identity to another. Their *social identity model of deindividuation* denied that deindividuated people act irrationally at the whim of environmental cues and suggested instead that they act in ways that match the norms of their identity group, whether or not that group is a crowd. This model later evolved into the *elaborated social identity model* of crowd behavior (Drury & Reicher, 2000; Reicher, 1996; Stott & Reicher, 1998). There is now considerable evidence consistent with the ideas of Reicher and the sociologists that group immersion and anonymity in the group do enhance depersonalization and hence conformity to the group's perceived norms (see Spears, 2017).

The Irrationality Of Crowds: An Area Of Dwindling Interest

Explanations and descriptions of crowd behavior have been a staple of meditations on human nature for millennia. The social psychology of crowds is a central topic in the eyes of many, and the claim to have a serious theory of human nature virtually required the theorist to explain the supposed chasm between the behavior of individuals and crowds. The social psychology of crowds was a central topic for early non-experimental social psychologists and continued to command attention during the field's early experimental period. However, the battle grounds for rationality debates among psychologists are no longer the relative powers of reason versus passion, and studies of crowds now focus much more on which factors affect crowd behavior without regard to whether this influence results in rational or irrational processes and outcomes (Hegselmann & Krause, 2002; Latané, 1981; Latané & Bourgois, 1996; Lorenz, Neumann, & Schroder, 2021).

Conformity: Suggestion, Prestige, And The Voice Of The Herd

Starting in the early 20th century, social scientists and the public shifted their attention from crowds to propaganda and mass advertising. There was great public fear of their effects on society and democracy, and the concept of suggestion--the uncritical acceptance of the opinion of another--was believed to be at the heart of both ills (Parsons, 2021). The popularity of this concept is difficult to appreciate. According to Gordon Allport's (1968) description of the field at the beginning of the last century, "nearly all the problems in social psychology became formulated both for scientists and for laymen in terms of suggestion" (p. 33). Asch (1948, p. 251) also acknowledged the centrality of the concept:

[T]he doctrine of suggestion, originally oriented to the formation of social misconceptions, has been given a general application, and has become the theory of the formation and change of opinions and attitudes. In consequence the psychology of attitudes is well-nigh universally (both in social psychology and in the social sciences generally) treated as at bottom an affair of suggestion and bias. This approach has penetrated nearly all regions of social psychology.

The psychology of suggestion was the primary explanation of, and tool for, consumer influence (Kuna, 1976). Initially, it was defined in contrast to critical reasoning—that is, it was considered squarely irrational. Subsequently, new perspectives and shifting definitions of rationality called this definition into question.

Suggestion As Irrational

Theorists who agreed suggestion was irrational embraced diverse definitions of the term. To McDougall (1909), “Suggestion is the process of communication resulting in the acceptance with conviction of the communicated proposition in the absence of logically adequate grounds for its acceptance” (p. 97). He claimed that suggestion resulted from the “subjection” instinct and was associated with an innate deference to authority or prestige. Le Bon (1895/1903) observed, “Whatever has been a ruling power in the world, whether it be ideas or men, has in the main enforced its authority by means of that irresistible force expressed by the word ‘prestige’” (p. 147). Both these authors describe suggestion as a primitive, irrational, psychological instinct of deferring to supposed authority.

Allport (1924) rejected this conception of suggestion, and instead described it as a behavioristic process or a form of conditioned response. He argued that learned responses are different from instincts in that they are acquired by individuals precisely *because* they are rational in their context, although they can become irrational if over-generalized. Evolutionary psychologists would later make the point that in fact the same is true of instincts, but with context being defined at the level of species rather than of individuals. We return to this idea later.

Allport (1924) also linked suggestibility to dependence on the opinions of others, or what he called the “voice of the herd”. He proposed that when individuals develop an “impression of universality” or, in more modern terms, when they perceive consensus, they become more submissive, and therefore more suggestible. As with conformity to norms, a reliance on the wisdom of crowds may not necessarily seem to modern scholars to be evidence of irrationality. However, for many philosophers, most notably Kant, independent judgment was a hallmark of rationality. Likewise, Kohlberg’s (1971) influential theory of moral development described reliance on others for moral judgment as a developmentally less advanced stage.

Could Suggestion Be Rational?

The concept of suggestibility lost popularity in the middle of the last century. The Hovland attitude group, for example, virtually never used the term, substituting the term “persuasion” for it (despite often reporting so-called “source effects” that bore a strong resemblance to what had previously been called prestige suggestion effects (Hovland & Weiss, 1951). The cognitive revolution replaced earlier conceptions of information processing with rationalist approaches, such as the theory of reasoned action (Ajzen & Schmidt, 2020). No longer were people seen as “captive, attentive and

gullible, and virtually 'helpless victims' in the hands of unscrupulous propagandists and advertisers" (Petty et al., 2009, p. 126).

A meta-scientific reason for this shift may have been the growing confidence in the strength of democracy and the rationality of the individual that began following the Allied victory in the Second World War (e.g., Allport, 1945; Cantril, 1958). Optimism about human nature was growing, at least among social scientists, and those who shared that sentiment—proponents of rationalist social-psychological models—sought to distance themselves from irrational phenomena such as suggestion, with its associations to instinct and its connotative association with trances and hypnotic states (Parsons, 2021). As Christian Borch (2012, p. 17) put it: "The reference to suggestion, with its underlying emphasis on features that countered rather directly the ideal of the constituent subject, was hard to digest for liberal sociologists who launched a vendetta against the concept, eventually rendering it sociologically inappropriate."

The growing optimism regarding the rationality of people may have had distal roots in sociocultural shifts around the end of the Second World War, but it also had a more proximal foundation in the waning of instinct theory and behaviorism, which left suggestion without a plausible basis (Jackson, 1988; Parsons, 2021). The field simultaneously saw the emergence of gestalt psychology, whose major architects (Köhler, Koffka, and Wertheimer) derived their principles from what they saw as the human tendency to find order in disorder. According to this perspective, the mind follows a predictable set of principles to make sense of stimuli captured by the eyes. As a result of this logical, systematic sense-making process, individuals can perceive uniform shapes and movement rather than simply collections of unconnected images. This depiction of the human mind portrays it as rational in the sense that it adheres strictly to a set of principles.

The emergence of gestalt psychology popularized the broader presumption that humans were rational, or that logical sense could be made of their behavior. Against this backdrop, Solomon Asch (1948) famously reinterpreted studies of the supposed irrationality of prestige suggestion, portraying the participants as following a thoughtful and deliberative process rather than an impulse or an instinct. In one early investigation that Asch subsequently reinterpreted, Lorge and Curtiss (1936) exposed American college students to statements such as "A little rebellion now and then is a good thing, and as necessary in the political world as storms are in the physical" and manipulated the alleged source of the statement. Lorge found that participants were much more likely to agree with the statement when it was attributed to Thomas Jefferson, incidentally the statement's true author, than when it was attributed to Vladimir Lenin, a Russian revolutionary. Lorge interpreted the results in terms of learning theory principles: "The shifts in evaluation placed on the quotations can be viewed as a function of the confirming reactions aroused by the esteem for the source." According to this interpretation, participants had learned to esteem Jefferson more—to grant him more prestige—than Lenin, and with those associations in place they mindlessly accepted the statement more when they thought Jefferson made it.

Asch (1948) agreed with Lorge that the difference in prestige between the two sources was probably key to his finding but challenged the irrationality of its impact. Rather than assuming that prestige led to blind acceptance, Asch proposed that participants' pre-existing knowledge caused them to imbue the statements with different meaning depending on the source. "A little rebellion" conjured a very different concrete image when Lenin as opposed to Jefferson was its advocate, and why shouldn't it? In Asch's famous phrase, what changes is the object of judgment, not the judgment of the object.

Another rationalist reinterpretation of people's tendency to conform to the opinions of groups (see Jahoda, 1959) came from Crutchfield's observations using his eponymously named Crutchfield (1955) apparatus—a set of cubicles that participants could sit in, with each cubicle displaying responses purported to come from other participants but in fact chosen by the experimenter to manipulate group norms and test conformity. Noting that his subjects showed much greater conformity to groups' views of objective reality (such as which among a set of lines was the longest) than to groups' views of subjective preferences (such as which among a set of lines one preferred), Crutchfield (1955, p. 198) concluded:

[...] conformity behavior under such group pressure, rather than being sheerly an indiscriminate and irrational tendency to defer to the authority of the group, has in it important rational elements. There is something of a reasonable differentiation made by the individual in his manner of reliance upon the group.

In other words, people do not conform to groups' judgments on merely any matter. Tastes can reasonably vary and, therefore, people do not by default adopt those of other group members. They only conform on matters of objective reality. When only one answer can be correct, and they find themselves in the minority, they assume, not unreasonably, that they are probably wrong.

Bias And Irrationality: A Primary Emphasis On The Rationality Of Processes

Both the original demonstrations of irrational suggestion and their subsequent rationalist reinterpretations shared the premise that irrationality was the result of *less thought*: Of the relative absence of cognitive processing, and of people abandoning reason to regress to a more primitive state driven by impulses and passions. They simply disagreed about whether suggestion resulted from this version of irrationality. In their wake, a different conception of human irrationality took center stage. Rather than referring to an overall decrease in thinking, irrationality became associated with systematic, directional bias that could infect thinking due to people's motives. To put it another way, the field replaced its previously *biological* conception of irrationality, wherein reason was disrupted or swamped by emotions or passions, with a *motivational* conception wherein reason was harnessed or directed by these forces.

The motivational conception of irrationality was not new and had waited patiently in the wings for centuries. It is difficult to identify a famous philosopher who did not think that the will was capable of coopting reason (e.g., Descartes, Machiavelli, Nietzsche, Schopenhauer). Francis Bacon (1620/1994) put it this way "The human understanding is not a dry light, but is infused by desire and emotion which gives rise to wishful science. For man prefers to believe what he wants to be true... In short, emotion in numerous, often imperceptible ways, pervades and affects understanding. Reason is a slave to passion."

Freud's theories, with his three layers of personality (id, ego, and superego) built one of the bridges that led from biological to motivational irrationality. He spoke of the capacity of the *ego* to defend the wishes and impulses of the *id* by means of defense mechanisms, such as rationalization. Although social psychology ultimately rejected the trio of Freudian homunculi, it embraced the idea that motives or needs could coopt human thought and action.

Demonstrating Apparently Motivated Bias

Studies soon appeared that demonstrated the capacity of people's motivations to direct their judgment and decision-making processes. The first generation of this research adopted a metaphor that has been termed the totalitarian ego (Greenwald, 1980): The self was presumed to have certain needs and sophisticated mental processes ensured that these needs were met. The research in this tradition often aimed to catalogue the needs of the totalitarian ego by demonstrating each need's capacity to coopt reasoning. In so doing they followed Freud's steam engine, or hydraulic, model. Need-based motivation ("psychic energy") was conceptualized as if it were steam compressed within a chamber; bottle-up too much energy and tension, and it will explode, taking over the direction of thought processes. The needs thus documented included those for self-esteem (Pyszczynski & Greenberg, 1987; Sedikides & Gregg, 2008), the derivative needs to see oneself as moral and competent (Pittman, 1998; Tappin & McKay, 2017), and needs associated with the functioning of the world and the capacity to successfully navigate it, such as the need to believe that the world is just (Lerner, 1980).

Cognitive dissonance theory offers a famous example of a psychological need: The need for cognitive consonance, or harmony between one's attitudes, beliefs, and behaviors. Threats to this need result in the aversive experience of cognitive dissonance. As in any drive state, the individual experiencing cognitive dissonance is motivated to restore psychological equilibrium. Although dissonance presumably can be created by any inconsistency in cognitions—for example, Festinger's (1957) example of standing in the rain and not getting wet—it seems particularly likely to arise when a cognition is dissonant with the belief that one is a competent and moral person (Aronson, 1969). In this way, dissonance arousal can be cast as a form of the need for self-esteem (Pyszczynski & Greenberg, 1987).

What made the need for cognitive consonance seem so compelling was the demonstration that it could produce a reaction that seemed on its face to be clearly irrational: Changing one's prior attitude to bring it in line with behavior. If the famous Festinger and Carlsmith (1959) study had found that lying to another participant for \$1 (as opposed to \$20) made people simply excuse their lie—for example, by saying it was no big deal, or that they lied because the experimenter was a nice guy—it would not have seemed so impressive. However, because people changed their perception of how much they liked the task, thereby making the lie less of a lie, it seemed a step beyond merely minimizing the lie's seriousness. Many of the classic dissonance studies had this quality: They caught people in the act of changing their attitude to avoid looking foolish or immoral.

Traditionally, researchers sought to establish the existence of a need or motive by following a three-pronged strategy (for reviews see Pittman, 1998; Pyszczynski & Greenberg, 1987). First, they would demonstrate that the pattern of cognitions presumed to be motivated by the need state is positively associated with psychological well-being (Gregory et al., 2011). For example, if the need for a belief in a just world was hypothesized to lead people to derogate the character of people who otherwise would seem to be suffering unjustly, researchers might demonstrate that participants who do derogate the character of undeserving victims are less distressed by their suffering (Hafer & Olson, 1989; Lerner & Miller, 1978). Second, researchers would show that increasing the drive state increases the incidence of the cognitions presumed to be motivated by the need in question (e.g., McFarland & Ross, 1982). For example, they might have participants witness or experience a clear-cut injustice and show that later in an unrelated setting they do more derogating of undeserving victims than do control participants (Lerner & Miller, 1978). Finally, researchers would establish that exogenous factors that reduce the drive state decrease the incidence of the putatively motivated

cognitions (Greenwald & Renis, 1978; Steele & Liu, 1983). For example, they might provide participants with evidence reassuring them that the world is fair overall, and show that in this condition, they did less derogating of undeserving victims.

In summary, the key to the steam-engine or hydraulic metaphor was the idea that as motivation associated with a drive-state built up, it sought release, which could be achieved through certain thoughts or behaviors. The build-up of frustration from self-esteem threats, for example, could be let out, and equilibrium restored, by directing aggression or prejudiced sentiment toward outgroups (Berkowitz, 1989). Research along these lines was interpreted as demonstrating irrational—meaning here biased by motivation—processes and outcomes. The process was assumed to be one in which motives coopted reasoning; moreover, many assumed this process was self-deceptively hidden leaving people unaware that their reasoning had been corrupted (see Ehrlinger & Dunning, 2003). The outcomes of these processes, or the beliefs that resulted from them were necessarily irrational by this definition: The participant's judgment of the self or other differed from what it would have been in the absence of motivation. Eventually, however, this view and this definition of (ir)rationality would be challenged, evoking ideas that harkened back to William James (1907/1975): Can behavior or thought legitimately be called irrational if it can be shown to benefit mental well-being (Sturm, 2012)?

Motivated Reasoning And The Computer Metaphor

The motivated bias cottage industry was disrupted in the 1960s when cybernetics came on the scene, and computers replaced steam engines as the prevailing metaphor for the mind. By the 1980s, the metaphor of the “mind-as-computer” was fully embraced by the emerging field of cognitive psychology, and it continues to dominate thinking today. The metaphor portrays the brain as hardware and the mind as software that is fundamentally concerned with information processing: memory storage and retrieval, hypothesis testing, and inference. A focus on the underlying computer metaphor led to a different approach to cognitive errors and biases. This new approach did not differ from the old one in its characterization of outcomes: The new approach continued to assume that certain beliefs or behaviors were biased or erroneous. Its new insight was to question whether these outcomes necessarily arose from “hot” motivational processes. Instead, it argued they might be costs incurred by the very “cold” cognitive processes that most often led to unbiased and accurate outcomes. The mind-as-computer metaphor inspired the view of the mind as a cognitive miser (Wilson & Brekke, 1994) that strived to maximize efficiency and conserve resources, at the expense of occasional inaccuracy.

This shift in perspective on why the mind makes mistakes sparked a second generation of bias research dedicated to attempts to demonstrate that effects previously assumed to reflect motivation instead arose through ordinary cognitive pathways (Miller & Ross, 1975). An example was Bem's (1972) famous reinterpretation of cognitive dissonance effects. Bem disagreed that participants in dissonance experiments were changing their attitudes to reduce an aversive motivational state produced by disharmonious cognitions. Instead, he proposed they were merely inferring their own attitudes from their behavior, as they would infer those of another person: “If I agreed to tell the next participant in a study that it was fun in exchange for one measly dollar, I must have thought it was fun”.

Another example of a cognitive reinterpretation of bias was Hamilton and Gifford's (1976) work on illusory correlations: Perceiving a relationship among people, events, or behaviors even when no such association exists. The motivational view held that illusory correlations resulted from people's

desire to see certain other groups in a positive or negative way. Hamilton and Gifford offered a competing cognitive account: Rare or novel occurrences are more salient and therefore tend to capture people's attention which causes them to overestimate the frequency with which they have observed the combination of two statistically unusual events (e.g., a member of a minority group and an unusual behavior such as violence).

A third example was social projection or the false consensus effect (Ross, Green & House, 1977). The false consensus effect refers to the tendency to see one's behavioral choices and judgments as more common than they are, or to exaggerate how relatively widespread one's own personal qualities, characteristics, beliefs, and actions are in the general population. Numerous explanations have been proposed to explain false consensus (Marks & Miller, 1987). First, echoing Freud's ideas about defense mechanisms, motivated accounts asserted that people assume others share their beliefs as a means of fulfilling their need to see their beliefs as valid and thus maintain their self-esteem. According to Holmes (1968), social projection represents people's "attempt to validate their beliefs by projecting their own characteristics onto other individuals."

Subsequently, a series of cognitive counter-explanations proposed that false consensus errors derive from mental processes that may be flawed but are not motivated. One such account posits that people's estimates of the population-level prevalence of a given belief will be biased by their selective exposure to samples consisting primarily of similar others. Another account argues that people's own view may be more salient to them than different views, and this salience causes them to overestimate the prevalence of their own view. Yet another claim is that false consensus arises from a basic tendency to adjust insufficiently from anchors: If people use the self as an anchor, they may insufficiently adjust for the knowledge of others' differences of opinion (Epley, Keysar, Van Boven, & Gilovich, 2004; Kruger, 1999).

Finally, a radically different explanation posited that false consensus errors derive from cognitive processes that are neither motivated *not* inherently flawed, but rather practically functional. This idea is akin to Bem's alternative account of cognitive dissonance, which pointed out that most of the time it is reasonable to look to one's behavior as an indicator of one's true attitudes. In the case of false consensus errors, the argument points to Bayesian reasoning: If one has no information aside from one's own belief, it is rational from a Bayesian perspective to assume that one is in the majority because, by definition, most people in any given social environment will be part of the majority (Dawes, 1989; Dawes & Mulford, 1996; Galesic, Olsson, & Rieskamp, 2018; Tarantola, Kumaran, Dayan, & De Martino, 2017; Vanberg, 2019). For example, if most people prefer dogs to cats, then most people will be correct if they assume that most others share their position: Most will assume (correctly) that, like them, people generally prefer dogs to cats, and only a minority will assume (incorrectly) that, like them, people generally prefer cats to dogs.

Compiling these findings along with numerous others, Nisbett and Ross's (1980) seminal book *Human Inference: Strategies and Shortcomings of Human Judgment*, promoted the image of humans as "intuitive scientists" whose inferential errors often arise not from their motivational needs, and indeed not even always from their cognitive limitations, but sometimes from their cognitive strengths. It also expanded the scope of the debate by introducing the field of social psychology to the pioneering work of Kahneman and Tversky (e.g., Kahneman & Tversky 1982; Tversky & Kahneman, 1974; 1982) on heuristic-based biases. Perhaps more than any other area of research, this work on heuristics was responsible for popularizing the idea that inferential errors could serve as windows into the mechanics underlying the reasonably accurate inferences that people routinely make.

Reconciling Motivation With Cognition

The claim that many inferential errors and biases have little to do with human motivations and needs provoked some resistance in the field and prompted multiple efforts to reestablish the role of motivation (McFarland & Ross, 1982; Miller, 1976). Nevertheless, Tetlock and Levi (1982) concluded that the cognition-motivation debate was unresolvable as it would never be possible to state with confidence that a particular error was due to motivational rather than a cognitive source. For example, manipulating a need state will also have cognitive consequences, therefore, it will be impossible to know whether the observed effects have a hot or cold origin. Accordingly, instead of trying to adjudicate between these two opposing camps, the field endeavored to integrate them, thereby leading to proposals about the interaction of motivational and cognitive processes in the production of errors and biases. These proposals took two related forms.

Motivational director, cognitive executor

Some prominent models adopted the premise of the intuitive-scientist—that individuals collect and analyze information without allowing emotional preferences to influence these cognitive processes directly (Kunda, 1990; Pyszczynski & Greenberg, 1987)—and proposed the metaphor of the motivated tactician (Fiske & Taylor, 1991). These models, emerging in the 1980s, suggested that once people set directional goals, they can launch cognitive processing on a path that is likely to achieve those goals. For example, if someone adopts a hypothesis based on wishful thinking (e.g., my child is smart), the intuitive scientist's ordinary and typically effective way of testing that hypothesis, which is to seek evidence that confirms it (Nickerson, 1988), will favor the confirmation of the desired conclusion even if it is inaccurate. By this account, "hot" directional goals can mimic "cold" expectancies in what is otherwise the same hypothesis-confirmation process.

This integrated view of motivation and cognition suggested an explanation for how people maintain an illusion of objectivity even while arriving at conclusions that are systematically biased in the direction they prefer (Heider, 1958; Kunda, 1990; Pyszczynski & Greenberg, 1987). They can search their memory for beliefs and rules that support their desired conclusion, creatively combine accessed knowledge to construct new beliefs that produce logical support for the desired conclusion, and so on. Moreover, they can do so while preserving the illusion of objectivity, because it feels to them as though they are merely testing a hypothesis. They do not recognize that they are accessing only a subset of the relevant facts and would probably access different beliefs and rules in the presence of different directional goals. They might even be capable of justifying opposite conclusions on different occasions.

Dual process models

A second set of models growing out of debates over rationality and motivation split the difference by positing that the brain has two distinct ways of processing information. Typically referred to as dual-process theories of information processing, these models quickly became popular in social psychology and in psychology generally (Chaiken & Trope, 1999; Gawronski, Luke & Creighton, 2021). The term *process* refers to the mental operations that transform environmental inputs into behavioral outputs (De Houwer, 2011; Gawronski & Bodenhausen, 2015; Marr, 1982); *dual* refers to the idea that there are two (sets of) qualitatively distinct processes to explain behavior (Gawronski, Sherman, & Trope, 2014; Moors, 2014).

An early dual process model, Petty and Cacioppo's (1986; 2018) *Elaboration Likelihood Model* (ELM), was an attempt not to reconcile motivation with cognition, but rather to reconcile rational (thoughtful) with irrational (unthoughtful) responses to persuasive attempts. Nevertheless, we include it here as it was one of the earliest theories to posit split modes of processing. The ELM labeled one route central and described it as "very rational": An individual closely considered arguments or information relevant to a persuasive appeal and came to a reasoned conclusion. It labeled a second route peripheral and described it as "not a very thoughtful one" (p. 256). On this route, people eschewed thoughtful deliberation in favor of more reflexive or automatic decision-making criteria.

Many different versions of dual process models have emerged whose scope goes beyond the dynamics of persuasion. What these models have in common—with each other but also to a degree with the ELM—is that they assume two processes, one that is assumed to operate in an automatic fashion, and the other, to function in a controlled fashion (Gawronski, Luke, & Creighton, 2021; Moors, 2014). Most models assume that the influence of motivation is primarily located in the former system, and that the latter, with its characteristically deliberative thinking style, cannot be used simply to select the beliefs that one prefers. Stanovich (1999) termed these two sets of processes System 1 and System 2, a distinction that Kahneman (2011) popularized in his book, *Thinking, Fast and Slow*.

Few of these models explicitly characterize their two sets of processes as rational and irrational, eschewing these terms and their normative connotations in favor of measurable attributes like whether the processing speed is fast or slow. An example is Strack and Deutsch's (2004) reflective-impulsive model that proposes two simultaneously operating systems of information processing, designated as the *reflective system* and the *impulsive system*. Nonetheless, the characteristic features distinguishing the two systems—thoughtfulness, deliberation, and vulnerability to the influence of motivation—are key to how the field has variously conceived of rational processes: Processes that are thoughtful, deliberate, and/or non-motivated could all be described as more rational than those that are not. We will return later to the rationality of the outcomes these dual processes produce.

Most work on dual-processes seeks to show how the two systems interact—for example, under what circumstances is only one process engaged, when does one dominate over the other in determining judgment or behavior (Gawronski, Luke & Creighton, 2021; Zajonc, 1980), or in what ways does one process use the other's outcome as an input to which it applies a correction (Gilbert, 1998). For instance, Haidt's (2001) *Social Intuitionist Model* proposes that moral judgments are "the automatic output of an underlying, largely unconscious set of interlinked moral concepts" (p. 1040), arising from the faster, less thoughtful set of processes, which are highly automatic and affectively charged, and operate unintentionally as well as outside of conscious awareness. These processes are moral intuitions, shaped by culture, and involve dimensions such as care-harm and loyalty-betrayal (Graham et al., 2013). The role of the slower, more deliberate set of processes is what we call moral reasoning, which Haidt describes as largely defending or justifying one's moral intuitions. In essence, this model describes moral judgments as almost exclusively determined by the faster, less thoughtful set of processes, and as appearing in the mind, like visual perceptions, without awareness of the processes that produce them. In this respect it stands in sharp contrast to Kohlberg's (1971) model of moral judgment, derived from Kant, that emphasizes the cognitive basis of moral judgment and portrays moral affect as following from rather than producing moral reasoning.

Motivation Versus Cognition Today

The debate between motivation and cognition remains active. There continue to be studies that seek to document that a phenomenon previously thought to be due to motivation is in fact a cognitive by-product. Consider self-enhancement effects, a sphere where motivational accounts have traditionally dominated cognitive ones (Alicke & Sedikides, 2011; Brown, 1986; 2012; Dunning, Leuenberger, & Sherman, 1995; Sedikides & Gregg, 2008; Wakeman, Moore, & Gino, 2019). The motivational view of self-enhancement highlights the desire to see the self in a positive light (Serra & DeMarree, 2016) because to do so feels better than the alternative (Alicke & Sedikides, 2011; Dufner, Gebauer, Sedikides, & Denissen, 2019; Tappin & McKay, 2017; Taylor & Brown, 1988). Recent work questions this view and suggests that certain cognitive-perceptual processes are sufficient to create overly positive self-images (Chambers & Windschitl, 2004; Galesic et al., 2018; Harris & Hahn, 2011; Heck & Krueger, 2015; Krueger, Freestone, & MacInnis, 2013). Most individuals observe more of their own behaviors than those of others, and most social contexts elicit more positive than negative behaviors; as a result, the person that most of us see engage in the highest number of positive behaviors is ourselves (Fiedler & Kutzner, 2015).

As another example, consider the hindsight bias: It seems intuitive that this bias occurs because people, when they learn something new, *want* to think that they always knew it and that their prior judgments reflected such knowledge. However, recent models suggest that it emerges as a by-product of ordinary cognition. When a person learns something new, they update their knowledge base. Then, in their effort to reconstruct their prior judgment (which is how all long-term memories are produced), they base that reconstruction on their updated knowledge base (Hoffrage, Hertwig, & Gigerenzer, 2000). According to this account, we have a hindsight bias not because we wish we had always been as smart and knowledgeable as we are today, but solely because of how memory works (Calvillo, 2012; Coolin et al., 2015).

These and other examples notwithstanding, the debate between motivation and cognition is no longer as central to the field as it once was, as most researchers found that they could live with the overarching compromise position that the two coexist and are responsible for different mental processes. It is now well-established that motives, preferences, and goals direct (or bias) information processing including attribution, attention, evaluation of evidence, information search, recall and knowledge activation, and the organization of information in memory (Molden & Higgins, 2012). At the same time, human beings appear constrained by reality and a commitment to being rational (illusion of objectivity) in their processing of information: According to a meta-analysis (Hart et al., 2009), people show a preference for high quality information, but also one for motivationally consistent information.

Dual process models, perhaps less popular today than they once were, remain influential. What was revolutionary about the models was the idea that two distinct modes of processing co-existed within the brain, they could operate in parallel or in coordinated sequence, and whether one won out over the other depended on a variety of factors. Another contribution of these models is they explicitly separated the rationality of process from that of outcome, generally concluding that neither of the two processes is guaranteed to provide an objectively good or bad outcome for a particular person.

This conclusion and its emphasis on what are good *for the person* ultimately led researchers to acknowledge the distinction between accuracy and functionality as standards from which to evaluate an outcome. From one perspective, we might deem a false belief to be an irrational

outcome—would it not always be better to know the truth? However, from another point of view, held as far back as William James (1907/1975), if that false belief helps someone thrive, it is difficult to say they should not hold it (Swann, 1986). An example is the overconfidence bias among children. It might seem dysfunctional, and certainly inaccurate, to hold an inflated estimate of one's own capacities, as the effect of this bias is that the child will take on tasks for which they are not prepared. However, Bjorklund and colleagues (1997) make a compelling counterargument: Children have extremely limited skills, so accurate self-appraisals might prevent them from ever trying anything new. This would deprive them of crucial opportunities to learn the very skills they lack. Presumably similar reasoning could apply to adults, most of whom have more skills than children but can still benefit from learning to an extent that outweighs the costs.

Newer Arguments For The Rationality Of Outcomes

As attention increasingly turned to outcomes in addition to processes, the new question that came into focus was whether the inaccuracies found in social perception were necessarily irrational (Cushman, 2021; Hahn & Harris, 2014). Three arguments emerged that answered three different versions of this question, with each offering its own standard for judging rationality.

Bias Versus Accuracy In Perception

The first, and perhaps tamest, criticism of bias research came in the form of studies that document unexpectedly high accuracy in some kinds of social perception. One prominent example of this line of research was that of Nalini Ambady and her colleagues (Ambady et al., 2000) on the accuracy of thin slice judgments—ones rendered on a very small amount of information. In one demonstration, Ambady and colleagues (1999) found that participants could judge the self-reported sexual orientation of targets with 70% accuracy from a mere 10-second silent videoclip, with self-reported gay and lesbian participants outperforming heterosexual participants as the slices of information provided got even thinner. In another study, participants were accurate in detecting pathological personality traits from 30-second videoclips of targets describing themselves (Friedman et. al., 2007).

These lines of research emphasize outcome over process. Rather than comment on the way people make thin slice judgments, they simply demonstrate that the judgments themselves are accurate, which one could say means they are rational. Nevertheless, controversy surrounds the source of that accuracy. One possibility is that people quickly pick up on cues that are genuine indicators of a certain type of person; if so, this would be a process one might label more rational. Another is it that certain cues reliably elicit certain (initially inaccurate) judgments and that a lifetime of being on the receiving end of this turns them into reality. This form of a self-fulfilling prophecy (e.g., Hong, Suk, Choi, & Na, 2021; Olivola & Todorov, 2010) seems rather less desirable. Ultimately the process by which thin slice judgments attain their accuracy may depend on the trait in question.

Thin slicing and other research documenting how person perception aligns with objective measurement (see, for example, Todorov, Mandisodza, Goren & Hall, 2005) certainly does not deny that bias can occur, nor does it explicitly position itself as a challenge or even a counterpoint to bias work. Its point of tension with this work is, therefore, a matter of focus: Rather than emphasize errors and whether they are systematically biased in one direction, some accuracy researchers chose instead to emphasize the overall and often surprising degree of accuracy in human

perception. The existence of this type of accuracy research alongside bias research (see Gilbert, 1998) demonstrates that one's view of humans' capabilities depends on the standard against which one compares them: If one's standard is perfection, humans will almost always fall short; if one's standard is what would be expected by chance alone, humans will at least sometimes impress us (Kahneman & Klein, 2009).

Bias Is An Artifact Of Experiments Designed To Elicit It

A second and more pointed criticism of bias research comes from scholars who suggest that findings of bias are due to specific and unnatural research conditions. For example, consider the false consensus effect we discussed earlier. Arguably, when people spontaneously estimate the prevalence of a certain behavior or belief it is most often because they want that information to guide some future behavior of their own; in other words, they most often have an incentive to get the correct answer. These conditions do not obtain in most studies that report false consensus effects and one study that did offer participants a reward for accurately approximating the proportion of the population that was like them found no evidence of false consensus (Engelmann & Strobel, 2000). The lack of incentives may account for some other biases observed in the lab, but likely not all of them, given that many are robust to manipulations of incentives (e.g., Speckmann & Unkelbach, 2022).

Another critique of bias research and its dependence on specific study parameters focuses on the better-than-average effect. This effect emerges across a variety of traits and skills; the prototypical study shows most people declare themselves to be better drivers than the average person. Proponents of this effect note that this evaluation is a mathematical impossibility: By definition, only half the people can be above average drivers. But this fact is only true if we take "average" to signify the median—if we take it to signify the mean and acknowledge that the distribution of accidents per driver is highly skewed, it is easy to imagine that most drivers do drive better (or at least more safely) than the arithmetic mean (Gigerenzer, 2015).

A final example concerns the bias toward overconfidence. It is relatively easy to demonstrate an overconfidence effect, whereby people's estimates of their accuracy exceed their actual accuracy on a set of objective test questions. But one pair of studies manipulated whether the test questions were selected to be difficult, like the typical study of overconfidence, or to mimic the natural learning environment (Juslin, 1994). These studies found overconfidence only in the former case in which the researchers were trying to trip up participants and not in the latter case that more closely resemble everyday life. We leave it to the reader to judge whether these two studies' results are sufficient to call into question the existence of what Kahneman has called the "most significant of the cognitive biases" (Kahneman, 2011, p. 255). In any event, we deemed it worth mentioning because it illustrates the view, held by some, that bias studies often stack the deck in favor of positive findings.

Indeed, the findings reviewed above, along with others, have been marshalled as evidence that bias researchers are themselves biased—toward producing evidence of human failings (Krueger & Funder, 2004). Critics variously attribute this researcher bias to a preference for the negative and a desire to contribute to the exhibits in the "Museum of Incompetence" (Krueger & Funder, 2004, p. 10; see also Kihlstrom, 2004), to ideological pre-commitments to certain ideas (e.g., that stereotypes are threatening; Fiedler, 2004), or to the field's focus on the individual at the expense of the social context in which that individual is operating (e.g., Haslam, Postmes, & Jetten, 2004). Others come to the defense of bias researchers and remind readers of the fundamental claim of Kahneman and

Tversky (1982): “That non-normative responses are an excellent tool to shed light on basic cognitive processes that would have gone unnoticed otherwise” (Klar & Levi, 2004, p. 37). According to Kahneman and Tversky, bias researchers do not identify irrational outcomes merely for the thrill of it, but rather to shed light on the sometimes, irrational cognitive processes by which people arrive at generally rational, accurate or functional outcomes.

The debate over whether lab studies exaggerate the extent of various biases or even produce false ones has led some to suggest that the problem is the focus on outcomes: “Focusing primarily on outcomes of any kind –whether positive, negative, or neutral –inhibits theoretical development, because outcomes of complex mental processes are inevitably context-dependent and therefore superficially inconsistent” (Epley, Van Boven, & Caruso, 2004, p. 21) Echoing a distinction we noted earlier, the argument here is essentially that outcomes are phenotypic, and concentrating on them obscures the more appropriate target of theoretical development: (genotypic) processes.

It's Survival That Matters, Not Accuracy

A third criticism of bias research—or at least what it interprets as evidence of human error or irrationality—comes from evolutionary psychology (Pinker, 2021). That perspective cautions that it is beside the point to ask whether people arrive at accurate beliefs or behave consistent with a perfectly rational analysis of a situation. Evolution did not maximize humans for rationality, but rather for survival and fitness: We should be asking whether people arrive at beliefs and select behaviors that will enhance their fitness. As an illustration, the source of the need for positive regard and the biases it produces has been traced to its evolutionary value (Sedikides & Gregg, 2008).

Having dismissed perfect accuracy and logical coherence as standards against which human judgment and behavior should be compared, evolutionary psychology offers an alternative. After specifying the scenarios that were reliably present in the ancestral environment—that is, the scenarios in which evolution would have optimized survival (Gigerenzer & Todd, 1999; Kenrich & Keefe, 1992)—we should expect *adaptive* beliefs and behaviors to dominate in modern instantiations of such scenarios. However, we should not necessarily expect equally adaptive responses to problems that are specific to the modern era, or that have not presented themselves for enough generations for evolution to occur. It is because “the modern world tends to create situations where some of the default values of evolutionarily adapted heuristic systems are not optimal” (Stanovich, 2007, p. 45).

Concluding On (Ir)Rationality

Although the extent of human (ir)rationality has intrigued social psychologists for over a century (Allport, 1954; Zajonc, 1999), and philosophers for much longer, consensus on this topic remains elusive. Debates that have focused on diagnosing process—for example, why do individuals act differently in crowds, why do people see certain behaviors and certain social categories as more common than they are, why do humans see themselves in such a positive light, why do they misjudge probabilities—have deepened our understanding of human psychology. However, debates on evaluating outcomes inevitably invite argument. For one thing, it is telling that any time an ostensibly irrational behavior is identified, be it panic behavior, prestige suggestion, or biased reasoning, this is invariably followed by claims that the behavior is in fact not so common. As Lopes’ (1991) ruefully notes, “somehow the message of irrationality has sprung free of its factual

supports” (p. 67). For another, it is notoriously easy to pick apart any given standard for what counts as (ir)rational: A belief that is irrational by the standards of accuracy may be rational by the standard of adaptiveness. Which standard we should most care about is ultimately a question of values better answered by philosophers than by social psychologists (Trafimow, 2015). But social psychologists have often made clear their opinions on this question, because, as our next section will make clear, not all of us want to escape value judgments, and even those who do may not be able to.

BASIC SCIENCE, RELEVANCE, AND ADVOCACY: WHERE SHOULD WE PLACE THE FULCRUM TO MOVE THE WORLD

The last tension we consider also has existed throughout the field’s history but has grown especially heated recently. It is the debate over what it is that social psychologists should contribute to the world. The field has long been driven by concerns with what its constituents have perceived as pressing problems and issues of social importance, such as fascism and authoritarianism, racism and prejudice, fake news, and the environmental crisis. The claim that the field *should* contribute something of value given these concerns is not contentious. What is contentious is precisely what that is: Enhancing, through basic science, humanity’s knowledge about itself, or helping, through intervention and advocacy, to solve pressing problems and issues of social importance?

No rule states that the goals of basic science and practical applicability must stand at odds with each other, however they inevitably pull researchers to prioritize different questions and different methods. An emphasis on the kind of theoretical importance and operational precision required by basic science can often diminish the real-world relevance or applicability of research and, conversely, increasing its relevance can sometimes diminish its capacity to inform basic science. Consequently, producing research and theory that is highly relevant and has actionable results while also contributing to basic science has been something of a holy grail for social psychology. Complicating discussions about how to optimize the two goals are disagreements on how even to define them.

What Counts As Basic Science, What Counts As Real-World Relevance?

What Is Basic Science?

During the heyday of logical positivism, there was broad consensus about the meaning and value of basic science: Work that rigorously applied the scientific (read: experimental) method with the aim of discovering universal processes. Research and theory that did not adhere closely to best scientific principles or focus on universal processes was not considered basic science, however relevant it might have been to social issues. Growing concerns about the scientific method in general (Feyerabend, 2010), about social psychology’s prized experimental method in particular (Gergen, 1973; Haslam & McGarty, 2001; McGuire, 1973; Ring, 1967), and the cultural and context sensitivity

of psychological processes led to cracks in this certainty. Recent charges that social psychology privileges and promotes a non-inclusive, biased conception of science and rigor (see Ledgerwood et al., 2021; Lewis, 2021; Roberts et al., 2020) widens these fissures further.

Additionally, concerns about the validity of experimental laboratory findings have continued to grow (Earp & Trafimow, 2015), recently focusing on p-hacking (Simmons, Nelson, & Simonsohn, 2011), lack of cumulative theory-building (McPhetres et al., 2021), flawed approaches to measurement (Flake, Pek, & Hehman, 2017), and the disappearance of behavioral measures (Baumeister et al., 2007; Cialdini, 2009; Doliński, 2018). Together these concerns cast further doubts on the value of the knowledge purportedly generated by laboratory experiments. With such doubts in mind, it has become more feasible to justify research by appealing to relevance.

What Is Relevance?

What to be relevant to

A key question is what social psychology seeks to be relevant to. It is easy to say that the field should be relevant to important social issues but who decides what those issues are--social psychologists themselves? Funding agencies? The public? When the Society for the Psychological Study of Social Issues (*SPSSI*) was founded in 1936, it defined the issues to which it sought to be relevant as the three Ps: Peace, Prejudice and Poverty. Contemporary social psychologists might wish to extend this list to incorporate three Es, Education, the Environment, and Equality. Funding priorities are often narrower and more subject to change than the ambit of social issues to which social psychologists think their field is or should be relevant. It is an open question whether the field can accurately identify what the public thinks the most important issues are. Whether the field *should* be swayed by this public opinion is a different question. George Miller (1969) who famously exhorted the field to “give psychology away” nevertheless cautioned that “pandering to public interest would destroy the integrity of psychology” (p. 40).

Is conceptual relevance enough?

A second key question is the degree to which real-world relevance means going beyond providing insights and understanding on important social issues and contributing concrete solutions to them. The latter is a much higher bar—while many papers’ discussion sections describe potential practical applications of their findings, far fewer go so far as to test and document them.

On the one hand, some argue that conceptual relevance (Deutsch, 1969) is the sufficient and indeed most desirable form of relevance. By this account, the role of a laboratory researcher is to illustrate and illuminate a concept relevant to the real world (e.g., the way people stereotype a particular group or the psychological effects of scarcity). The goal of conceptual relevance is clearly compatible with the goal of basic science, as is implicit in Marilyn Brewer’s (1985) advocacy for so-called “analogue research”. An experimental paradigm qualifies as an analogue if the researcher has systematically represented elements of a specifiable real-world situation in the laboratory. As an example of a laboratory analogue, Brewer cites research on social dilemmas—situations in which a group of individuals each acting in reasonable self-interest leads to collective disaster such that everybody is worse off than if they had not behaved self-interestedly (Hardin, 1968). Because laboratory analogues incorporate “the essential features of the problem of resource conservation in

the real world,” she argues that the findings they generate have high relevance to their real-world counterparts.

On the other hand, many researchers today would be skeptical of the notion that analogue research and conceptual relevance can truly contribute to resolving real-world problems (Giner-Sorolla, 2019). Instead, these researchers would like to see instrumental relevance: Research that directly demonstrates the (in)effectiveness of specific approaches in achieving real progress on social issues. Whether research counts as instrumentally relevant depends on its sampling: What justification can be given for generalizing findings based on undergraduates or crowdsource workers to, for example, the behavior of fishermen in the developing world facing a social dilemma? It also depends on the unique features of the real-world context to which the research purports to apply: The translation of lab findings into a workable solution for a particular real-world social dilemma often requires considerable contextual, on-the-ground knowledge and experimentation. Consequently, some would argue that the findings generated in lab analogues are unlikely to provide sufficient practical guidance for those trying to avoid collective disaster in a real-world social dilemma (e.g., Berkman & Wilson, 2021). Acknowledging this reality has led government and private funding agencies in America, Europe, and elsewhere to be less supportive of basic research that does not specify its relevance to important social issues (Giner-Sorolla, 2019; Otten & Fischhoff, 2022).

Distinguishing relevance from newsworthiness

Survey data suggest that the public’s appetite for social psychological research has grown (Campbell & Reiman, 2022). However, this appetite may be stronger for what Festinger (1980) called “newsworthy” research, as opposed to that which is truly relevant to pressing social problems. For instance, the website Reddit (one of the most influential websites on the internet, [Shaer, 2012]) provides summaries of social psychological studies that readers can upvote (like) (McPhetres, 2019), and the website *Character & Context*, the blog of the *Society for Personality and Social Psychology*, features “latest findings” its editors think will interest readers. The research that gets highlighted on these platforms—for instance, on ghosting, homophobia, and marijuana use—is often relevant to everyday life and piques the interest of the public and journalists (Strack, 2012). But it only sometimes speaks to, and even more rarely offers solutions for, the three Ps or the three Es, or the social issues that Cartwright (1949) prescribed 75-years ago as the key foci for social psychology: “preventing atomic war, eliminating racial prejudices, and establishing labor-management peace.” We suspect it would gratify many social psychologists if today’s research on these topics generated more of the clicks valued by bloggers and websites.

Two Models For Instrumentally Relevant Research

As they reflect on the tension between basic science and solving concrete problems, social psychologists wishing to do at least some of the latter have chosen between at least two different models: 1) action research, as described by Lewin and others (Cunningham, 1993; Trist & Murray, 1990), and 2) intervention research, which is more common in today’s environment.

Action Research: Solving A Specific Community’s Concrete Problem Through Collaboration

Bridging the gap between social theory and social practice was more than a pie-in-the sky aspiration for Lewin; it was essential to the functioning of a democratic society (Cunningham, 1993). He believed that social problems should serve as the impetus for public inquiry within democratic communities (Kemmis, 1988). He acknowledged the challenge of bridging this gap: Although he believed in universal psychological laws, he also acknowledged that “laws do not tell *what* conditions exist locally, at a given place at a given time” (1946, p. 44).

Lewin’s proposed means for overcoming this challenge and combining theory building with research on practical problems was action research. He was vague about the precise meaning of this term, possibly because he died very shortly after introducing the concept and wrote only 22 pages on the topic (Peters & Robinson, 1984). Concurrently, a similar action research thrust developed in Great Britain; the disciplinary group that pioneered this work later formed the *Tavistock Institute of Human Relations* in London (Elden & Chisholm, 1993).

Of the many definitions of action research, each of which highlights different components (Chen, Huang & Zeng, 2018; Shani & Coghlan, 2021), most agree that its starting point is a real-world social situation. This situation must be one that the people involved as well as researchers define as problematic, and these actors must want to seek to improve it through collaborative inquiry and iterative research. This starting point highlights two of the key distinctive features of action research: 1) it is designed and executed in collaboration with research participants, hence the knowledge transfer is two-way rather than one-way transfer from supposed experts to communities of laypeople, and 2) it involves a continuous and iterative emergent process of research and learning rather than a sequence of studies that one could plan in advance as part of a standard grant application (Cunningham, 1993). In principle, the goals of action research are lofty: To improve the situation and the circumstances of those experiencing it while adding to scientific knowledge. In this sense, it meshes with the central tenet of John Dewey’s pragmatism philosophy, which is that intellectual inquiry begins with a problematic situation (Susman, 1976). Simultaneously, it assumes that the knowledge generated by action research will contribute to theory development.

The classic example of action research in social psychology is Coch and French’s (1948) involvement in the Harwood Manufacturing Company, a producer of pajamas, located in Virginia. The problematic situation was that the workers, who were mostly young women, resisted changes to their work practices and to their job roles that the management saw as essential to the productive functioning of the company. Researchers documented various forms of resistance, including grievances, low productivity, and loss of personnel. Then they talked to the workers to better understand their resistance and analyzed the situation in a Lewinian framework, by identifying the restraining and facilitating forces that operated in a state of quasi-stationary equilibrium. Subsequently, they designed experiments to test hypotheses about how to reduce restraining forces and increase facilitating forces. Two primary facilitating forces were more transparent communication by management about the need for changes, and participation by the workers in goal setting and implementation.

After several iterations, various behavioral and attitude measures showed that the intervention was impressively successful. For example, employees in the experimental groups took only a couple of weeks to return to baseline levels of productivity following a disruptive change, whereas those in the control group were still operating below baseline after a month. Coch and French’s (1948) research still serves as one of the best examples of action research because of how felicitously it

combined the method's key components: collaborative inquiry, hypothesis generation and testing, and contribution to scientific knowledge pertaining to overcoming resistance to change.

Despite being applauded widely, action research has not played a significant role in social psychology since Lewin's time (Simon & Wilder, 2018). Sanford (1970) delivered this succinct post-mortem on the movement over 50 years ago: "despite the exuberance in the late 1940s, Lewin's action-research never really got off the ground, it was never widely influential in psychology or social science" (p. 7). The main reason for this was that action research posed a very difficult challenge: Pursuing the simultaneous goals of enacting social change and acquiring scientific knowledge. Couched as it was in a concrete setting, not everyone was willing to assume that the findings of action research carried theoretical implications that could generalize more broadly to other settings.

In addition to their skepticism about its generalizability, proponents of basic science further discounted action research for its willingness to combine, rather than isolate, manipulations of key variables to create the desired social climate, whether in the lab or the field. Action research reflected Lewin's comfort with and commitment to considering a social system in its natural environment. In the words of the owner of the factory where Coch and French conducted their research, "Lewin was much more interested in having significant research started on major social problems, even if the approach was crude, than on unimportant problems with nice, neat, precise methods" (quoted in Marrow, 1969, pp. 154-155). This practice was out of step with the ethos that characterized social psychology going forward and offended the sensibilities of scholars such as Festinger, who emphasized internal validity and believed that it was essential that experiments permit the identification of the causal impact of any one variable on another. Festinger's student Eliot Aronson (1999, p. 87) put it thus: "Leon was not interested in improving the human condition. Not in the least. He approached research in social psychology as a puzzle to be solved, the way a chess master approaches a chess problem: Trying to understand human behavior and doing good research (not doing good) were more than enough to keep him excited."

Festinger was to leave an enormous footprint on the field, so it is worth emphasizing with another example just how little regard he had for the relevance of social psychological research to pressing social problems. Harold Gerard (1999) recounted how Festinger, his dissertation advisor, reacted to his decision to leave the safety and comfort of the laboratory to study the effects of racial desegregation (post -*Brown v. Board of Education*) on minority performance in schools in the Riverside School District in California. Gerard's project could not have been more socially relevant in mid-1960s America, but when he described its scope and purpose to Festinger, the response he got was "Hal, I thought you were an intelligent man" (p. 60). Having one's foray into socially relevant field research dismissed in this manner by one's advisor would have been discouraging to say the least no matter who that advisor was. But that this was the attitude of the preeminent social psychologist of the time must have had a broader impact on the ambitions of aspirants in the field.

In short, as appealing as Lewin's ideal *scholar-practitioner* model was, fulfilling it proved elusive. Researchers felt compelled to choose a side in the applied versus basic research divide; professional associations, journals, and funding agencies demanded it (Simon & Wilder, 2018). When researchers do follow the principles of action research, engaging communities and using their insights to inform their work in an iterative fashion, they often find it necessary to relegate that qualitative aspect to supplementary materials (e.g., Wu & Paluck, 2025). Thus, action research still exists but is rarely practiced by those wearing the basic researcher hat (Greenwood, 2002), and generally no longer aspires to generate general scientific knowledge (Peters & Robinson, 1984; Shani & Bushe, 1987;

Susman & Evered, 1978). Consequently, it is more often found siloed in specific domains (Glassman, Erdem, & Bartholomew, 2013), particularly in business and organizational dynamics (Argyris, 1982), community development (Hiebert & Swan, 1999), and education (Postholm, 2011).

Current studies conducted under the banner of action research are highly varied (see Bradbury, 2015; Elden & Chisholm, 1993; Schein, 2010), and typically engage participants in hypothesis generation and research design implementation to an even greater degree than Coch and French (1948) did. This tendency has led to the emergence of a new term, participatory action research (PAR), and a new goal of empowering participants through competence building and other forms of self-development (Elden & Chisholm, 1993). An even more developed form of this research is called critical participatory action research (CPAR), which has the additional goal of functioning as an advocacy tool for social justice through the formation of research collectives. In CPAR, not only are the design, methods, and analyses iterative and participatory, but the choice of research questions and research products are also decided collectively (Fine & Torre, 2019; Torre, Stoudt, Manoff, & Fine, 2017). An example of this is the national participatory survey by/for lesbian, gay, bisexual, trans, queer, intersex, asexual, plus (LGBTQIA+) youth reported by Fine and Torre (2019).

Concept-Based Intervention Research: Applying A Single Concept To Multiple Domains

If action research, defined by its iterative and participatory features, has not flourished in social psychology, other forms of research aimed at improving social welfare surely have. This work focuses on trying to effect change on a specific social issue, but does not meet the criteria of action research, either because it is conducted on convenience samples in or out of the lab, or without the iterative engagement with those most affected by the issue (e.g., work that aims to reduce political polarization; Voelkel et al., 2023). This research is sometimes called intervention research, applied research, and increasingly translational research (Bardo & Pentz, 2012; Wetherington, 2015).

The form of intervention research we describe here is one whose starting point is a particular concept that the researcher has studied in a laboratory context, and now wants to deploy in the real world for social benefit. This approach has grown in popularity during recent years as a means of demonstrating the validity of social psychological findings following the so-called replication crisis: The case for the “realness” of a psychological process is rendered more compelling if its manipulation enhances social welfare in the real world. Examples are applications of the Elaboration Likelihood Model to practical challenges involving attitude change efforts (Petty & Brinol, 2020), attachment theory to construct intervention programs that foster the broaden-and-build cycles of security in these domains (Mikulincer & Shaver, 2019), theories of the self (mindset) to improve individual functioning in multiple settings (Dweck, 2006), and subjective meaning-making concepts to social problems such as poor achievement, personal and intergroup conflict (Cohen, 2022; Walton & Wilson, 2018).

The social psychological model that has been applied most extensively, especially in the domain of health behavior, is the theory of planned behavior (Ajzen, 2005, Ajzen & Schmidt, 2020), which proposes that behavior is a function of personal attitudes, subjective norms, and perceived behavioral control. Applications of this theory have been published almost exclusively in applied journals, but more recent intervention studies are increasingly published in journals viewed as higher status by the mainstream. The highbrow aversion that existed to applied research for most of the field’s history (see Streufert, Streufert, & Streufert, 2021) seems to be receding.

Compared with action research, contemporary concept-driven intervention research may appeal more to the basic science crowd. If its intention is to demonstrate the broad applicability of a particular theoretical concept, it can be interpreted as documenting generalizability, and can connect, by analogy, to the presumed connection between universalism and basic science. The question remains, however, if the single concept approach to intervention is the most effective (Pettigrew & Hewstone, 2017). The intervention may display the merit of the researcher's favorite conceptual variable, but it is unlikely to ameliorate the problem it addresses as fully as had the choice of relevant theories been more ecumenical. The message that there is no shame in using someone else's theory (Mischel, 2006) is not easy to heed given current professional incentives (Giner-Sorrola, 2019).

A hugely influential force in the tradition of contemporary intervention work was Thaler and Sunstein's (2008) book, *Nudge: Improving Decisions About Health, Wealth, and Happiness* and its successor (2021). Their key observation is that features of choice architecture can be manipulated in ways to prompt people to make decisions that benefit themselves and others. That is, in an area where people's typical choices go against their stated preferences, one can alter the environment in which options are presented to encourage the preferred choice. The prototypical example of a nudge is the altering of which choice option is presented as the default (Davidai, Gilovich, & Ross, 2012). For example, employees are much more likely to choose to contribute to a retirement plan if the choice to contribute is the default, and they must actively opt out to avoid contributing, rather than the reverse set-up where the default is to not contribute, and people must actively opt in if they wish to do so. One appeal of nudges is that their implementation appears relatively straightforward, requiring of the implementer neither deep theoretical insight into the psychology underlying the manipulation nor deep knowledge of the target population. The apparent ease of outsourcing the implementation of an intervention to practitioners is an important determinant of its popularity, as has been the case with the theory of planned behavior (Ajzen & Schmidt, 2020) and social norms marketing (Miller & Prentice, 2016).

"Nudge units" tasked with employing choice architecture (mechanics of judgment) to promote social good have sprung up in many countries, beginning with *The Behavioural Insights Team* created in the UK in 2010 (Halpern 2015; Sunstein, 2016). The evidence on nudges is mixed, with some supporting their effectiveness (Bernartzi et al., 2017) and some questioning it (Maier, Bartos, Sanfley & Wagenmakers, 2022; Mertens et al., 2022; Van Rookhuizen et al., 2021). One concern expressed about nudges is that the effects of choice architecture on behavior are highly sensitive to context and population heterogeneity (IJerman et al., 2020). For example, in the United States, reminding people of their past purchases reduced their subsequent spending (Soman, 2001), but in Korea a scaled-up version of this nudge backfired, increasing spending among most consumers (Kim et al., 2020). It remains unclear whether this is due to population differences, seemingly minor differences in the delivery mechanism of the reminder, or some other moderator (Soman & Hossain, 2020).

Findings such as these have prompted a growing recognition that leveraging the full effectiveness of nudges requires attention to the broader environmental and institutional conditions in which nudges are situated (Ewert & Loer, 2021; Schmidt, 2017; Wu & Paluck, 2021). Choice architecture is often embedded in a broader system and infrastructure; thus, understanding the complexity of these systems is critical for deploying nudges effectively (Chater & Loewenstein, 2022). Silver bullet-like behavioral interventions, no matter how devoutly wished for, are elusive given system complexity, contextual variation, and population heterogeneity (Bryan et al., 2021).

Other Contemporary Forms Of Relevant Research

Not all social psychology research concerned with real-world relevance falls neatly into the categories of action and intervention research. For example, the aim of some field research is to demonstrate how psychological concepts can explain, not necessarily ameliorate, real-world social problems. Moreover, we noted earlier the gradual shift toward emphasis on outcomes or dependent measures, rather than processes or independent variables, and the accompanying emergence of subfields focused on phenomena. Examples of such subfields are law, environment, education, and health, each of which has a dedicated chapter in this *Handbook*. Research falling within these subfields is necessarily relevant, at least conceptually if not always instrumentally.

Social psychologists working in phenomena-focused subfields typically have identified a series of important issues or questions relevant to that domain that they think their discipline should address (Forgas, Crano, & Fiedler, 2020). Examples are how the reliability of eye-witness testimony can be increased (Cutler & Penrod, 1988); how energy conserving behaviors can be enhanced (Carrus et al., 2021); how female participation in STEM courses are to be augmented (Diekman, Weisgram, & Belanger, 2015); and how people's compliance with medical regimes can be improved (Haslam et al., 2009; Petty, Barden & Wheeler, 2009; Sensenig & Cialdini, 1984). Not all subfields have an explicit or direct welfare-enhancing agenda (e.g., political psychology) and not all research that seeks to promote social welfare—for example, research aimed at increasing volunteerism (Snyder & Omoto, 2008; Snyder, 2009) or curbing bullying (Triggs, 2014)—has attained subfield status.

As a stark illustration of social psychologists' appetite for being relevant and addressing social problems, one need only look at the community's response to the Covid-19 pandemic. Social psychologists immediately began producing papers describing ways in which social psychological knowledge could be used to understand and combat COVID-19, and journals were eager to publish them. By one count, 540 new papers about the psychology of COVID-19 were written and shared (via the psychology preprint server *PsyArXiv*) within the first 6 months of the pandemic (Syed, 2022).

This response certainly suggests that social psychologists believed that they had something of value to offer. Some analyses support that view (Van Bavel et al., 2020). Other researchers have expressed doubt that there was a sufficient scientific knowledge base for many of the analyses and recommendations of social psychologists and other psychologists (IJzerman et al., 2020; Lewis & Wai, 2021). Some have even documented what they saw as misleading insights and dangerous misstatements made by psychologists in their zeal to be relevant during the pandemic (Ritchie, 2020). The charge that the foundation of social psychology science was insufficient to provide reliable counsel on the pandemic resonates with a concern that Festinger (1980) expressed 60 years ago about the perils of seeking to be relevant without a sufficient base of scientific knowledge: "Action without knowledge is akin to wandering in the dark—ineffective no matter how determined and moral the wandering is." (Festinger, 1980, viii).

More time must pass before answering the questions of how helpful social psychologists' responses to the pandemic were and what social psychologists learned from the pandemic. Social scientists contributed much to the World War II military effort (Breen, 2002), but they also developed a formidable corpus of theoretical insight from that involvement, much of it summarized in the highly influential book *The American Soldier* (Stouffer et al., 1949). One can only hope—in the spirit of searching for a silver lining—that the pandemic proved a similarly fruitful source of theory generation for social psychologists. It would certainly be the hope of Kurt Lewin who strongly

believed, in addition to the sentiment expressed in his more famous aphorism (“there is nothing as practical as a good theory”) that “there is nothing so theoretical as a good practical problem” (Cartwright, 1978, p. 178).

An Evolving Field: Dissemination, Theory, And Appeal To New Generations Of Researchers

More than with any of the other tensions we have described in this chapter, it strikes us that this one has followed a clear and linear trend. As decades pass, the field seems to place greater priority on relevance in various forms. This has had consequences for how, and to what audiences, social psychologists disseminate their findings, and how scholars think about the relationship between their work and theory. Perhaps most importantly, it influences the type of person who is attracted to our field--its future generations. Moreover, as the preoccupations in the field impact who enter the field, those who enter impact the preoccupations of the field (Harris 2025; Morawski, 2012; Risi et al., 2022; Stanovich & Toplak, 2022). The latter is perhaps historically clearest in the case of the impact that women’s increased numbers had on the field’s research agenda (Berscheid, 1992; Reis, 2012), including but not limited to the greater emphasis on dyadic relations.

Audience Pressures And Dissemination

When scholars do research that they consider socially relevant, they typically want the world to know about it. For this purpose, they must disseminate their research findings in a way that gains the attention of those in positions to change practices, typically in fields of education, health, or public policy. Social psychologists disseminate their work in multiple ways, including speaking at professional meetings of practitioners, giving talks in public fora like the TED conference, writing popular books or opinion pieces, contributing to blogs and other online media, and generating policy reports for government agencies. All these activities have increased in recent decades across the globe. The efforts of social psychologists to be more public-facing sometimes seem to reflect their conviction that they are as (if not more) deserving than practitioners of another social science discipline that more often sit at policy making tables—economics envy, if you will.

Two other sources of pressure that have shaped how social psychologists disseminate their work are the public and funding agencies. The public generally does not see abstract theoretical laboratory research as speaking to issues relevant to them; therefore, the field has tacked accordingly. Bolstering traditional arguments for external validity and mundane realism is now the assertion that they are necessary for the public to recognize the relevance of social psychology (Berkman & Wilson, 2021; Cialdini, 2009; Ellsworth, 2010; Giner-Sorolla, 2019; Lin et al., 2021; Osborne-Crowley, 2020; Rozin, 2009; Sunstein, 2016; Wojciszke & Bocian, 2018). Furthermore, funding agencies increasingly require evidence of past and future knowledge translation, or efforts to engage a relevance-hungry public.

There are many other indices of the growing status of intervention or field research in social psychology. More journals, including the most prestigious ones, are open to publishing such research; more government grants and private foundation money are available for it; and more researchers in social programs at elite universities are doing it. Festinger and others might have seen this as an unfortunate increase of lowbrow pursuits, but that view is seldom voiced today.

In summary, pressure from the public, media, and funding agencies has sensitized social psychologists to the real-world relevance of their work. This self-reinforcing cycle has meant that, over time, those who work in the field on projects of social relevance have seen their status increase. Correspondingly, those who see themselves as champions of basic science have found themselves needing to justify their work more than they used to. The increasing migration of social psychologists into professional schools, especially business schools, has further accelerated this shift.

The Status Of Theory

The push for relevance has important implications for the status of theory in the field, and for how we prioritize or even define basic science. In the past, when people criticized the state of theory their complaint was about its dearth (see Ellemers, 2013; Kelley, 2000; Kruglanski, 2001; Sherif, 1970). However, recently the tables have turned: Critics now push back against what they see as the field becoming overly theoretical and insufficiently descriptive (Rozin, 2001), esoteric (Witte & Brandt, 2011) and increasingly complex (Quiñones-Vidal, José López-García, Peñaranda-Ortega, & Tortosa-Gil, 2004; Sherman, Buddie, Dragan, End, & Finney, 1999).

This recent criticism of theoretical excess would probably surprise and dismay previous generations of social psychologists. Cartwright (1948) likely captured the position that most of his generation held on the importance of theory in addressing social issues: “But serious doubts may be raised as to whether the fundamental social problems of the modern world can be solved without a major emphasis upon research designed essentially to refine concepts and to test the relationships among theoretical constructs.” In a similar spirit, generations of SPSSI presidents strove to legitimate their society’s commitment to theory in the scientific and the public sphere (Capshew, 1999; Cherry, 2001; Winston, 2011). Currently, the position on how to increase social relevance seems to have shifted, with recommendations being made “to seek ways to make psychological science more data-driven than theory-driven...” (Ferguson, 2015).

Even contemporary social psychologists who see theory as having practical relevance to social issues may hold a different conception of what makes a theory relevant than did previous generations. Contemporary proponents of theoretical relevance (e.g., Berkman & Wilson, 2021) are fond of citing Lewin’s (1943) dictum: “There is nothing so practical as a good theory.” But Lewin presumably had his mathematized field theory in mind as an example of a good theory. This theory would not meet the practicality criterion of permitting “realistic adaptation to a specific context by practitioners” (Berkman & Wilson, 2021, p. 864). To use Lewin’s field theory to create change, practitioners almost surely would have needed extensive collaboration with social psychological researchers, like what took place in the Coch and French (1948) defining example of action research. Practical applications of even the relatively straightforward theory of planned behavior likely benefits from the participation of trained social psychologists (Trafimow & Osman, 2022).

Although it may be difficult to envision how theories might truly be translated into solutions that are easily and independently implemented by practitioners, this does not mean that theories cannot have a practical impact. One of the most interesting observations George Miller (1969) made when he described how psychology can promote human welfare by “giving psychology away” is that psychology has the revolutionary power to change people’s conception of themselves— “what is humanly possible and what is humanly desirable” (p. 1066). For example, he argued that Freud’s theories, although they ultimately fell out of favor with most psychologists, nonetheless had a profound influence on the way Western culture portrays the self, and on how easily inhabitants of

that culture accept the irrational components of human nature and the reality of unconscious impulses. As an illustration, even though *The Implicit Association Test* (Greenwald, McGhee, & Schwartz, 1998) owes little theoretically to Freudian theory, the readiness of the public to accept it owes a great deal.

Who Is Entering This Evolving Field?

There is another consequence (or perhaps driver) of the move toward greater relevance and away from basic research--who chooses to become a social psychologist (Harris, 2025). The talents, temperaments, and dedication required to do research that pushes the frontiers of social impact versus basic science are different and always have been. As social psychologists rarely either simultaneously or sequentially do both, and because, in our view, incentives have shifted in the direction of social relevance, there may be changes in who is attracted to our field. In turn, changes in the field's constituents may push it further toward social relevance. In previous generations, a person inclined toward activism and advocacy might have eschewed academic social psychology in favor of a public policy school or might not have considered graduate education or a career in research. Today, that person would find plenty of prospective supervisors who, unlike Festinger, would celebrate their impulse to venture outside of the lab in search of practical solutions to real problems and ways to promote social justice.

The Lewinians saw themselves as hard-headed idealists (Patnoe, 1988). In the words of one of his students (Deutsch, 1999, p. 12):

Lewin was not only an original tough-minded theorist and researcher with a profound interest in the philosophy and methodology of science, but he was also a tender-hearted psychologist who was deeply involved with developing psychological knowledge that would be relevant to important human concerns. Lewin was both tough-minded and tender-hearted; he provided a scientific role model that I have tried to emulate.

In this characterization, commitment to basic science is compatible with commitment to social relevance. There is no doubt many today who share this sentiment and find that doing basic research satisfies their desire to make a positive contribution to key social issues, albeit an indirect one that may be realized far in the future.

Others pressing for more social relevance today see it differently, perhaps because they perceive an urgency to contribute more directly to immediate positive social change. Berkman and Wilson (2021, pp. 869-870) make this argument for a shift away from basic science: "Perhaps the most pressing of these factors is the potential that we risk discouraging bright, talented people from entering our field. Socially engaged people might not find the motivation to dedicate sustained attention to abstract theoretical questions when historical geopolitical events are unfolding around the globe." In other words, as some see it, a focus on basic science does not help inform, but rather detracts from relevance to pressing social issues.

We suspect that most social psychologists throughout the field's history, if asked, would self-report the same dual self-conception as Lewin, embracing identities as both tough-minded and tender-hearted. However, even if social psychologists across generations all reported both identities, it is unlikely that they would weigh the two comparably or conceive of them similarly. We suspect that many of today's social psychologists would strain to see the tender-heartedness in previous generations, who in turn would strain to see the hard-headedness in many of today's generation.

Values And Advocacy

Some of the actions that social psychologists have taken to make themselves more relevant to the real world have prompted considerable debate, typically surrounding the role that moral values do or should play in science, as well as in advocacy decisions. One form of the debate concerns the way the values of social psychologists influence the kinds of questions they ask and the answers they discover. A recent iteration has focused on political orientation as a broad marker of values (Jussim et al., 2015; Honeycutt & Jussim, 2022; Martin, 2016; Pinker, 2003). A second form of the debate concerns the fidelity of advocacy efforts to the strength of the science underlying them. Social psychologists can exaggerate the strength and consistency of policy relevant research out of a desire to promote policies they think are morally preferable (Ferguson, 2021; Mitchell, 2018). The third form of the debate concerns whether it is appropriate in the first place for social psychologists, or their professional organizations, to advocate for specific policies (Elson et al., 2019; Ertelt, 2006; Kendler, 1993).

Value Neutrality In Research

Science has traditionally assumed itself to be objective at least in some form, but there is increased uncertainty as to whether this is desirable or even possible. Philosophers of science have long recognized that values inevitably play a role in scientists' choice of what to study and what questions to ask. Max Weber argued thus when he claimed that social sciences were by necessity value-relevant (Weber, 1904/1949). Weber insisted that scientists could not avoid having their values color their interpretations of what they observed: "All knowledge of cultural reality, as may be seen, is always knowledge from particular points of view" (1904/1949, p. 81). However, in line with the traditional view, he firmly believed that science *should* strive to be value free, by which he meant free from value *judgment*: According to Weber, it was not the scientist's role to determine whether a certain behavior was desirable. Many social psychologists today would agree.

In more recent decades, philosophers of science have questioned whether it is possible for science to be value free given that science as a discipline, social or natural, relies by necessity on at least some values. In his influential essay *Values in Science*, McMullin (1982) pointed out that the very standards by which we judge the merits of a scientific theory (e.g., Kuhn, 1977) are determined by what we value. He related the story of a disagreement over quantum theory between Neils Bohr and Albert Einstein that occurred because Bohr placed a higher value on predictive accuracy (which quantum theory offered) whereas Einstein prioritized consistency with existing knowledge (with which quantum theory clashed). Predictive accuracy and consistency with other facts are both indicators of a theory's reasonableness; to decide which to weight more heavily, we must rely on values. Likewise, Rooney (1992) notes that when we choose to accept a theory because it offers a simple or parsimonious explanation of a phenomenon, that reflects a cultural preference for simplicity that is not grounded in empirical reality.

At least in certain circles, thus, "the claim that science is value-laden might no longer even seem controversial" (McMullin, 1982). Within social psychology, Reicher and Haslam (2006) articulated the role of values in the choices of research topics by focusing on classic studies such as the Stanford Prison Experiment and Milgram's obedience paradigm. They argued that these studies contributed to the creation of ethics review boards that necessarily require value-based decisions: weighing benefit to society against harm to individual participants. How, if not by consulting values, does a person (or an impersonal board) determine what counts as a societal benefit or a harm to

participants, and judge its magnitude? In turn, these value-based decisions determine the research that is deemed permissible and thus the discoveries that can be made. The point is not that ethics review boards should not exist, but rather that we are fooling ourselves if we cling to the belief that values do not shape what scientific knowledge looks like, or that political zeitgeists exert no impact on the selection or definition of problems to study (Adams et al., 2019; Collier et al., 1991; Paicheler, 1988).

These arguments against the feasibility of a value-free science have generally failed to resonate with mainstream social psychology. When critical psychological theorists make the claim, which is obvious and uncontroversial to philosophers of science, that value-neutral research is impossible (Adorno, 2005; Mednick, 1989; Richardson & Fowers, 1997), they often face backlash from social psychologists who prefer the traditional assumption of objectivity. Perhaps the most salient illustration of this occurrence in contemporary times is the debate over the influence of political values on social psychological theory and research. Many have pointed to the decidedly liberal skew among social psychologists as a problem, given the importance of values to science (Baumeister et al., 2015; Jussim et al., 2015; Inbar & Lammers, 2012; Redding 2001; Tetlock, 1994). If value-neutrality is an impossibility, then at least competing values should be represented or so the argument goes. Others have tested empirical claims that they see as implied by that view (e.g., that studies that purported to demonstrate conclusions favorable to liberals, compared to conservatives, will be accepted based on weaker evidence, Duarte et al., 2015).

The debate over whether and how to address the overrepresentation of political liberals in social psychology is far from over. From the point of view of the history that we have covered here, it seems naïve to think that this overrepresentation does not influence the questions we ask and the knowledge we generate, or that we can eliminate that influence by asking scholars to accomplish the (probably impossible) feat of abandoning their ideologies. A more promising avenue might instead be to accept the idea that values will inevitably shape our science and address the reality head on. As Douglas (2014, p. 178) states it: “Different scientists will have different values, and thus see different inferences as warranted. But with the value-free ideal set aside, this source of rational disagreement can be brought out into the open and become a source for robust discussion both within and beyond the scientific community.”

Advocacy And The Facts

One way in which values shape scientists' behavior is through their desire to advocate on behalf of causes they believe in. Professional groups such as the American Psychological Association (APA) and American Medical Association (AMA) often release public policy statements that speak to the current state of scientific knowledge as it relates to social problems or policies (Ertelt, 2006). These statements are meant to provide information for policy makers or simply inform the public, and ideally, they form objective summaries of current science. But because they are written by human beings who must determine which findings are relevant, and which are sufficiently consistent, it is easy to question their objectivity (Elson et al., 2019). When their writers appear to be partisan, policy statements can damage the scientific reputation of their field (Lilienfeld, 2002; 2012; O'Donohue & Dyslin, 1996).

Speaking from vast experience with policy related research, one social psychologist counselled that anyone venturing into the policy domain should “expect to be tempted to overstep one's competence” and “expect one's empirical results to be distorted, both deliberately and inadvertently” (Fischhoff, 1990, p. 652).

Research And Public Policy Advocacy

Social psychologists frequently see their research through the lens of public policy. But it is a question of considerable debate whether research can ever be interpreted as justifying a public policy. The argument that it cannot is based on the distinction between fact and value. Weber, for instance, believed that it was not a social scientist's place to get directly involved in policy debates, because these necessarily entail judgments about what goals are appropriate for a society to pursue. In his view, a social scientist can conceivably recommend data- or theory-driven strategies likely to be effective in achieving a certain policy goal, but their commitment to being value-free prohibits them from weighing in on the goal itself. This perspective goes back at least to Plato (1987), but in the context of modern psychological research, Kendler (2004) represents it with this thought experiment: "If bilingual education results in lower academic performance than English-only education, a scientist—as a scientist—cannot conclude that bilingual education should be abandoned." As psychological facts cannot validate a moral principle of public policy, Kendler argues that "the [hypothetical] evidence that bilingual education results in poorer academic performance would not refute the position that preservation of ethnic identification is a more important value than the academic performance of minority children." From this perspective, the role of science is not to identify the correct public policy but instead to provide unbiased information so that voters and their representatives can make informed rather than uninformed decisions. In the words of Passmore (1953), "social policy is determined in light of facts, but is not deduced from them." (p. 675). The recognition of this may account for the hesitancy of many social psychologists to enter the sphere of public policy (Esses & Dovidio, 2011).

Contrasting with this view, some social psychologists live comfortably with the *scholar-activist* identity (Allen & Dodd, 2018; Miles & Fassinger, 2021). In recent decades, there has been increasing emphasis in the literature and media on the role of psychologists regarding political action and the promotion of social justice (American Psychological Association [APA], 2015; Arfken & Yen, 2014; Fox & Prilleltensky, 1996; Kakkad, 2005; Pipes, Holstein, & Aguirre, 2005; Prilleltensky & Nelson, 1997; Sue, Arredondo, & Mc Davis, 1992; Teo, 2015; Walsh, 2015). Some have argued that the field's ethical commitment to social justice requires psychologists to expand their professional activities to the political domain (Vera & Speight, 2003).

It is difficult to justify advocacy without appealing to values. Some scientists believe that they can separate their reading of the data and their values. For example, explaining her support for affirmative action, Phoebe Ellsworth (2021) says:

I believe that diversity makes for better learning and decision-making in some kinds of groups, but that for other kinds of groups there is no strong evidence that this is so (Eagly, 2016), but I favor affirmative action even in groups that have not been shown to benefit from diversity because I think it is fair to give disadvantaged people a chance.

However, given how values can shape the questions we ask as scientists, a critic might note that such an explanation fails to address the possibility, for example, that social psychologists have generally not attempted to test ideas about the potentially undesirable, rather than simply neutral, consequences of diversity. An alternative approach was taken by Crosby and colleagues (2003), who wrote an unapologetic case for the public policy of affirmative action that drew criticism on charges of both mixing science and values (Kendler, 2004) and misreading the empirical support for affirmative action policy (Johnson, 2004). Ultimately, the advocacy role is one that is inextricably

linked to values; therefore, the most responsible approach might be to strive to be transparent and introspective about one's role in what one puts forward.

As it may be impossible for our science to avoid the influence of values entirely, individual scientists may not be able to circumvent completely the role of advocate. Even if one does not believe that one should advocate for public policies, often it is difficult to publicize one's research findings without sounding like one is advocating for a particular policy more than might want to (Fischhoff, 2013). Donald Campbell (1975) termed this situation the over-advocacy trap. The components of this trap are twofold. First, the challenge of communicating one's findings to the public often leads one to simplify them in ways that make them sound more definitive than they are. As Blanton and Ikizer (2019) put it, "the neutrality of the science in no way provides assurance of the neutrality of the science communication" (p. 175). Second, one's research findings can be used by others to support certain policies. Therefore, researchers who explore areas where passions run high—for example, climate science, stem-cell research, racial bias—cannot simply opt out of the debates that shape how others will interpret and react to their findings. Invested actors on both sides will use scientific findings to bolster their position (Elson et al., 2019). If their findings are to be used by advocates regardless, social psychologists might as well take an active role in spelling out how their findings do and do not support the efficacy--if not the desirability—of relevant policies.

GOING FORWARD

The other chapters in this *Handbook* attest to the vibrancy and breadth of the field of social psychology. They also reveal the wealth of knowledge that the field has accumulated over its history, both on topics that have been central to it since its inception and ones that have only recently commanded attention. It is clearly a field that will endure. Given this, what speculations does our review of four of the field's historically most enduring tensions offer about what the future of social psychology might look like—its conceptual foci, its methodological approaches, and its connections with other social sciences? What will change, and what will remain the same?

To assist in addressing these questions, it is helpful to view the future from a historical perspective—conveniently provided by the centenary of Allport's 1924 book. Much has changed in the field since Allport's epochal treatise, with many changes occurring soon after its publication: For instance, the behaviorist theory that guided Allport's exposition has long since receded into deep mists of history, as has his narrow conception of the purview of social psychology with its focus restricted to reactions to impoverished social stimuli. His similarly reductionist claim that there is no group psychology distinct from individual psychology also has long been discounted. And the relationship of social psychology to other disciplines has shifted since Allport and others succeeded in securing our discipline a home in psychology departments, separate from philosophy and other social science departments.

It is worth emphasizing how big a shift this move to psychology was, at the time. For over thirty years, George Herbert Mead taught a famous course at the University of Chicago entitled "Contemporary Social Psychology," but he did it as a member of the Philosophy Department. Social psychology's comfort with being housed alongside other areas of psychology has ebbed and flowed during the last century. Currently, we sense that there has been more questioning of its relation to other areas of the field than there was previously and greater interest in being housed in academic units other than psychology departments (e.g., Business Schools, Public Policy Schools). Perhaps relatedly, enthusiasm is growing for research that adopts interdisciplinary perspectives, multi-level

analysis, and multi-researcher collaboration (Ellemers, 2021), especially among funding agencies (Lyall et al., 2013; Rylance, 2015). Predicting that social psychology will fully exit psychology departments and integrate into an interdisciplinary network seems unwise—the short-lived experience of interdisciplinary training programs such as those created at Michigan and Harvard in the 1960's remains an important cautionary tale (Jackson, 1988; Johnson, 1988; Schmidt, 2022; see Lemieux, 2012)—but contemporary social psychology currently fraternizes with the other social sciences at a level not witnessed for well over 75 years. What this commingling portends will be revealed in the fulness of time.

Much has also remained the same in the last 100 years. Many would claim that one constancy from Allport's time to the present is what Serge Moscovici (1972) called "methodolatry": The conviction that the experiment offers the clearest window into social psychological processes. The epistemological status of the experiment has certainly been challenged in the intervening century, and the hope that it holds the key to discovering universal processes has diminished, but it remains the most common methodological arrow in the social psychologist's quiver. How long it will remain so is unclear. Certainly, the day of the high impact laboratory experiment is over, gone the way of Egyptian embalming techniques in Aronson's (1999) phrase, replaced largely by experiments conducted on platforms such as MTurk and Prolific. Nonexperimentally generated "big data" seems destined to play a big future role in the field both as a means of testing and generating hypotheses. The relationship of the individual to the social environment remains the hardest nut to be cracked. Their interdependence was recognized as far back as Wundt and their indivisibility featured prominently in the theories of Lewin, Cooley, Mead, Tajfel, Moscovici and many others. What has proved elusive is consensus on the method by which this indivisibility can be interrogated. The experiment may be seen as not up to the task, but interpretive alternatives have also not been widely embraced.

The behaviorist theory that Allport championed may have quickly lost favor, but it was not replaced by a similarly encompassing theory. Some were proposed—for example, field theory, gestalt theory, symbolic interaction theory, exchange theory, and more recently Harold Kelley and his colleagues' interdependence theory (Kelley et al., 2003; van Lange, 2015; Reis & Arriaga, 2015)—but none of these have acquired a grand-theory status. The lack of an overarching theory to ground and frame our collective work has no doubt contributed to the perception that social psychology lacks coherence as a field. Kelley (2000) made this point by quoting from the popular science writer, Morton Hunt, and his book *The Story of Psychology* (1993, pp.396-397), in which he asked: "What extremely busy and productive field of modern psychology has no clear-cut identity and not even a generally accepted definition?" Hunt's answer:

Social psychology. It is less a field than a no man's land between psychology and sociology, overlapping each and impinging on several other social sciences," because "social psychology has no unifying construct (as did behaviorism and Gestalt psychology) but grew like crabgrass in uncultivated regions of the social sciences.

This statement may be harsh and contentious, but the lack of coherence in the topics investigated by social psychologists is a common lament. Roger Brown introduced his classic social psychology textbook (Brown, 1965) with the assertion that he could see no common denominator uniting the list of subjects generally considered to belong to social psychology. This observation likely still resonates with today's textbook authors. The lack of a common denominator, or what Zajonc (1999, p. 200) termed a "core subject matter", makes theory development difficult and augurs against the emergence of a grand theory anytime soon.

Nevertheless, given the changing interests, experiences, and temperaments of those entering the field the overarching grand-theory project may not have the appeal it once did. Accepting the paucity of social psychological universals might have been dispiriting and discouraging to previous generations of social psychologists, but much less so to those currently entering the field. Discovering the highly particularistic nature of social behavior would seem to make social psychology more interesting not less to many of them. Similarly, many of the current generation may find formalization less appealing than did their predecessors and may find relevance and social impact more appealing. Ultimately, we can say confidently that the end of social psychology history should not be expected any time soon.

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ENDNOTES

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