

Measurement and Methodology in Social Cognition: A Historical Perspective

Alan J. Lambert and Laura Scherer

Abstract

This chapter offers a historical perspective on the methodological trajectory of social cognition. It begins by considering a number of preliminary issues as they bear on the various definitions of *social cognition*, with special consideration of how methodological issues figure prominently in those definitions. The chapter next considers the various desiderata in methodology, highlighting cases in which these issues are particularly germane to social cognition. This is followed by discussion of the circumstances surrounding the rise and fall in popularity of some prominent methodological paradigms in the field, including a number of important methodological issues surrounding the emergence of dual-process models. The final section considers some recent critiques of the “social cognition approach,” placing them in a larger historical context and discussing how they overlap with a long-running debate about the value of experimental paradigms in social psychology.

Key Words: social cognition, methodology, historical perspective

Introduction

Let’s face it: chapters on measurement and methodology aren’t sexy. Or, at least they’re not supposed to be. This state of affairs may be attributable to a general perception about the science of method, often regarded as less enticing than the world of theory. As Diener (2009) has noted, “many people, even researchers themselves, think of measurement as a technical affair to be performed by the slower and less creative scientists, while the geniuses are busy formulating grand theories” (p. 1). However, methodology isn’t the Rodney Dangerfield of science it might appear to be: It’s actually gotten a lot of respect over the years. For example, many people are surprised to learn that the majority of Nobel prizes in science have been awarded for developments in method and measurement, not theory (Greenwald, 2012).

In actuality, of course, method and theory cannot be separated. These elements are intertwined in any

empirical science (Cronbach & Meehl, 1955), producing a bidirectional dynamic such that method can influence, as well as be influenced by, the process in which researchers develop, test, and refine their theoretical formulations. These are the considerations that led Greenwald (2012) to suggest, “there is nothing so theoretical as a good method,” an obvious allusion to Kurt Lewin’s (1951) famous dictum (“there is nothing so practical as a good theory”). Like Greenwald, we believe that methodology and theory are interconnected. Much of this chapter was written with this principle in mind.

In this chapter, we offer a historical perspective to the methodological trajectory of social cognition. We begin by considering a number of preliminary issues as they bear on the various definitions of *social cognition*, with special consideration of how methodological issues figure prominently in those definitions. We next consider the various desiderata in methodology, highlighting cases in which these

issues are particularly germane to social cognition. This is followed by discussion of the circumstances surrounding the rise and fall in popularity of some prominent methodological paradigms in the field. We then consider a number of important methodological issues surrounding the emergence of dual-process models. In the last section of our chapter, we consider some recent critiques of the “social cognition approach.” We place these critiques in a larger historical context, discussing how they overlap with a long-running debate about the value of experimental paradigms in social psychology.

Why Methodology Is Especially Relevant to Social Cognition

One of the overall goals of our chapter was to take a “historical approach” to the methodological trajectory of social cognition. In other words, we wished to understand where the field has been, and how and why it got there. However, such discussion begs the question: What, exactly, is *social cognition*? Several writers have attempted to answer this question, each arriving at a somewhat different conclusion (e.g., Devine, Hamilton, & Ostrom, 1994; Hamilton, 2005; Wyer & Srull, 1989; see also Chapter 2 of the present volume). In most cases, however, writers emphasize the importance of method, even to the point of making methodology, quite literally, a defining feature of the field.

For example, according to one view, “social cognition is not defined by any particular content or substantive issue, but rather by an *approach* to one’s content or issues” (Devine et al., 1994; p. 5, emphasis in original). By “approach,” Devine et al. refer not only to foundational theoretical assumptions (say, about the importance of underlying process) but also to the methodological tools used to understand the phenomenon. A similar perspective is offered by Hamilton (2005), as well as by Wyer and Srull (1989). As such, social cognition can be regarded as a subdiscipline of psychology in which researchers rely on the theory, method, and perspective of cognitive psychology to study matters of long-standing interest to social psychology.

This does not mean that it is useful or meaningful to identify someone as “doing social cognition research” simply because (for example) he or she likes to use priming measures. The key is how those methodological techniques are being *used* on a theoretical level. In other words, social cognition researchers tend to use methodological techniques from the cognitive domain precisely because those techniques are able to provide leverage about issues

(say, prejudice) that social psychologists tend to care about. However, one must be careful not to overstate the influence of cognitive psychology, either. For one thing, even if some of the more frequently used methodologies were originally developed by cognitive psychologists, social cognition researchers often *use* these methodologies in different ways.

To choose just one example, priming methodologies (defined broadly) have been used in a wide variety of substantive and methodological contexts (cf. Bargh & Chartrand, 2000). In the cognitive literature, these tasks were originally developed, at least in part, to understand the automatic and controlled processes involved in semantic activation or other aspects of lexical processing (cf. Meyer & Schvaneveldt, 1971; Neely, 1977; West & Stanovich, 1982). However, priming tasks have been employed by social cognition researchers (with countless variations) in the service of studying all sorts of phenomena that fall well outside of the typical range of issues in cognitive psychology, such as research on mood and emotion (Beall & Herbert, 2008; De Houwer & Hermans, 1994), stereotyping and prejudice (Devine, 1989; Fazio, Jackson, Dunton, & Williams, 1995; Jones & Fazio, 2010), and research on the self (Kraus & Chen, 2009).

What Makes for a Good Method?

Earlier in this chapter, we suggested that there is nothing so theoretical as a good method. However, this begs the question: What determines whether any given method is, in fact, “good?” One easy answer is that good methods satisfy all of the usual criteria one would find in any standard discussion of research design, including various aspects of construct validity and statistical reliability (Campbell & Fiske, 1959; Shadish, Cook, & Campbell, 2002).

Concern with psychometric issues tends to become important whenever a subdiscipline is grappling with a rapid influx of new measurement techniques. The recent surge in popularity of indirect attitude measures (Gawronski & Payne 2010) provides a case in point. On the one hand, many of these measures employ priming-based techniques quite different from traditional, pencil-and-paper approaches (DeHouwer, 2006). Although this *methodological approach* to measuring attitudes is relatively new, the *questions being asked* about these measures are not.

For example, one of the critical questions to be raised about these new measures is the extent to which they are tapping sentiments different from those tapped by traditional attitude techniques (Klauer & Musch, 2003) and whether behaviors predicted by indirect measures tend to be similar

to, or different from, the actions predicted by more traditional assessment techniques (Greenwald, Poehlman, Uhlmann, & Banaji, 2009; Lambert, Payne, Ramsey, & Shaffer, 2005; McConnell & Liebold, 2001). This is, essentially, a question about validity. As such, these issues are relevant to important principles (e.g., the well-known “nomological net”) laid out by Cronbach and Meehl (1955) more than 50 years ago. Historical concern with reliability (internal, test-retest; cf. Cronbach, 1951) have recently been raised in this area as well. For example, researchers have probed whether such techniques are more prone to measurement error than other measures, as well as whether measurements taken at any given session are likely to be reasonably stable over time (Cunningham, Preacher, & Banaji, 2001).

The aforementioned research nicely illustrates the interrelation between methodology and theory. For example, one provocative aspect of the implicit attitude literature has been the strikingly low correlations sometimes observed between direct and indirect measures. Such dissociations are sometimes used to justify and defend the view that these measures are tapping distinct and largely independent processing systems (cf. Roediger, 1990). However, the lack of correlation between these measures can be partially attributed to the relatively low reliability of indirect measures compared with direct measures. Although this state of affairs is not optimum, there are ways of correcting for such differences in measurement error (e.g., through the use of structural equation modeling techniques). Use of such techniques has revealed that the actual correspondence between direct and indirect measures may be substantially greater than is commonly thought. As Cunningham et al. (2001) note, such results have important theoretical implications, to the extent that they may “contradict the idea of a complete dissociation between implicit and explicit attitudes” (p. 17; see also Greenwald & Nosek, 2008).

Our purpose here was not to provide an extensive cataloguing of the numerous questions that have been raised about indirect measures (see Gawronski & Payne, 2010, for an excellent overview). Rather, our intent was to drive home a larger point: Concerns about the psychometric properties of such measures (“methodology”) are inextricably connected to questions about the underlying nature of the attitudes that are supposedly tapped by them (“theory”). At the same time, good methods are not *only* about satisfying well-known criteria of validity and reliability. Below we consider some additional issues that, in our view, are equally important.

Good Methods Offer Strong Theoretical Leverage

Methodological paradigms can also be evaluated in terms of their ability to provide good *theoretical leverage*. In other words, methods can be appraised in terms of their ability to provide insight into, and the opportunity to test, one or more conceptual frameworks of interest. However, measures that have long been considered to be useful (i.e., “good”) may turn out to be less useful in the wake of important shifts in theoretical assumptions. We are certainly not the first to make this point. However, its implications are worth exploring in the specific context of research and theory in social cognition.

As one example, research and theory on impression formation was, for many years, dominated by the Asch/Anderson methodological paradigms in which participants were given a list of trait adjectives that ostensibly described a single individual (Anderson, 1971; Asch, 1946). However, the limitations of this approach started to become apparent by the early or mid-1970s, with the “cognitive revolution” within psychology (Baars, 1986). This was especially true with respect to the lack of insight these older methodologies provided about the various stages of information processing (Hastie et al., 1980). As a result, new methodological paradigms, about which we will have more to say later, developed within the field of social cognition to provide better leverage on these matters (Wyer & Srull, 1989).

There is a related sense in which methodologies can offer theoretical leverage: so-called *critical tests*, in which researchers attempt to resolve a controversy between two or more theoretical models. One important issue is whether the method or measure at hand is one for which the competing theories offer distinctly different predictions. If not, this obviously limits opportunity to distinguish between the theories in the first place. In the face of such limited leverage, researchers may of course wish to devise a methodology that can provide better leverage. However, the competing theoretical models, *themselves*, may not be amenable to a critical test in the first place. This can occur for several reasons, but one determining factor is that one or more both models may not be sufficiently well specified on a theoretical level to offer predictions that are empirically distinct from the other model. Unfortunately, such scenarios occur more often than one might think. Indeed, despite the intrinsic appeal of critical tests designed to end theoretical debates, these tests rarely result in a satisfactory resolution to the

controversy in question (Greenwald, 1975; 2012; see also Barsalou, 1990).

We have gone into some depth on this issue because it provides some key insights into the trajectory and growth of social cognition. The early years of our field were dominated by experimental paradigms designed to test the ways that people represent information about individuals and groups in long-term memory (Hastie & Kumar, 1979; Wyer & Gordon, 1984). As we shall discuss in more detail later, researchers ultimately discovered that it is relatively difficult to develop methodological paradigms that can provide clear-cut evidence for one type of mental representation over another (Barsalou, 1990). This lack of methodological traction may have been partially responsible for a shift in the theoretical and methodological approach of social cognition research, away from a central concern with mental representation, and more toward what has variously been called *co-acting subsystems* (Abelson, 1994) or *dual processing models* (Chaiken & Trope, 1999; see also Chapter 14 of the present volume).

Good Methods Are Flexible

Good methodologies are *flexible*, in the sense that they can be safely modified (i.e., without jeopardizing the integrity of their original design) to yield new insight into one or more theoretical issues. Here again, this is relevant to the growth and expansion of social cognition. For example, one strength of sequential priming tasks (Neely, 1977) is that the stimulus onset asynchrony (SOA; the length of time between the presentation of the prime and the subsequent target) can be easily modified. This flexibility allows insight into the role of controlled processes because such processes are more likely to occur if SOA is large than if it is small. This flexibility has, in turn, allowed researchers to gain valuable insight into the interplay between automatic and controlled processes (Fazio, 1990; see also Payne, Jacoby, & Lambert, 2005). Our point is not simply that SOA is related to the study of controlled processes because this is already widely known. Rather, the important issue, from our perspective, is that sequential priming procedures are unusually adaptable and that this methodological flexibility, *in and of itself*, has facilitated a great deal of research and theory on the nature of dual processing theories.

As another and fairly recent example, researchers have proposed and implemented modifications of the implicit attitude task (IAT; cf. Greenwald, McGhee, & Schwarz, 1998), such as whether participants are instructed to categorize the target stimuli

in terms of global “good versus bad” categories or, alternatively, in terms of categories that are more relevant to people’s personal attitudes (Olson & Fazio, 2004). The recognition that the IAT could be systematically modified has turned out to be valuable in its own right, providing insight into a number of broad theoretical issues in ways that would not have been possible if researchers had simply continued to use the task in a format identical to the way that it was originally proposed.

The “Catchiness” Factor

It is easy to find examples of sound methods that meet any and all criteria for methodological goodness, including but not limited to those noted above (validity, reliability, theoretical leverage, modifiability). However, some methods are more impactful—“catchier”—than others. Here, we refer not only to sheer popularity of a measure (i.e., the number of labs using methodology X), although to be sure, it is remarkable how certain methodologies are adopted more readily than others. Rather, the most impactful methodological paradigms are those that ultimately influence how researchers *think about* one or more substantive issues. What factors make any given method impactful? Or, to borrow terminology from the best-selling book, *Made to Stick* (Heath & Heath, 2007), what determines whether any given method seems to have that inherent “stickiness” of all good ideas?

As Fiske (2003) has noted, one way of predicting the impact of social psychological research is determining the extent to which it surprises us or even makes us uncomfortable, in the sense of challenging our comforting beliefs and intuitions about human nature:

Having to rearrange how one understands a corner of human nature is very annoying—and a significant sign of a classic in the making. The old classics all had that property of being obnoxious. Who wanted to know that human beings would shock each other to death, simply because a nondescript experimenter told them to do so (Milgram, 1965)? Too disturbing. Who wanted to know that people would believe their own falsehoods, simply because they had been underpaid to lie (Festinger & Carlsmith, 1959)? Very disruptive. Who wanted to know that people would fail to help someone dying, simply because they were ready to let someone else do it (Darley & Latané, 1968)? Quite disillusioning. Yet these are the counterintuitive studies that attracted many of us to social psychology. (p. 203)

Here, Fiske (2003) was primarily focused on the reasons why certain theoretical paradigms seem to “catch on” in the field of social psychology (and, perhaps, other fields as well). However, her point is relevant, too, to method. Without question, the most popular methodologies within social cognition are those that have been used to *demonstrate* these surprising and/or disturbing insights into human nature. As we discuss later, two of the more influential methodological paradigms in our field—priming and memory—were stimulated by some classic studies whose counterintuitive findings strongly challenged conventional assumptions. One could argue that it was the *theoretical implications* of these findings that were most important, not the methodological operations that produced them. We agree. However, such important theoretical findings would have never seen the light of day if the researchers had not been able to formulate methods capable of reliably generating such findings over a wide variety of different experimental settings.

A Tale of Two Paradigms: Priming and Memory

There is no single “defining” method of social cognition. Nevertheless, if one were forced to choose the types of methodological paradigms that are most *representative* of the “social cognition approach” (i.e. the methodologies that most clearly articulate what is distinctive about our field), we believe that these would probably be priming and memory (see Chapters 11 and 17 of the present volume). This point should be self-evident to anyone with even a passing familiarity with the field. However, in case anyone needs some concrete documentation, consider a list of key readings in social cognition recently presented by Hamilton (2005). Of the 32 empirical articles included in that volume, nearly all used some sort of priming or memory method. We consider these two paradigms in turn below.

Priming Paradigms

We use the term *priming paradigm* in a broad way, to include procedures designed to gain insight into the antecedents and/or consequences of mental accessibility (for an excellent review, see Bargh & Chartrand, 2000). In other words, our focus is on a complex *class* of methodological operations, as opposed to any specific type of priming task per se. Priming paradigms have always played an important role in social cognition, beginning with the now familiar “Donald paradigm” used in a series of well-known articles that appeared in the late

1970s and early 1980s (Higgins, Rholes, & Jones, 1977; Srull & Wyer, 1979). In this paradigm, the cognitive accessibility of a given trait was shown to systematically influence the interpretation of an ambiguously described target person (see also Wyer & Srull, 1981).

The actual priming procedures used in these studies were, arguably, not particularly novel. In particular, they often represented relatively modest modifications of methodological techniques that have been used by cognitive psychologists for decades. Hence, the value and impact of these paradigms was not so much that they literally represented brand new methodological innovations. Rather, their impact was driven, at least in part, by the fact that they brilliantly synthesized theory and method, insofar as they showed how the use of well-established priming paradigm could be creatively *applied* to the study of impression formation and other matters of historical interest to social psychologists.

THREE FUNCTIONAL APPLICATIONS OF PRIMING METHODOLOGIES

Although this delineation is somewhat artificial, priming paradigms (at least as they have been used in social cognition) can be clustered into three broad, somewhat overlapping categories. Beginning in the mid-1970s, priming paradigms were used in the capacity as an *independent* variable, manipulating (often with between-subjects manipulations) the relative accessibility of a given set of targeted constructs. The main thrust of this work was to show how manipulation of cognitive accessibility could have an effect on a variety of different stages of processing, including attention, encoding, and retrieval of social information. Early on, a major focus was on how manipulation of accessibility could affect how people process information about others (Bargh & Pietromonaco, 1982; Higgins et al., 1977; Srull & Wyer, 1979), but later on, this research was extended to show the consequences of such manipulations for the behavior of the self (e.g., Bargh, Chen, & Burrows, 1996; Dijksterhuis & van Knippenberg, 1998).

A second “individual difference” orientation also used priming paradigms. However, the primary focus here was on the use of such methodologies as a psychological *measure* of accessibility, rather than as a manipulation. The main assumption of this work, rooted in early theoretical work by George Kelly (1955), was that certain types of personality constructs are likely to be habitually (chronically)

accessible, owing to the centrality of those traits to the self and/or their frequent activation over time (Bargh, Bond, Lombardi, & Tota, 1986; Markus, 1977). For example, “independent self-schematics” are people for whom the trait *independent* is likely to be chronically accessible, as a basis for processing information not only about the self but about others as well.

Priming paradigms also became popular in a third way, when used in the service of assessing attitudes, including but not limited to people’s evaluation of stereotyped groups. This paradigm can be seen as a hybrid of the two approaches described above. On the one hand, this approach was concerned with the measurement of individual differences in automatic processing, in the sense that the attitudes being assessed were assumed, at least at the outset of this research, to be based on relatively stable, preexisting associations (Fazio et al., 1995; Greenwald et al., 1998; but see also Scherer & Lambert, 2009a, for a challenge to some of these assumptions). On the other hand, this paradigm also involves manipulation of prime type, although such manipulations are made on a trial-by-trial basis (i.e., within subjects) rather than between subjects. The nature of the target, too, is typically manipulated within subjects, so as to yield all possible combinations of prime and target for each participant. For example, a typical sequential priming task might manipulate the nature of the prime (e.g., black vs. white vs. control) along with target words (e.g., *hostile*, *kind*, *wonderful*, *evil*) that vary in their evaluative consistency and/or denotative relatedness to the primed category.

Hence, one can readily see that the term *priming paradigm* can mean several different things. Nonetheless, most priming paradigms are bound together by a common theme, namely that many aspects of social life are driven by automatized processes over which we have limited control and which often occur largely outside of conscious awareness. Here again, the “discomfort” factor, highlighted by Fiske (2003), becomes relevant. The large role of automaticity in human thought and action is now generally accepted by most researchers. However, the rapid growth and popularity of these priming paradigms can be attributed to the then-radical (and still unsettling) idea that many aspects of our social life are governed by processes over which we have relatively little control. This challenge to our “comfort zone” was perhaps most obvious in the field of prejudice. Here, priming methodologies showed that people’s automatized sentiments toward minority groups are often more negative than their own

personal beliefs. Such implications were first demonstrated in a classic and highly impactful study by Devine (1989), about which we will have more to say in a later section.

Explicit Memory Paradigms

As in the previous section, it is important to define our terms at the outset. By “explicit memory paradigms,” we refer to studies that used measures of memory (free recall and, to a lesser extent, recognition) in which overt queries are used to probe memories about a previous event (Richardson-Klavehn & Bjork, 1988). As with priming procedures, such measures were originally developed by cognitive psychologists. Further paralleling the trajectory of priming research, the surge of interest in these measures among *social* psychologists was driven by a set of influential articles that appeared in the late 1970s and early 1980s. Just two years after the appearance of the seminal priming article by Higgins et al. (1977), another highly influential article appeared, that by Hastie and Kumar (1979), which employed a widely used measure of memory from cognitive psychology—free recall—as its primary dependent variable. A large part of the excitement over the Hastie and Kumar (1979) article can be attributed to what was, at the time, regarded as a rather counterintuitive finding. In particular, their findings showed that people displayed better recall of information that was *inconsistent* with their overall expectations about another person. Such results were seemingly at odds with the kinds of predictions that a motivational account (say, dissonance theory) might offer, which would often predict better recall of information that was consistent, not inconsistent, with one’s prior expectations.

The Hastie and Kumar (1979) article was no less influential than that of Higgins et al. (1977). Indeed, Hastie and Kumar are widely acknowledged as helping to stimulate the emergence of what is generally known as the *person memory paradigm*. This paradigm (which, again, is best regarded as a class of methodologies, rather than any specific technique) most often involved giving participants an initial expectancy about a target person (e.g., as being a warm, kind person), followed by descriptions of behaviors (e.g., *helped an old woman cross the street*; *set a cat’s tail on fire*; *cheated on a chemistry exam*) that varied in their evaluative consistency with, and descriptive relevance to, that expectancy. The primary dependent variable in such paradigms typically included a surprise free recall task. Importance was placed not only on the types of information that

participants recalled about the target person but also on the order in which the information was recalled, the latter typically analyzed through clustering analyses (Roemer, Thompson, & Brown, 1971).

Much of the early momentum driving the emergence of the person memory paradigm reflected underlying interest in explaining why, exactly, one might find superior recall for expectation-inconsistent information and to identify the conditions under which one might find the opposite effect (i.e., a recall advantage for expectancy-consistent information). This goal led to the development of theoretical models that stipulated how overall expectations about a particular person might lead to the formation of different kinds of associative links with specific behaviors or traits, depending on their descriptive and/or evaluative consistency with those expectations (Wyer & Srull, 1989).

However, researchers working in this area eventually developed a much broader set of theoretical aspirations that went far beyond merely trying to explain why people might have better memory for information that violated their expectations. For example, this approach was employed to gain insight into the way that people represent information about various elements of their social environment (people, social categories, events) in long-term memory (e.g., Carlston, 1980; Dreben, Fiske, & Hastie, 1979; Hamilton, Katz, & Leirer, 1980; Lingle & Ostrom, 1979; Srull, Lichtenstein, & Rothbart, 1985; Wyer & Martin, 1986). Such research led, in turn, to further use of traditional memory paradigms in the service of gaining insight into other issues, such as the degree to which people make spontaneous trait attributions about others (e.g., Carlston & Skowronski, 2005; Skowronski, Carlston, Mae, & Crawford, 1998).

A CAVEAT

We do not wish to give the impression that social cognition has been literally restricted to the use of two methodological paradigms, one using priming and one using memory. Indeed, entire subdisciplines within the field, such as attribution research (e.g., Gilbert & Hixon, 1991) and research and theory on emotion (Schwarz & Clore, 2007), do not fall neatly into either of these two classes. Nevertheless, even if those areas did not literally employ priming or memory operations, the *theoretical models* relevant to those areas was heavily influenced by research that did. For example, most of what we now know about the difference between automatic and controlled processing has been afforded by the

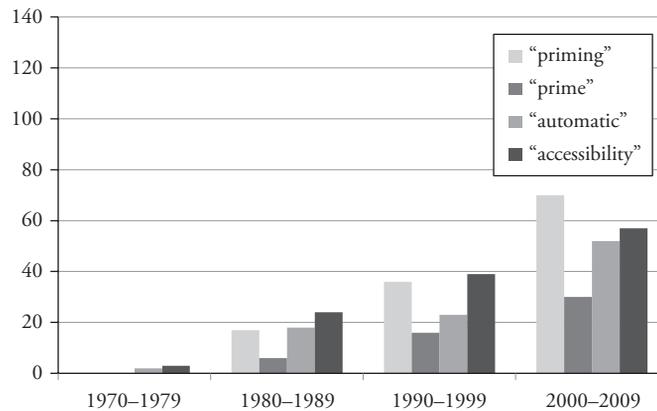
use of priming methodologies. These insights led, in turn, to an understanding of the differences between heuristic and systematic processes in attitude change and persuasion (Chen & Chaiken, 1999; Petty & Wegener, 1999). This is true even though persuasion paradigms, themselves, rarely use priming methods.

To summarize, a large part of the birth and development of social cognition has been driven—directly or indirectly—by the use of two general classes of methodological paradigms, memory and priming. Without question, these paradigms heavily dominated social cognition for at least 15 years. The more interesting question, however, is why one of these paradigms has continued to thrive and grow, whereas the other has not. We address this issue below.

On the Use of Priming Versus Explicit Memory Paradigms Across Four Decades of Social Psychological Research

During the preparation of this chapter, we wondered if there might be a way of empirically documenting the rapid rise of interest in priming and memory methodologies in the field of social psychology as a whole. As we shall show, although this endeavor confirmed some of our hunches, other aspects of this analysis produced some rather surprising results.

Such an analysis could be done in any number of ways, each with its own respective advantages and disadvantages. For our purposes, we took a fairly straightforward approach, in which we simply tracked the frequency of representative terms appearing in the abstract of articles published in all three sections of the *Journal of Personality and Social Psychology* (hereinafter, *JPSP*). This analysis was done for four separate decades (1970–1979, 1980–1989, 1990–1999, and 2000–2009). As for our focus on *JPSP*, this choice was driven by the fact that this journal is widely acknowledged as the premier outlet in social psychology as a whole and is not specifically oriented toward social cognition research. This provided us with an opportunity to show, albeit in a broad way, historical trends in the use of priming and memory paradigms in a major journal that does *not* explicitly cater to more cognitively oriented research programs. (A parallel set of comparisons was made with other top-tier journals, including *Personality and Social Psychological Bulletin* and the *Journal of Experimental Social Psychology*, and these yielded implications very similar to those reported below.)



Please provide captions for Figures 3.1.

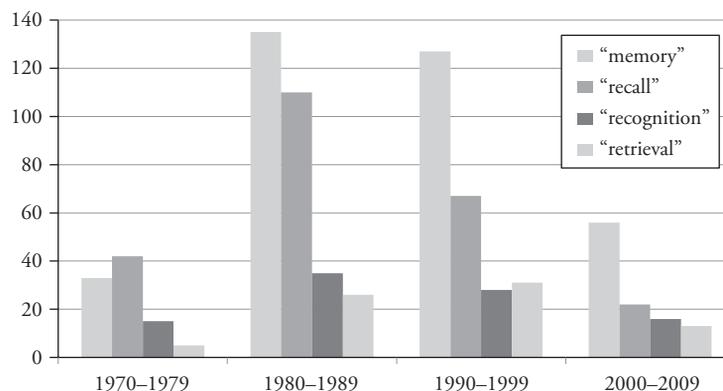
Figure 3.1

In the case of priming, we conducted separate frequency counts for two terms obviously associated with this methodology (*priming*, *prime*) along with two terms (*automatic*, *accessibility*) that lie at the heart of the processes tapped by these operations. As seen in the top part of Figure 3.1, these trends suggest a clear and consistent increase in the use of priming methodologies since 1970. Most impressive is the fact that priming methodologies (along with the relevant processes tapped by these measures) were, at least in the pages of *JPSP*, almost unheard of before 1980.

Now consider the explicit memory paradigms. In this case, we conducted an analogous frequency analysis of the general term *memory* along with two specific classes of memory measures (*recall* and *recognition*) and the obviously related term *retrieval*. The results of this analysis are shown in Figure 3.2. On the one hand, one aspect of this analysis parallels that seen with priming paradigms: Beginning in the 1980s, there was a dramatic increase in the

number of *JPSP* publications that employed traditional measures of memory. This trend is almost certainly attributable to the emergence of the aforementioned literature on person memory. It is worth noting that the person memory paradigm relied on free recall far more than recognition. If the popularity of memory studies in *JPSP* was attributable to the rise of person memory studies, then one ought to find the most dramatic increase in studies that used the former (recall) as opposed to the latter (recognition). As seen in Figure 3.2, this is in fact the case.

However, these analyses also reveal something else: a dramatic *drop* in the number of memory studies published after 2000. This decrease is particularly evident in the case of free recall. Indeed, between 2000 and 2009, the number of studies using free recall ($n = 40$) was less than *one fifth* of the number ($n = 238$) published between 1980 and 1990. Even more surprising, the number of such studies published in the past 10 years is less than half of the



Please provide captions for Figures 3.2.

Figure 3.2

number of studies published in the 10 years *before* the emergence of social cognition (1970–1979).

A Tale of Two Paradigms, Redux

What would have caused a rapid decline in research employing traditional memory paradigms while, at the same time, research relying on priming methodologies continued to thrive and grow? It initially occurred to us that this change might have reflected a *general* disenchantment with explicit memory measures not only among social psychologists but also among cognitive psychologists. This explanation is appealing in that it could also explain the simultaneous rise and sustained growth of priming paradigms that, as noted earlier, can be used as a way to gain insight into implicit memory processes.¹

As compelling as this explanation might seem, it does not appear to be true. If our analyses reflected a general loss of interest in explicit (vs. implicit) memory processes, one would expect to see a parallel decline in cognitive psychology. To investigate this possibility, we conducted an additional search for the frequency of studies using traditional memory measures (using the same search terms as noted above), but this time focusing on the *Journal of Experimental Psychology/Learning, Memory, and Cognition (JEP/LMC)*, a major outlet for the experimental study of memory in cognitive psychology. As before, we conducted these analyses across four decades (1970–1979; 1980–1989; 1990–2000; 2000–2009). These analyses revealed no evidence, whatsoever, of any decline of interest in explicit memory processes, as the frequency with which studies published in *JEP/LMC* remained virtually unchanged during this period. Hence, whatever the reason for the decline in interest in traditional memory paradigms, this decline appears to be specific to social psychology.

In our view, the key to understanding these changes is to consider how these measures were being *used* (i.e., the theoretical issues that were being probed by employing them). In the case of cognitive psychology, such measures have long been used to understand something basic about memory processes *per se*, such as those factors that can determine one's ability to remember a list of previously presented information. In other words, the memory measure was being used to understanding something about *memory*. Arguably, this was less true of social cognition research. In other words, the original reason for the explosion of interest in memory paradigms, dating back to the late 1970s, arguably

had little to do with an interest in social memory *per se*. Rather, the primary use of such measures was often as a vehicle to gain insight into the kinds of cognitive representations that people might form of single individuals as well as social categories.

This approach was extremely impressive in terms of the level of specificity it attempted to provide about the representations that people might form about their social environment, including the various levels (and sublevels) of the representations that people might form about a single individual. At the same time, researchers were well aware that any given set of “objective” information could be cognitively organized in a variety of different ways. This being the case, researchers naturally started to consider ways of formulating studies that could provide diagnostic tests for whether representational model X versus representational model Y provided the best fit to the observed data.

Around the late 1980s and early 1990s, researchers began to realize that it was difficult to test between rival (i.e., competing) models of memory representation. As Barsalou (1990) has noted, for example, there is no easy way to test the viability of prototype-based versus exemplar-based models of categorization. This is because any given model can, with only the most modest modifications, handle almost any pattern of results. A similar problem arose for the aforementioned research in social cognition, which relied heavily on memory measures to postulate the existence of rather complex representations of individual targets and/or social categories. Any given pattern of data was, more often than not, consistent with a wide range of representational models. Hence, it became difficult to test whether any given hypothesized representation was superior to other, alternative representational models. This, of course, calls into question the utility of such measures, at least in the context in which they were being used.

A second problem concerned the kinds of theoretical assumptions being made about the tests themselves. Although this was rarely stated explicitly, the prevailing assumption seemed to be that recall and recognition measures were largely free of the influence of controlled processes. This assumption turned out to be unrealistic because any given measure of memory often reflects the operation of controlled as well as automatic processes (cf. Jacoby, 1991). This realization, in turn, arguably began to erode and undercut the usefulness of these measures (again, at least in the context in which they were being employed).

A General Shift in Focus from Mental Representation to Co-acting Subsystems

The differences in trajectory for priming versus traditional memory paradigms can be seen as reflecting a more general shift in focus away from cognitive representation, in the direction of gaining insight into different types of cognitive processes. This includes but is not limited to the distinction between automatic and controlled processes. Although cognitive psychologists had been aware of the importance of this distinction for some time (cf. Schneider & Shiffrin, 1977), it was not until the mid to late 1990s that social cognition researchers really began to probe this distinction (and its potential implications) in any systematic way. One of the earliest signposts of this change appeared in 1994, when Robert Abelson suggested that “the understanding of independent, often antagonistic mental subsystems is, I believe, of great potential importance for social psychology” (Abelson, 1994, p. 27). Abelson’s prediction turned out to be prescient, as he correctly anticipated an explosion of interest in a variety of such process dualities (cf. Chaiken & Trope, 1999; Smith & Decoster, 2000).

Theoretical assumptions about process (say, about the nature of automatic vs. controlled processes) are nearly always related to assumptions about cognitive representation. Hence, we do not mean to suggest social cognition has completely lost all interest in mental representation. To choose just one example, one of the current issues to arise from this literature is whether “automatic stereotype activation” is best understood in terms of the activation of exemplars, as opposed to abstracted information about the category as a whole (De Houwer, 2001; Mitchell, Nosek, & Banaji, 2003). Such questions are relevant to a variety of choices that researchers make about the design of their studies, such as whether the task, itself, uses specific exemplars (e.g., individual pictures of black or white faces) as opposed to abstract category labels (e.g., *black*, *white*). Nevertheless, it would be fair to say that the *primary* interest in these measures is in terms of the information that they yield about process, rather than mental representation.

Earlier in this chapter we suggested (following Fiske, 2003) that such “waves of interest” often occur when research paradigms are able to generate results that not only are counterintuitive but also have the potential to make us feel uncomfortable. This is certainly true in this case as well. Indeed, Abelson (1994) proposes that part of the appeal

of research on these co-acting subsystems is that they suggest some rather disturbing disparities that could exist within any given individual, especially with respect to socially sensitive beliefs and attitudes. One of the first demonstrations of this point was made by Devine (1989), in what is generally regarded to be one of the most influential papers to appear in all of experimental social psychology during the past 50 years.

Through the use of a priming task, Devine (1989) was able to demonstrate the existence of negative, automatized associations with the category of blacks, even among participants whose own personal beliefs about this group were apparently quite positive. Although some of the specific implications of the Devine (1989) article have been modified or qualified over the years (e.g. Lepore & Brown, 1997; see also Gawronski & Payne, 2010), its impact on the field as a whole cannot be overestimated, especially in terms of effectively calling attention to the importance of considering the role of automatic versus controlled processes in driving social behavior and judgment (see also Chapter 5 of the present volume).

A Closer Look at the Methodologies Used to Test Dual Processing Models

Most researchers now accept the basic premise that mental life involves the operation of dual processes (or, to use Abelson’s phraseology, co-acting subsystems), of which the most well known is the distinction between automatic and controlled processes. But how, exactly, did psychologists come to believe that these subsystems exist? What measures and methodologies were used to demonstrate evidence for them?

Here again, there has been a heavy “borrowing” from cognitive psychology and, in particular, theory and research on observed dissociations between different types of memory measures. On historical grounds, one of the earliest discussions of such dissociations comes from a well-known case involving the Swiss neurologist Claparède (1911), who concealed a sharp pin in his fingers while shaking the hand of one of his amnesic patients. As described by Feinstein, Duff, and Tranel (2010),

The sharp pin surprised the patient and elicited a small amount of pain that quickly dissipated. Within minutes, the patient had forgotten the encounter. Yet, when Claparède tried to reintroduce himself shortly thereafter, the amnesic patient adamantly refused to shake his hand. When pressed to explain her reaction,

the patient retorted, "Is there perhaps a pin hidden in your hand?" Claparède claims, however, that even with repeated questioning the patient could never explicitly remember that she, herself, had been stuck in the hand with a pin. (p. 7674)

Hence, even though the patient displayed no conscious recollection of the traumatic incident when specifically asked, that patient's *actions* (in the form of avoidance behavior) seemed to reflect retention of some sort of memory of the doctor. In essence, then, even though the patient knew that she felt uncomfortable around the doctor, she did not appear to be able to remember the specific episode that gave rise to that discomfort.

As we can see, then, cognitive psychologists have long been aware of the potential for such memory dissociations. However, the current explosion of interest in memory dissociations can be traced, in large part, to the emergence of now-classic series of studies that appeared in the cognitive literature in the early to mid-1980s (Graf & Mandler, 1984; Graf & Schacter, 1985; Jacoby & Dallas, 1981; Schacter, 1987; see also Warrington & Weiskrantz, 1968). Without question, the impact of this research was due to the enormous impact that such studies had on the kinds of theoretical models that researchers developed in a number of different areas, including but not limited to memory. Here again, though, methodology is also important. In particular, researchers were able to develop elegant procedures that could reliably *demonstrate* such dissociations with relatively straightforward laboratory tasks. For example, participants who show poor performance in their ability to remember a previously presented word from an earlier study list (e.g., *dinosaur*) nonetheless showed significantly better than chance performance on a word stem/fragment completion (e.g., *d_n_sa_*) compared with participants who had never been exposed to that word in the first place (Graf, Squire, & Mandler, 1984).

There are at least two ways of interpreting such findings (Roediger, 1990). One, the *separate systems* view, assumes that these different types of memory tasks are thought to tap different and largely independent memory systems, which are assumed to involve the activation of neurologically distinct areas in the brain. In this case, one should expect dissociations between direct and indirect tasks because they are under the influence of largely independent memory systems. The idea of mapping task onto process has proved to be popular in the social cognition literature, especially when explaining dissociations

between traditional (e.g., pencil-and-paper) measures of attitudes and priming-based methodologies. In other words, the dissociations between these measures are often taken as evidence that the two types of tasks are tapping fundamentally different processing systems.

As intuitive as this "different tasks for different systems" view is, it is almost certainly false, at least in its strong form. In other words, it is unlikely that all measures must, necessarily, fall into one of two contrast categories that tap one or the other of two fundamentally different types of memory systems. As Roediger (1990) notes,

An alternative proposal is that many dissociations between standard explicit and implicit memory tests may reflect the operation of different cognitive procedures required by the tests. Rather than assume that implicit and explicit tests tap separate memory systems, the guiding assumption of processing theories is that memory tests are composed of various component processes and dissociations between tests reflect the operation of different processes. Because it is well-known that many variables can create dissociations between different types of explicit memory tests (such as recall and recognition) the same explanatory principles used in understanding these dissociations can be brought to bear in explaining dissociations between explicit and implicit tests. (p. 1048)

This position (often called the *processing or procedural view*) has generally received good support. One of the most significant implications of this work is that it is important to *identify the specific type of cognitive operation being measured or manipulated by any given task*. This is important because two tasks can often tap distinctly different processes, even though they might each "belong" to the same category of task type (i.e., are both considered to be explicit, or are both considered to be implicit).

A concrete example may be useful at this point. Among the many types of implicit memory tasks developed in the literature, some tend to focus on so-called lower level perceptual processes, whereas others are more relevant to higher order conceptual meaning (Cabeza, 1994). Research has shown that performance on one type of implicit task can overlap with (i.e., correlate with, or influence) another type of implicit task, provided that the tasks are sufficiently similar in terms of the kinds of processes they recruit. Conversely, dissociation effects are likely to occur, even between two seemingly similar implicit tasks, if there is a "mismatch" in the type of processes to which they are relevant. Such dissociation might

arise, for example, if one measure taps perceptual processes and the other taps conceptual mechanisms. Explicit tasks, too, can vary in terms of whether they tap perceptual or conceptual processes, and replicable dissociations can arise even between two seemingly similar explicit tasks if they are tapping different processes.

Mapping Process onto Measures, and Vice Versa: Challenges to the Process-Pure Assumption

The processing view, noted above, is sometimes misunderstood to imply that there must be a simple one-to-one correspondence between any given task and a single type of cognitive operation, in the sense that any given task recruits one and only one type of cognitive process. However, any given task can, in fact, tap more than one process. As a convenient example from the prejudice literature, consider so-called old-fashioned measures of racism (cf. McConahay, 1986), in which participants are asked to indicate their attitudinal reactions (along a Likert-type scale) toward a series of blatant racial statements (e.g., *blacks are inferior to whites*). Such measures seem worlds apart from a priming-based indirect measure of racial attitude. In other words, such tasks make no attempt, whatsoever, to disguise the fact that they are concerned with racial prejudice. Moreover, the response itself (circling a number) entails an overt, highly controllable action.

Nevertheless, several different kinds of automatic processes are likely to play an important role in guiding responses in such blatant measures of racism, despite their manifest dissimilarity with indirect measures. For one thing, the process of reading and comprehending the question, itself, involves largely automatic processes. This might seem to be a trivial point, but it is not. This is because the racial referents—*blacks* versus *whites*—are quite ambiguous, in the sense that participants could spontaneously interpret the term *blacks* to refer to a particular subgroup, such as poor, young, urban black males, without consciously realizing that they are doing so. For example, the term *blacks* could, depending on the context in which it is encountered, lead to the spontaneous activation of certain types of exemplars over others, and this could obviously affect how people answer the question. The upshot of these considerations is that even the most explicit attitude task is likely to involve a combination of automatic and controlled processes.

An analogous set of considerations applies to many IATs. As just one example, consider the

well-known weapon identification task initially developed by Payne (2001). On the one hand, the task satisfies all of the important criteria that are usually associated with implicit measures: Even though the task is designed to measure racial attitudes and beliefs, this is done in a way that avoids overtly asking participants to reveal such information. Moreover, the task itself contains several elements designed to severely restrict the capacity of participants to exert cognitive control over their responses, such as the use of extremely short SOAs. This task also employs the use of a short response window, which forces participants to respond extremely quickly.

These features allow researchers to assess the presence of automatized associations by tracking the impact of white and black primes on the frequency of errors that participants make on the target task. (Most participants are significantly more likely to misidentify a tool as a weapon when it is immediately preceded by a black as opposed to a white prime.) Even with all of these elements in place, controlled processes, too, ultimately play an important role in this type of task (Payne, 2005). Stated differently, even though the task is designed to minimize opportunity for control, it is unlikely that there is a task that literally reduces the role of cognitive control to zero. Hence, just as explicit tasks are likely to involve important roles of automatic as well as controlled processes, the same is true of implicit tasks.

Process Dissociation

The upshot of these considerations is that there is no such thing as a process-pure measure (Jacoby & Dallas, 1981). Or, at the very least, psychologists have yet to find one. Appreciation of this principle has been aided by the process dissociation procedure, as proposed by Jacoby (1991). Originally developed in the cognitive domain (largely in the context of memory research), this procedure has, in recent years, been extended and expanded to the realm of social psychology in a variety of different ways. (Readers seeking more detailed discussion of the process dissociation procedure and its potential applicability to the study of automaticity and control in social cognition may wish to consult more detailed analyses offered by Payne and Bishara (2009; see also Payne, Jacoby, & Lambert, 2005; Sherman, 2008). However, it is useful to offer some general comments on this procedure in the larger historical context of how social cognition researchers have attempted to study automaticity and control.

The process dissociation procedure (hereinafter, PDP) cannot neatly be categorized as a methodological procedure, or as an analytic technique, or as a theory of automaticity versus control. In fact, it is all of these things. The PDP is derived from the theoretical logic of creating two different kinds of within-subjects conditions in which the automatic and controlled processes either do or do not work in opposition to each other. This theoretical logic applies to all implementations of the PDP, including memory, but articulation of this logic in the context of the guns and tools task, noted above, provides a convenient example. In that task, some trials are designed so that automatic processes would foster one kind of response, whereas controlled processes would favor another. This would include, for example, trials in which a tool is paired with a black prime. Here, because the target is actually a tool, the ability of participants to attend to the properties of the target and to maintain accurate control at the response stage (i.e., respond in line with one's conscious intentions) is under the dominion of controlled processes. However, the automatized associations of weapons with blacks is what drives participants to mistakenly press the "gun" key, despite the fact that the target is, in fact, a tool. Other trials are designed so that reliance on either automatic or controlled processes would lead to the identical response. This would include, for example, trials in which black primes were paired with guns; note here that the correct response, "gun," would be made regardless of whether automatic or controlled processes were being used as a basis for responding.

Including both types of conditions in a single study affords the opportunity to derive estimates of the degree to which automatic as well as controlled processes are playing a role in a particular task. As with all statistical or analytic procedures, derivation of these estimates are based on some important assumptions, perhaps the most important of which is that automatic and controlled processes are independent (Jacoby, 1991). Although this assumption has elicited some debate over the years (Curran & Hintzman, 1995; Jacoby, Begg, & Toth, 1997), such independence is, in fact, empirically verifiable. For example, a critical aspect of the guns and tools paradigm is that manipulations influencing opportunity to exert cognitive control, such as varying SOAs, should affect values of the controlled, but not the automatic, parameter. Conversely, manipulations relevant to the accessibility of mental constructs, such as the nature of the prime being presented, should affect the automatic, but not the controlled, parameter.

The PDP provides a compelling illustration of how theoretical a good method can be. Indeed, in recent years, PDP has, in one form or another, been used to address several long-standing issues of theoretical interest to social psychologists (Conroy, Sherman, Gawronski, Hugenberg, & Groom, 2005; Ferreira, Garcia-Marques, Sherman, & Sherman, 2006; Sherman, 2008). To choose just one example from our own research, this approach allowed us to gain leverage on a long-standing ambiguity in what is one of the oldest research paradigms in social psychology: social facilitation (cf. Triplett, 1898). Briefly, the social facilitation paradigm has been concerned with the interesting consequence of public (or quasi-public) settings, namely, that they tend to increase the probability that people rely on well-learned, dominant responses. Two different explanations have been offered for such findings (cf. Baron, 1986). According to the automaticity-based explanation, public settings strengthen or augment the activation of the well-learned associations underlying the production of the dominant response in question. A different, control-based explanation assumes, instead, that the public settings erode people's ability to exert cognitive control over their responses, making it more likely that people will rely on well-learned basis for responses. In other words, the same finding—increased likelihood of dominant responses in public settings—can be explained in terms of changes in automaticity, changes in control, or changes in both kinds of processes.

Lambert, Payne, Jacoby, Shaffer, Chasteen, and Khan (2003) investigated these matters in the context of a series of studies on stereotyping and prejudice. We were drawn to this domain because social facilitation theory offers a rather counterintuitive prediction with respect to the effects of private versus public settings on the propensity of people to act in stereotypical or prejudicial ways. To the extent that stereotypes or prejudicial feelings represent a kind of dominant (i.e., well-learned) response, certain types of public settings should have the potential to increase, not decrease, the probability that people will rely on these stereotypical beliefs as a basis for judgment. This is opposite to what one might intuitively guess to be the impact of public settings, in which normative pressures to avoid the appearance of bias is presumably stronger than in private settings. Across a series of studies, Lambert et al. (2003) were able to show that public settings did in fact produce this kind of counterintuitive result. But, more importantly, use of the PDP permitted heretofore unavailable leverage in showing

that these effects were due to decreases in control rather than to changes in automaticity (see also Conrey et al., 2005).

Critiques of the Social Cognition Approach

The social cognition approach has not been without its critics (Baumeister, Vohs, & Funder, 2007; Patterson, 2008) lamenting, among other things, the proliferation of “finger movement” methodologies. We suspect that many of the readers of this handbook take it for granted that studying social psychological phenomena using cognitive methods is a legitimate endeavor. However, this view is not shared by all. Although methodological tools from cognitive psychology *can* be applied to the study of social psychological issues, the question of whether social psychologists *should* be doing so represents a lively area of debate. As one might expect, our own position on this debate is hardly unbiased. Nevertheless, dogmatic defense of any scientific discipline is rarely a good thing, and the growth and maturity of a discipline can often be facilitated by careful and thoughtful consideration of such critiques.

Given the theme of this chapter, it is perhaps fitting that we consider these critiques in a larger historical context. To begin, there is a long history of controversy with respect to the methodologies used within social psychology, with lively and often contentious debate between those who have voiced opposition to laboratory-based methodologies in social psychology (e.g., Babbie, 1975; Bannister, 1966; Gilmour & Duck, 1980; Harre & Secord, 1972; Strickland, Aboud, & Gergen, 1976) and those who have offered vigorous defense of such approaches (Anderson, Lindsay, & Bushman, 1999; Berkowitz & Donnerstein, 1982; Mook, 1983). This debate, which began to emerge in the 1960s, has surrounded several well-known experimental paradigms, including the Milgram obedience studies (Milgram, 1965), the bogus stranger attraction paradigm (Byrne, 1969), and the minimal group paradigm (Billig & Tajfel, 1973).

At least three themes emerge from this debate, including (1) laments about the general artificiality of laboratory settings; (2) concern about the use of self-reports or other pseudobehaviors, such as button pushing, that have no recognizable connection to real-life behavior; and (3) paucity of research on actual behavior between and among two or more individuals. Three viewpoints, below, provide a representative sampling of this perspective. The first was directed specifically at the bogus stranger paradigm

developed by Byrne (1969), whereas the second and third provide critiques of laboratory research more generally:

What is glaringly wrong here is the contrast between the laboratory situation, in which the action of the participants is confined to a series of dispassionate judgments based upon written descriptions, and the face-to-face situation where a person reacts to another person with warmth and friendliness and a feeling of spontaneous liking... the dissimilarity between the life situation and the laboratory situation is so marked that the laboratory experiment really tells us *nothing* about the genesis of liking and friendship among real people. (Harre & Secord, 1972; pp. 51–52, italics in original)

The greatest weakness of laboratory experiments lies in their artificiality. Social processes observed to occur within a laboratory setting might not necessarily occur within more natural social settings. (Babbie, 1975, p. 254)

In order to behave like scientists we must construct situations in which our subjects... can behave as little like human beings as possible and we do this in order to allow ourselves to make statements about the nature of their humanity. (Bannister, 1966, p. 24)

Toward a Greater Understanding of the Meaning of “Artificiality”

As a number of theorists have noted (cf. Mook, 1983), such critiques were likely to have elicited some head scratching among many practicing researchers, who take it for granted that many aspects of their methodologies are, in fact, artificial. What seemed to be missing in these critiques is the recognition that studying human behavior in the laboratory is rarely an end in and of itself. Rather, such observation is made in the service of gaining greater insight into *psychological process*. Clearly, methodologists needed a new vocabulary to make this distinction more clear.

This need was met, elegantly, by Aronson and Carlsmith (1968) through their introduction of the terms *mundane realism* and *psychological realism*. Mundane realism refers to the extent to which the actual methodological procedures in the study correspond to the kinds of experiences we have in everyday life. Many—although certainly not all—laboratory paradigms tend to be low in mundane realism, insofar as several aspects of their methods have no obvious counterpart in real life. On the other hand, *psychological realism* refers to the extent

to which the study captures underlying psychological *processes* that are occurring outside of the laboratory. Most important for our purposes, a study can be low in mundane realism but high in psychological realism. For example, the Milgram paradigm is obviously artificial. However, it has been quite successful in showing how certain elements of obedience processes can play an important role outside of the laboratory (e.g., as a way of explaining the actions of prison guards during the Holocaust).

Analogous considerations apply to many methodological paradigms in social cognition. For example, many priming methods are undeniably low in mundane realism (i.e., are “artificial”). However, these studies are designed to yield insight into aspects of automatic and controlled processes that may play an important role in driving behavior outside of the laboratory. For example, there are many real-life domains in which people’s ability—or lack thereof—to suppress unwanted impulses is extremely important, such as those pertaining to depressive rumination or to people’s efforts to suppress cravings of various sorts (cf. Wegner, 1989). Much of what we now know about suppression in these and many other domains have been derived from laboratory-based priming studies. Again, these methods are often artificial. However, when such priming studies are successful, they are successful precisely *because* they are able to offer insight into relevant processes of automaticity driving behavior in many real-life contexts.

There is another advantage to the distinction between psychological and mundane realism. In particular, it sidesteps the potential confusion arising from the use of the older term *external validity*, which had multiple meanings depending on the context in which it was used. In some contexts, external validity was used more or less synonymously with what Aronson and Carlsmith (1968) would call mundane realism: the extent to which the methodological operations resemble situations and events one sees outside of the laboratory. In other cases, external validity referred to the “generalizability” of the findings, that is, whether their implications extended beyond the specific context in which the study was conducted (e.g., whether a study conducted with adults would be replicated with children; cf. Shadish et al., 2002). Generalizability is an important issue, but it is distinct from the issue of mundane vs. psychological realism.

To summarize, there are three distinct issues at hand here, all of which speak to different aspects of method. One issue is whether any given method

does or does not resemble real-life scenarios. This is mundane realism. Another issue is whether the method is able to yield insight into processes that operate outside of the laboratory. This is psychological realism. Entirely distinct from these two types of realism, however, is a third issue, that of generalizability, which raises question as to whether the findings obtained in any given study are likely to be observed for other samples or in other contexts, not part of the original study.

A SENSE OF DÉJÀ VU?

The recent critiques of social cognition are certainly worth attending to, in the spirit of fair and open debate. Nonetheless, it is also fair to ask whether these critiques are specific to social cognition per se or whether they seem to reflect the same concern over laboratory-based artificiality seen in the aforementioned articles, published nearly 40 years ago. In our view, at least, the latter is more likely. For example, these newer critiques offer (among other things) concern about the “science of self reports and finger movements” (Baumeister, Vohs, & Funder, 2007), along with laments about the artificiality of laboratory studies and the lack of attention to actual social behavior (e.g., Furr & Funder, 2007; Hebl & Dovidio, 2005; Patterson, 2008; see also Ickes, Robertson, Tooke, & Teng, 1986; Neisser, 1980).

These authors raise a number of good points. Nevertheless, many (although perhaps not all) of the concerns being voiced by these writers seem very similar to the aforementioned critiques that emerged in the 1960s and 1970s. Curiously, though, these newer critiques make no mention of this earlier debate. This disconnect is unfortunate. Most important is that the distinction between mundane and psychological realism (Aronson & Carlsmith, 1968) helped diffuse some key misunderstandings in a similar debate 30 years ago, and it seems (to us, anyway) that these distinctions would be useful in the current debate as well. The point is not whether finger movements are any “less real” than a handshake. Human behavior comes in all shapes and sizes. The key point, to our mind, is how much we can *learn* by observing any behavior in a variety of different sorts of contexts.

It is also important to realize that various aspects of *mental* life—our thoughts, cognitions, hopes, dreams, fantasies, speculations, wishes, memories, emotions—are an important part of human psychology. In some cases, such mental activity is directly tied to behavior. In other cases, however,

the connection is less apparent. Alternatively, there may be no connection at all. We mention this point because there is a temptation to conclude that the *ultimate purpose* of social psychological studies is to understand and predict overt behavior.

To be sure, explaining and understanding behavior is very important. However, understanding behavior is not the *sole* purpose of social psychology. Here, we draw from Gordon Allport's famous (and widely accepted) definition of social psychology: an attempt to understand and explain how the *thought, feeling, and behavior* of individuals are influenced by the actual, imagined, or implied presence of others (Allport, 1985, p. 5; emphasis added). Suppose that Allport had left out "thought" and "feeling" from the foregoing passage and focused only on "behavior." If so, then one could use that perspective as justification for insisting that the validity of any study depends, ultimately, on what it tells us about behavior. However, we suspect that Allport chose his words carefully, highlighting the fact that mental life can be worthy of study in its own right, regardless of whether it is connected to "overt behavior" or not.

Looking Forward

In writing this chapter, our main goal was to provide a historical perspective of social cognition, focusing on its origins and how it has developed and grown over the years. What, then, of the future? We are mindful of the fact that people are not particularly good at predicting their own behavior (Epley & Dunning, 2000; Vallone, Griffin, Lin, & Ross, 1990). What kind of success, then, could we ever have in predicting the behavior of a whole generation of researchers? Nevertheless, it may be useful to offer a few tentative assessments about the future.

One question concerns what the future might hold with respect to research and theory on implicit attitudes and, in particular, the proliferation of different methodological techniques used to measure them. The number of such tasks has now grown to the point that, in a recent volume by Gawronski and Payne (2010), *three separate chapters* were required to address the proliferation of IATs: one for the IAT (Teige-Mocigemba, Klauer, & Sherman, 2010), a second for sequential priming tasks (Wentura & Degner, 2010), and a third for more traditional, pencil-and-paper techniques (Sekaquaptewa, Vargas, & von Hippel, 2010).

On the one hand, the proliferation of these tasks can be seen as a good thing. In other words, the development of new measurement techniques certainly has the potential to generate new insights into the

nature of implicit attitudes. On the other hand, the rapidly growing number of measures raises the risk of theoretical fragmentation and isolation because individual researchers develop preferences for using one particular task to the exclusion of others. In theory, much leverage can be gained by comparing, in a single study, the results obtained by two or more types of IATs. There are some theoretical and methodological challenges to the multimethod approach in this literature (involving, e.g., complications arising from the administration of multiple priming tasks to the same participant). However, when such an approach *is* used, it can be very effective.

At least two areas of new growth already seem to be in full stride in social cognition, with each responsible for the introduction of a new set of methodological techniques. One is the rapid growth and expansion of social neuroscience, which, as the term implies, uses methodological techniques from neuroscience (e.g., functional magnetic resonance imaging and electroencephalography) in the service of shedding further light on many of the same basic issues that have long been of interest to social cognition researchers, such as attention, categorization processes, and emotion (Amodio, 2010; Lieberman, 2010; see also Chapter 34 of the present volume).

A second area of research already showing sure signs of growth is developmental social cognition, that is, the application and extension of existing theoretical models within social cognition to the study of judgment and behavior among young children and even infants (Baron & Banaji, 2006; Dunham, Baron, & Banaji, 2007; Olson & Dunham, 2010; see also Chapter 36 of the present volume). This latter point is worth emphasizing, insofar as this extension is clearly relevant to the aforementioned issue of generalizability. In other words, setting aside debates about the supposed artificiality of laboratory settings, an important goal of social cognition research is, indeed, to investigate whether extant theoretical models can be extended to samples and methodological settings not yet considered by previous research.

A Final Word

It has been more than 25 years since the publication of the first handbook of social cognition (Wyer & Srull, 1984). That three-volume set, adorned in a bright green cover, consisted of 17 chapters, nearly all of which were concerned with the already rapid growth in substantive areas of research within the field (e.g., person memory, categorization, automatic vs. controlled processes). Within that volume

was a provocative and brilliantly integrative methodology chapter written by Thomas Srull (1984). Looking back at that chapter, it is striking to see how much has changed. These changes pertain to differences not only in the methodological tools being used by researchers but also in the kinds of questions being asked. As social cognition continues to grow and expand, we have little doubt that this will lead to the emergence of new questions along with new tools that are designed to answer them.

Author Notes

This chapter is dedicated to the spirit and creative enthusiasm of Robert S. Wyer, whose general perspective on social cognition pervades much of what we have written here. Special appreciation is expressed to Tony Greenwald and Roddy Roediger for their valuable comments on earlier drafts of this chapter. The authors also wish to gratefully acknowledge the input of several additional colleagues who offered valuable “early perspective” regarding the historical trajectory of social cognition, well before we began writing this chapter, including comments by Mahzarin Banaji, Tory Higgins, Larry Jacoby, Norbert Schwarz, Jim Sherman, and Eliot Smith.

Notes

1. The distinction between “priming tasks” and “memory tasks” is meaningful and reflects a basic distinction made in a number of different disciplines. Nevertheless, this distinction is, in one sense, artificial. In particular, one could make the case that many priming tasks are, de facto, measures of memory. For example, most if not all instances of facilitation in priming paradigms depend on some sort of retrieval of a representation stored in long-term memory. In the absence of this memorial activation, there is no priming. Also, methods that are called “memory tasks” in the cognitive literature are often referred to as “priming tasks” in the social cognition literature. However, these complications pertain mostly to the overlap between priming and *implicit* (as opposed to explicit) memory tasks. The authors wish to thank Mahzarin Banaji for her valuable perspective on this particular point.

References

Abelson, R. P. (1994). A personal perspective on social cognition. In P. G. Devine, D. L. Hamilton, & T. M. Ostrom (Eds.), *Social cognition: Impact on social psychology* (pp. 15–37). San Diego, CA: Academic Press.

Allport, G. W. (1985). The historical background of social psychology. In Lindzey, G; Aronson, E. *Handbook of social psychology*. New York: McGraw-Hill.

Amodio, D. M. (2010). Can neuroscience advance social psychological theory? Social neuroscience for the behavioral social psychologist. *Social Cognition*, 28, 695–716.

Anderson, C. A., Lindsay, J. J., & Bushman, B. J. (1999). Research in the psychological laboratory: Truth or triviality? *Current Directions in Psychological Science*, 8, 3–9.

Anderson, N. H. (1971). Integration theory and attitude change. *Psychological Review*, 78, 171–206.

Aronson, E., & Carlsmith, J. M. (1968). Experimentation in social psychology. In G. Lindzey and E. Aronson (Eds.), *Handbook of social psychology* (2nd ed., Vol. 2). Cambridge, MA: Addison Wesley.

Asch, S. E. (1946) Forming impressions of personality. *Journal of Abnormal and Social Psychology*, 41, 258–290.

Baars, B. J. (1986) *The cognitive revolution in psychology*. New York: Guilford Press.

Babbie, E. R. (1975). *The practice of social research*. Belmont, CA: Wadsworth.

Bannister, D. (1966). Psychology as an exercise in paradox. *Bulletin of the British Psychological Society*, 19, 21–26.

Bargh, J. A., Bond, R. N., Lombardi, W. J., & Tota, M. E. (1986). The additive nature of chronic and temporary sources of construct accessibility. *Journal of Personality and Social Psychology*, 50, 869–878.

Bargh, J. A., & Chartrand, T. L. (2000). The mind in the middle: A practical guide to priming and automaticity research. In H. T. Reis & C. M. Judd (Eds.), *Handbook of research methods in social and personality psychology* (pp. 253–285). New York: Cambridge University Press.

Bargh, J. A., Chen, M., & Burrows, L. (1996). Automaticity of social behavior: Direct effects of trait construct and stereotype activation on action. *Journal of Personality and Social Psychology*, 71, 230–244.

Bargh, J. A., & Pietromonaco, P. (1982). Automatic information processing and social perception: The influence of trait information presented outside of conscious awareness on impression formation. *Journal of Personality and Social Psychology*, 43, 437–449.

Baron, A., & Banaji, M. (2006). The development of implicit attitudes: Evidence of race evaluations from ages 6, 10, and adulthood. *Psychological Science*, 17, 53–58.

Baron, R. S. (1986). Distraction-conflict theory: Progress and problems. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 19, pp. 1–40). New York: Academic Press.

Barsalou, L. W. (1990). On the indistinguishability of exemplar memory and abstraction in category representation. In T.K. Srull & R.S. Wyer (Eds.), *Advances in social cognition, Vol. III: Content and process specificity in the effects of prior experiences* (pp. 61–88). Hillsdale, NJ: Erlbaum.

Baumeister, R. F., Vohs, K. D., & Funder, D. C. (2007). Psychology as the science of self-reports and finger movements: Or whatever happened to actual behavior? *Perspectives on Psychological Science*, 4, 396–403.

Beall, P. M., & Herbert, A. M. (2008). The face wins: Stronger automatic processing of affect in facial expressions than words in a modified Stroop task. *Cognition and Emotion*, 22(8), 1613–1642.

Berkowitz, L., & Donnerstein, E. (1982). External validity is more than skin deep: Some answers to criticisms of laboratory experiments. *American Psychologist*, 37, 245–257.

Billig, M., & Tajfel, H. (1973). Social categorization and similarity in intergroup behavior. *European Journal of Social Psychology*, 3, 27–51.

Byrne, D. (1969). Attitudes and attraction. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 4). New York: Academic Press.

Cabeza, R. (1994). A dissociation between two implicit conceptual tests supports the distinction between types of conceptual processing. *Psychonomic Bulletin & Review*, 1, 505–508.

Campbell, D. T., & Fiske, D. W. (1959). Convergent and discriminant validation by the multitrait-multimethod matrix. *Psychological Bulletin*, 56, 81–105.

- Carlston, D. E. (1980). The recall and use of traits and events in social inference processes. *Journal of Experimental Social Psychology*, *16*, 303–328.
- Carlston, D. E., & Skowronski, J. J. (2005). Linking versus thinking: Evidence for the different associative and attributional bases of spontaneous trait transference and spontaneous trait inference. *Journal of Personality and Social Psychology*, *89*, 884–898.
- Chaiken, S., & Trope, Y. (1999). *Dual-process theories in social psychology*. New York: Guilford Press.
- Chen, S., & Chaiken, S. (1999). The heuristic-systematic model in its broader context. In S. Chaiken & Y. Trope (Eds.), *Dual-process theories in social and cognitive psychology* (pp. 73–96). New York: Guilford Press.
- Claparède, E. (1911/1951): Recognition and me-ness. In: D. Rapaport (Ed.), *Organization and pathology of thought: Selected sources* (pp. 58–75). New York: Columbia University Press.
- Conroy, F. R., Sherman, J. W., Gawronski, B., Hugenberg, K., & Groom, C. J. (2005). Separating multiple processes in implicit social cognition: The Quad Model of Implicit Task Performance. *Journal of Personality and Social Psychology*, *89*(4), 469–487.
- Cronbach, L. J. (1951). Coefficient alpha and the internal structure of tests. *Psychometrika*, *16*, 297–334.
- Cronbach, L. J., & Meehl, P. E. (1955). Construct validity in psychological tests. *Psychological Bulletin*, *52*, 281–302.
- Cunningham, W. A., Preacher, K. J., & Banaji, M. R. (2001). Implicit attitude measures: Consistency, stability, and convergent validity. *Psychological Science*, *12*, 163–170.
- Curran, T., & Hintzman, D. L. (1995). Violations of the independence assumption in process dissociation. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *21*, 531–547.
- De Houwer, J. (2001). A structural and process analysis of the Implicit Association Test. *Journal of Experimental Social Psychology*, *37*, 443–451.
- De Houwer, J. (2006). What are implicit measures and why are we using them. In R. W. Wiers & A. W. Stacy (Eds.), *The handbook of implicit cognition and addiction* (pp. 11–28). Thousand Oaks, CA: Sage.
- De Houwer, J., & Hermans, D. (1994). Differences in the affective processing of words and pictures. *Cognition and Emotion*, *8*, 1–20.
- Devine, P. G. (1989). Stereotypes and prejudice: Their automatic and controlled components. *Journal of Personality and Social Psychology*, *56*, 5–18.
- Devine, P. G., Hamilton, D. L., & Ostrom, T. M. (Eds.) (1994). *Social cognition: Impact on social psychology*. San Diego, CA: Academic Press.
- Diener, E. (2009). *The science of well-being: The collected works of Ed Diener. Social Indicators Research Series* (Vol. 37). The Netherlands: Springer.
- Dijksterhuis, A., & van Knippenberg, A. (1998). The relation between perception and behavior or how to win a game of Trivial Pursuit. *Journal of Personality and Social Psychology*, *74*, 865–877.
- Dreben, E. K., Fiske, S. T., & Hastie, R. (1979). The independence of item and evaluation information: Impression and recall order effects in behavior-based impression formation. *Journal of Personality and Social Psychology*, *37*, 1758–1768.
- Dunham, Y., Baron, A., & Banaji, M. R. (2007). Children and social groups: A developmental analysis of implicit consistency in Hispanic Americans. *Self and Identity*, *6*(2–3), 238–255.
- Epley, N., & Dunning, D. (2000). Feeling “holier than thou”: Are self-serving assessments produced by errors in self or social psychology? *Journal of Personality and Social Psychology*, *79*, 861–875.
- Fazio, R. H. (1990). Multiple processes by which attitudes guide behavior: The MODE model as an integrative framework. In M. P. Zanna (Ed.), *Advances in experimental social psychology* (Vol. 23, pp. 75–110). New York: Academic Press.
- Fazio, R. H., Jackson, J. R., Dunton, B. C., & Williams, C. J. (1995). Variability in automatic activation as an unobtrusive measure of racial attitudes: A bona fide pipeline? *Journal of Personality and Social Psychology*, *69*, 1013–1027.
- Feinstein, J. S., Duff, M. C., & Tranel, D. (2010). The sustained experience of emotion after loss of memory in patients with amnesia. *Proceedings of the National Academy of Sciences*, *107*, 7674–7679.
- Ferreira, M. B., Garcia-Marques, L., Sherman, S. J., & Sherman, J. W. (2006). Automatic and controlled components of judgment and decision making. *Journal of Personality and Social Psychology*, *91*(5), 797–813.
- Fiske, S. T. (2003). The discomfort index: How to spot a really good idea whose time has come. *Psychological Inquiry*, *14*, 201–206.
- Furr, R. M., & Funder, D. C. (2007). Behavioral observation. In R. Robins, C. Fraley, & R. Krueger (Eds.), *Handbook of research methods in personality psychology* (pp. 273–291). New York: Guilford Press.
- Gawronski, B., & Payne, B. (Eds.) (2010). *Handbook of implicit social cognition: Measurement, theory, and applications*. New York: Guilford Press.
- Gilbert, D. T., & Hixon, J. G. (1991). The trouble of thinking: Activation and application of stereotypic beliefs. *Journal of Personality and Social Psychology*, *60*, 509–517.
- Gilmour, R., & Duck, S. (1980). *The development of social psychology*. London: Academic Press.
- Graf, E., Squire, L. R., & Mandler, G. (1984). The information that amnesic patients do not forget. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *10*, 164–178.
- Graf, P., & Mandler, G. (1984). Activation makes words more accessible, but not necessarily more retrievable. *Journal of Verbal Learning and Verbal Behavior*, *23*, 553–568.
- Graf, P., & Schacter, D. L. (1985). Implicit and explicit memory for new associations in normal and amnesic subjects. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *11*, 501–551.
- Greenwald, A. G. (1975). On the inconclusiveness of “crucial” cognitive tests of dissonance versus self-perception theories. *Journal of Experimental Social Psychology*, *11*, 490–499.
- Greenwald, A. G. (2012). There is nothing so theoretical as a good method. *Perspectives on Psychological Science*, *7*, 99–108.
- Greenwald, A. G., McGhee, D. E. & Schwartz, J. L. (1998). Measuring individual differences in implicit cognition: The implicit association test. *Journal of Personality and Social Psychology*, *74*, 1464–1480.
- Greenwald, A. G., & Nosek, B. A. (2008). Attitudinal dissociation: What does it mean? In R. E. Petty, R. H. Fazio, & P. Briñol (Eds.), *Attitudes: Insights from the new implicit measures* (pp. 65–82). Hillsdale, NJ: Erlbaum.
- Greenwald, A. G., Poehlman, T. A., Uhlmann, E. L., & Banaji, M. R. (2009). Understanding and using the Implicit Association Test: III. Meta-analysis of predictive validity. *Journal of Personality and Social Psychology*, *97*, 17–41.

- Hamilton, D. (Ed.). (2005). *Social cognition: Key readings*. New York: Psychology Press.
- Hamilton, D. L., Katz, L. B., & Leirer, V. O. (1980). Organizational processes in impression formation. In R. Hastie, T. M. Ostrom, E. B. Ebbesen, R. S. Wyer, D. L. Hamilton, & D. E. Carlston (Eds.), *Person memory: The cognitive basis of social perception* (pp. 121–153). Hillsdale, NJ: Erlbaum.
- Harre, R., & Secord, P. F. (1972). *The explanation of social behaviour*. Oxford, UK: Blackwell.
- Hastie, R., & Kumar, P. A. (1979). Person memory: Personality traits as organizing principles in memory for behaviors. *Journal of Personality and Social Psychology*, 37, 25–38.
- Hastie, R., Ostrom, T. M., Ebbesen, E. B., Wyer, R. S., Hamilton, D. L., & Carlston, D. E. (Eds.) (1980). *Person memory: The cognitive basis of social perception*. Hillsdale, NJ: Erlbaum.
- Hebl, M., & Dovidio, J. F. (2005). Promoting the “social” in the examination of social stigmas. *Personality and Social Psychological Review*, 9, 156–182.
- Heath, C., & Heath, D. (2007). *Made to Stick: Why Some Ideas Survive and Others Die*. Random House.
- Higgins, E. T., Rholes, W. S., & Jones, C. R. (1977). Category accessibility and impression formation. *Journal of Experimental Social Psychology*, 13, 141–154.
- Ickes, W., Robertson, E., Tooke, W., & Teng, G. (1986). Naturalistic social cognition: Methodology, assessment, and validation. *Journal of Personality and Social Psychology*, 51, 66–82.
- Jacoby, L. L. (1991). A process dissociation framework: Separating automatic from intentional uses of memory. *Journal of Memory and Language*, 30, 513–541.
- Jacoby, L. L., Begg, I. M., & Toth, J. P. (1997). In defense of functional independence: Violations of assumptions underlying the process–dissociation procedure? *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 23, 484–495.
- Jacoby, L. L., & Dallas, M. (1981). On the relationship between autobiographical and perceptual learning. *Journal of Experimental Psychology: General*, 110, 306–334.
- Jones, C. R., & Fazio, R. H. (2010). Person categorization and automatic racial stereotyping effects on weapon identification. *Personality and Social Psychology Bulletin*, 36, 1073–1085.
- Kelly, G. (1955). *The psychology of personal constructs* (Vols. I, II). New York: W. W. Norton.
- Klauer, K. C. & Musch, J. (2003). Affective priming: Findings and theories. In J. Musch & K. C. Klauer (Eds.), *The psychology of evaluation: Affective processes in cognition and emotion* (pp. 7–50). Mahwah, NJ: Erlbaum.
- Kraus, M. W., & Chen, S. (2009). Striving to be known by significant others: Automatic activation of self-verification goals in relationship contexts. *Journal of Personality and Social Psychology*, 97, 58–73.
- Lambert, A. J., Payne, B. K., Jacoby, L. L., Shaffer, L. M., Chasteen, A. L., & Khan, S. K. (2003). Stereotypes as dominant responses: On the “social facilitation” of prejudice in anticipated public contexts. *Journal of Personality and Social Psychology*, 84, 277–295.
- Lambert, A. J., Payne, B. K., Ramsey, S., & Shaffer, L. M. (2005). On the predictive validity of implicit attitude measures: The moderating effect of perceived group variability. *Journal of Experimental Social Psychology*, 41, 114–128.
- Lepore, L., & Brown R. (1997). Category and stereotype activation: Is prejudice inevitable? *Journal of Personality and Social Psychology*, 72(2), 275–287.
- Lewin, K. (1951) *Field theory in social science: Selected theoretical papers*. In: D. Cartwright (Ed.). New York: Harper & Row.
- Lieberman, M. D. (2010). Social cognitive neuroscience. In : S. T. Fiske, D. T. Gilbert, & G. Lindzey (Eds.), *Handbook of social psychology* (5th ed., pp. 143–193). New York: McGraw-Hill.
- Lingle, J. H., & Ostrom, T. M. (1979). Retrieval selectivity in memory based impression judgments. *Journal of Personality and Social Psychology*, 37, 180–194.
- Markus, H. (1977). Self-schemata and processing information about the self. *Journal of Personality and Social Psychology*, 35, 63–78.
- McConnell, A. R., & Liebold, J. M. (2001). Relations between the implicit association test, explicit racial attitudes, and discriminatory behavior. *Journal of Experimental Social Psychology*, 37, 435–442.
- McConahay, J. B. (1986). Modern racism, ambivalence, and the Modern Racism Scale. In J. Dovidio & S. Gaertner (Eds.), *Prejudice, discrimination, and racism* (pp. 91–125). Orlando, FL: Academic Press.
- Meyer, D. E., & Schvaneveldt, R. W. (1971). Facilitation in recognizing pairs of words: Evidence of a dependence between retrieval operations. *Journal of Experimental Psychology: General*, 90, 227–234.
- Milgram, S. (1965). Some conditions of obedience and disobedience to authority. *Human Relations*, 18(1), 57–76.
- Mitchell, J. P., Nosek, B. A., & Banaji, M. R. (2003). Contextual variations in implicit evaluation. *Journal of Experimental Psychology: General*, 132, 455–469.
- Mook, D. G. (1983). In defense of external invalidity. *American Psychologist*, 38, 379–387.
- Neely, J. H. (1977). Semantic priming and retrieval from lexical memory: Roles of inhibitionless spreading activation and limited-capacity attention. *Journal of Experimental Psychology: General*, 106, 226–254.
- Neisser, U. (1980). On “social knowing.” *Personality and Social Psychology Bulletin*, 6, 601–605.
- Olson, K. R., & Dunham, Y. (2010). The development of implicit social cognition. In B. Gawronski and B. Payne (Eds.), *Handbook of implicit social cognition: Measurement, theory, and applications* (pp. 241–254). New York, NY: Guilford Press.
- Olson, M. A., & Fazio, R. H. (2004). Reducing the influence of extra-personal associations on the Implicit Association Test: Personalizing the IAT. *Journal of Personality and Social Psychology*, 86, 653–667.
- Patterson, M. L. (2008). Back to social behavior: Mining the mundane. *Basic and Applied Social Psychology*, 30, 93–101.
- Payne, B. K. (2001). Prejudice and perception: The role of automatic and controlled processes in misperceiving a weapon. *Journal of Personality and Social Psychology*, 81, 181–192.
- Payne, B. K. (2005). Conceptualizing control in social cognition: How executive control modulates the expression of automatic stereotyping. *Journal of Personality and Social Psychology*, 89, 488–503.
- Payne, B., & Bishara, A. J. (2009). An integrative review of process dissociation and related models in social cognition. *European Review of Social Psychology*, 20, 272–314.
- Payne, B. K., Jacoby, L. L., & Lambert, A. J. (2005). Attitudes as accessibility bias: Dissociating automatic and controlled components. In R. Hassin, J. Bargh, J. & Uleman, (Eds.), *The New Unconscious*. New York: Oxford University Press.
- Petty, R. E., & Wegener, D. T. (1999). The Elaboration Likelihood Model: Current status and controversies. In

AQ:
Please
provide
place of
publi-
cation.

- S. Chaiken & Y. Trope (Eds.), *Dual process theories in social psychology* (pp. 41–72). New York: Guilford Press.
- Richardson-Klavehn, A., Bjork, R. A. (1988): Measures of memory. *Annual Review of Psychology*, *36*, 475–543.
- Roediger, H. L. (1990). Implicit memory: Retention without remembering. *American Psychologist*, *45*, 1043–1056.
- Roenker, D. L., Thompson, C. P., & Brown, S. C. (1971). Comparison of measures for the estimation of clustering in free recall. *Psychological Bulletin*, *76*, 45–48.
- Schacter, D. L. (1987). Implicit memory: History and current status. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *13*, 501–518.
- Scherer, L. D., & Lambert, A. J. (2009a). Contrast effects in priming paradigms: Implications for theory and research on implicit attitudes. *Journal of Personality and Social Psychology*, *97*, 383–403.
- Scherer, L. D., & Lambert, A. J. (2009b). Counterstereotypic exemplars in context: Evidence for intracategory differentiation using implicit measures. *Social Cognition*, *27*, 523–550.
- Schneider, W., & Shiffrin, R. M., (1977). Controlled and automatic human information processing: Detection, search, and attention. *Psychological Bulletin*, *84*, 1–66.
- Schwarz, N., & Clore, G. L. (2007). Feelings and phenomenal experiences. In E. T. Higgins & A. Kruglanski (Eds.), *Social psychology: Handbook of basic principles* (2nd ed., pp. 385–407). New York: Guilford Press.
- Sekaquaptewa, D., Vargas, P., & von Hippel, W. (2010). A practical guide to paper and pencil implicit measures of attitudes. In B. Gawronski & B. K. Payne (Eds.), *Handbook of implicit social cognition: Measurement, theory and applications*. New York: Guilford Press.
- Shadish, W. R., Cook, T. D., & Campbell, D. T. (2002). *Experimental and quasi-experimental designs for generalized causal inference*. Boston: Houghton-Mifflin.
- Sherman, J. W. (2008). Controlled influences on implicit measures: Confronting the myth of process-purity and taming the cognitive monster. In R. E. Petty, R. H. Fazio, P. Briñol, R. E. Petty, R. H. Fazio, & P. Briñol (Eds.), *Attitudes: Insights from the new implicit measures* (pp. 391–426). New York: Psychology Press.
- Skowronski, J. J., Carlston, D. E., Mae, L., & Crawford, M. T. (1998). Spontaneous trait transference: Communicators take on the qualities they describe in others. *Journal of Personality and Social Psychology*, *74*, 837–848.
- Smith, E. R., & DeCoster, J. (2000). Dual-process models in social and cognitive psychology: Conceptual integration and links to underlying memory systems. *Personality and Social Psychology Review*, *4*, 108–131.
- Strull, T. K. (1984). Methodological techniques for the study of person memory and social cognition. In R. S. Wyer, Jr., & T. K. Srull (Eds.), *Handbook of social cognition* (Vol. 2, pp. 1–72). Hillsdale, NJ: Erlbaum.
- Strull, T. K., Lichtenstein, M., & Rothbart, M. (1985). Associative storage and retrieval processes in person memory. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *11*, 316–345.
- Strull, T. K., & Wyer, R. S. (1979). The role of category accessibility in the interpretation of information about persons: Some determinants and implications. *Journal of Personality and Social Psychology*, *37*, 1660–1672.
- Strickland, L. J., Aboud, F. E., & Gergen, K. J. (1976). (Eds.), *Social psychology in transition*. New York: Plenum Press.
- Teige-Mocigemba, S., Klauer, K. C., & Sherman, J. W. (2010). Practical guide to Implicit Association Task and related tasks. In B. Gawronski & B. K. Payne (Eds.), *Handbook of implicit social cognition: Measurement, theory, and applications* (pp. 117–139). New York: Guilford Press.
- Triplett, N. (1898). The dynamogenic factors in pacemaking and competition. *American Journal of Psychology*, *9*, 507–533.
- Vallone, R. P., Griffin, D. W., Lin, S., & Ross, L. (1990). Overconfident prediction of future actions and outcomes by self and others. *Journal of Personality and Social Psychology*, *58*, 582–592.
- Warrington, E. K., & Weiskrantz, L. (1968). New method of testing long-term retention with special reference to amnesic patients. *Nature*, *217*, 972–974.
- Wegner, D. M. (1989). *White bears and other unwanted thoughts: Suppression, obsession, and the psychology of mental control*. New York: Viking/Penguin.
- Wentura, D., & Degner, J. (2010). A practical guide to sequential priming and related tasks. In B. Gawronski, & B. K. Payne (Eds.), *Handbook of implicit social cognition: Measurement, theory, and applications* (pp. 95–116). New York: Guilford Press.
- West, R. F. & Stanovich, K. E. (1982). The source of inhibition in experiments on the effects of sentence context on word recognition. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *8*, 385–399.
- Wyer, R. S., & Gordon, S. E. (1984). The cognitive representation of social information. In R. S. Wyer & T. K. Srull (Eds.), *Handbook of social cognition* (pp. 73–150). Hillsdale, NJ: Erlbaum.
- Wyer, R. S., & Martin, L. L. (1986). Person memory: The role of traits, group stereotypes and specific behaviors in the cognitive representation of persons. *Journal of Personality and Social Psychology*, *50*, 611–675.
- Wyer, R. S., & Srull, T. K. (1981). Category accessibility: Some theoretical and empirical issues concerning the processing of social stimulus information. In E. T. Higgins, C. P. Herman, & M. P. Zanna (Eds.), *Social cognition: The Ontario symposium* (Vol. 1, pp. 161–197). Hillsdale, NJ: Erlbaum.
- Wyer, R. S., & Srull, T. K. (Eds.) (1984). *Handbook of social cognition* (Vols. 1–3). Hillsdale, NJ: Erlbaum.
- Wyer, R. S., & Srull, T. K. (1989). Memory and cognition in its social context. Hillsdale, NJ: Erlbaum.