

All Aboard! ‘Social’ and Nonsocial Priming are the Same Thing

John A. Bargh
Yale University

Citation: Bargh, J. A. (2021). All aboard! Social and nonsocial priming are the same thing. *Psychological Inquiry*, 32, 29-34.

[Commentary on target article by Sherman & Rivers, “There’s nothing social about social priming”, in same issue of *Psychological Inquiry*.]

Sherman and Rivers make a laudable effort to reconcile and heal a divide within psychology (see also Klatzky & Cresswell, 2014) and we are all indebted to their initiative. The goal of this commentary is to supplement their analysis by reinforcing and expanding their main point, that ‘social’ and ‘nonsocial’ priming are the same thing. The fictional nature of the social vs. non-social priming distinction becomes even more obvious when we consider some relevant background on the origins of social priming research. That is, the notion of ‘priming’ existed in the verbal learning research domain (with effects that lasted many minutes, not just a second or so) well before the same term was used by Meyer and Schvaneveldt (1971) in their spreading activation research (in which DOCTOR primed NURSE, and OCEAN primed WATER, in a lexical decision task). Moreover, application of the “social priming” label is in some cases even more arbitrary than Sherman and Rivers suggest; there are many examples of “social priming” studies that are not labeled as such — and thus have received relatively little skepticism — simply because the researchers who performed them are not considered social psychologists.

Dissolving the arbitrary boundary between social and non-social priming has important implications for debates about the reality of social priming. Specifically, it forces anyone who denies the reality of social priming writ large to deny the reality of priming in general, including so-called “non-social” priming, because there is no natural boundary between the two classes of phenomena. By the same token, any skeptic who wishes to accept the reality of “non-social” priming cannot deny the reality of “social priming” in general, and must instead take the weaker position of denying the reality of a specific subset of findings. Such a skeptic could argue that particular experiments fail to replicate, but not be permitted to then generalize this to the conclusion that all experiments conventionally labeled “social priming” do not replicate. This is important, as many skeptics wish to make strong, eliminativist claims about social priming

without saddling themselves with the burden of also disproving phenomena like semantic priming, implicit memory, automatic approach-avoid effects, behavioral mimicry, and so forth. Such selectivity is not an option.

1. “Social” priming is an implicit memory effect, not a sequential priming effect

My dissertation research circa 1980 was inspired by the original Higgins et al. (1977) ‘social priming’ study. I then spent the next decade in the same social psychology program at NYU and collaborated with Higgins on several studies on the underlying mechanism and time course of these ‘social priming’ effects (Bargh et al., 1986; Bargh et al., 1988; Higgins & Bargh, 1987; Higgins et al., 1985; Lombardi et al., 1987). Based on this first-hand experience, I can report that the roots of these studies were not the Meyer and Schvaneveldt (1971) sequential priming studies, but the Segal and Cofer (1960; see Segal, 1967) demonstration of carry-over implicit memory effects of stimuli encountered in one experiment into an ostensibly unrelated ‘second experiment’. This they called ‘priming’ (see also Grand & Segal, 1966; Koriat & Feuerstein, 1976). [There is a fascinating historical sidebar here, because this first public reference to carry-over verbal priming effects was made at a Monday morning paper session of the 1960 American Psychological Association annual convention, in Chicago’s Morrison Hotel. The person who gave the paper right before Segal in that session, and so was present at the birth of verbal priming research, was a new Stanford assistant professor named Gordon Bower.]

The paradigm that Higgins et al. (1977) used was based on a ‘memory-word’ paradigm developed by Segal in which words presented in one experiment were then more likely to be used as responses in a second, ostensibly unrelated experiment – even when the participant failed to recall these words (Grand & Segal, 1966). That is, the stimulus words had an effect on subsequent responses in the absence of explicit recall – the definition of an *implicit memory*

effect. That explicit memory for the stimulus words was not necessary to produce these carryover verbal priming effects into the ‘second experiment’ of the study was quickly verified by Warrington and Weiskrantz (1968, 1974; and later, by Schacter & Graf, 1986). The same verbal priming effects first reported by Segal and Cofer (1960), in an ‘unrelated studies’ paradigm that became the basis for many subsequent ‘social priming’ studies (see Bargh & Chartrand, 2000), remained intact in densely amnesic patients who had no explicit memory of the priming words.

As Sherman and Rivers note, it was the ‘long’ duration of social priming effects that was an original reason given by skeptics for questioning their validity (e.g., C. Harris et al., 2013). According to this argument, social priming effects that lasted many minutes, from one experimental task to the next, couldn’t possibly be real because priming effects in cognitive psychology – namely, in sequential priming tasks such as Meyer & Schvaneveldt (1971) – were fleeting and lasted less than a second. Klatzky and Creswell (2014) in their efforts – unique at the time -- to reconcile sequential priming with ‘social priming’ echoed this prevailing understanding about ‘social priming’ within cognitive psychology: “The term priming has its origins in research concerned with the spread of activation from one concept or neural site to another (e.g., Meyer & Schvaneveldt, 1971).”

But as we’ve seen, this was not true -- the term ‘priming’ had its origins in the verbal priming, implicit memory research of Segal a decade earlier. And in fact, the term ‘priming’ predates even the verbal learning researchers of the 1960s, because it was first used in psycholinguistics by Karl Lashley in 1951 to refer to a preparatory buffer state in the rapid production of spoken language. Tory Higgins, who performed the first ‘social priming’ study (Higgins, Rholes, & Jones, 1977) was well aware of Lashley’s concept of response priming, as his graduate training at Columbia was in psycholinguistics (e.g., Huttenlocher & Higgins, 1972).

The Higgins et al. (1977) study was based on the verbal learning, ‘unrelated studies’ paradigm and not the sequential priming task. Participants were exposed to synonyms of certain personality traits as part of a first, memory experiment – they had to hold a specific word in mind while they named the background color of a slide, on each trial. Next, in what participants believed to be an unrelated experiment, they read about a target person named Donald who behaved in ways ambiguously related to the primed traits, such as sailing across the ocean alone, or preferring to study by himself instead of with classmates. Those participants who had been exposed to memory words such as ‘adventurous’ and ‘independent’ formed more positive impressions of Donald than did participants who had been previously exposed to relevant terms such as ‘reckless’ and ‘aloof’. A separate control experiment showed that participants could recall about half of the memory words at the end of the color-naming first task, equal proportions of trait words and control words.

Later follow-up experiments focused on whether these effects were implicit or explicit memory effects. If they were explicit, and depended on conscious awareness and memory for the priming stimuli, then they might be due to experimental demand. But the evidence was clearly on the side of being implicit memory effects, for two reasons. First, participants evidenced no awareness of having been influenced by their prior exposure to trait terms in the ostensibly unrelated “first experiment”. For example, in careful post-experimental debriefing, less than 5% of the participants thought that their work on the priming task had influenced their responses on the impression task in some way, none of these participants could correctly state the experimental hypothesis, and even so these participants were replaced in the design and their data removed from the analyses (Higgins et al., 1985, p. 64). And secondly, trait priming effects

on liking judgments were, if anything, stronger in these studies for participants who could not later recall the primes than for those who could (Higgins et al., 1985; Lombardi et al., 1987).

Thus, these early priming studies were forerunners of the notion of implicit memory, in that stimuli that could not be explicitly recalled nonetheless had influences on subsequent responses (e.g., Schacter, 1987). The difference in duration of the effects in “social” versus cognitive sequential priming studies that Sherman and Rivers raise as an issue therefore may just be due to the fact that one is a memory phenomenon and the other is a spreading activation effect. Even short-term implicit memory effects last considerably longer than a second or so.

The design and conduct of the ‘social priming’ studies also support the conclusion that the ‘social priming’ studies involved implicit and not explicit memory effects. Researchers were careful to probe for any explicit memory for the priming stimuli, they removed data from participants who showed even a hint of awareness of a possible influence of the priming task on the subsequent task, and demonstrated that the priming effects were stronger for those participants who could not later recall any primes than for those who could. Also, most social priming studies were careful to take procedural steps to make sure the experimenters were blind to the participant’s randomly assigned condition, for example by having a third party perform the random assignment and deliver the priming manipulation (or control task) to participants via a sealed envelope (Bargh et al., 1996, Experiment 2). These procedural safeguards ensured that the priming effects did not depend on the participant’s awareness of the hypothesis or even any explicit recall of the priming stimuli, contradicting the later claims that they were ‘Clever Hans’ effects (Doyen et al., 2012; Yong, 2012) that required awareness of hypothesis by the participant, or awareness of participant condition by the experimenter.

No one familiar with the research evidence questions implicit memory *per se* as a phenomenon. These effects are very robust and remain intact even when explicit or conscious processing is damaged and not present (Reber, 1992), as in alcoholism, stroke, trauma, disease, tumors and other disorders (e.g., Johnson et al., 1985; Shimamura, 1986; Shimamura et al., 1987). Again, the same verbal priming effects discovered by Segal and which were the basis for subsequent ‘social priming’ studies, were also demonstrated in densely amnesic patients despite the complete absence of any explicit memory for the priming words (Warrington & Weiskrantz, 1968, 1974; Schacter & Graf, 1986).

Because social priming studies are actually implicit memory studies, if one accepts the reality of implicit memory, then one cannot reject social priming wholesale. Of course, this does not mean accepting the reality of all findings in the “social priming” literature; no one should doubt that a nontrivial proportion of these findings are false positives, since false positives are inevitable consequences of our statistical methods, and for many decades were made more likely by a reliance on small sample sizes. The point is that rejecting social priming in general necessarily entails rejecting many other broad classes of phenomena, including implicit memory. This is one of Sherman and Rivers’ main points: that social and non-social priming studies are the same thing. They follow the same procedures and use the same types of stimuli. Both forms involve presentation of priming stimuli in one context, such as a ‘first experiment’, and show the effects of these stimuli in a second, purportedly unrelated experiment. Any distinction between social and non-social priming then becomes arbitrary, and without basis in fact.

And so priming skeptics have a choice. If social and nonsocial priming are the same thing, then if one is fictional, both are fictional. Skeptics of ‘social’ priming must then also be skeptical of the nonsocial verbal priming and implicit memory effects in the literature. If on the

other hand one accepts the reality of nonsocial priming in general, based on the extensive evidence that verbal priming and implicit memory results are robust and reliable phenomena, then one cannot reject social priming in general. There are only two options for skeptics here: to take a strong stance against priming writ large or to make weaker inferences from failed replications. The choice is theirs, but the options are not.

2. ‘Social priming’ is just priming done by social psychologists

As further evidence of the arbitrary nature of the distinction between social and nonsocial priming, many priming studies are not labeled “social priming” despite using methods nearly identical to those from canonical “social priming” studies. All that distinguishes these studies from “social priming” studies is that the researchers who conducted them are not categorized as social psychologists. For example, decision scientist Elke Weber and her colleagues (Zaval et al., 2014) showed that the current weather conditions affected survey responses on the dangers of global warming – it was rated as more of a threat on warmer days, and less of a threat on cooler days. These researchers conceptualized the effect of the current weather as a priming effect – and did an additional experiment showing that verbal priming with words related to warm or cold produced the same effect on survey responses as did the day’s weather conditions. They argued that the effect of the current weather on attitudes towards global warming is through its priming effect on the same internal warm or cold concept that the prime words had activated.

Behavioral economist Ernst Fehr and colleagues showed in several studies that priming a person’s different identities changes their motivations and their moral behavior. In one study, Cohn et al. (2014) sent an email to investment bankers at home on the weekend. They randomly assigned the bankers to either first type in a description of their weekday office environment, or a control environment unrelated to their work. Then all participants engaged in a coin toss game

in which they were substantially rewarded for the number of heads they tossed – but tossed them by hand and reported, on the honor system, how many heads they had tossed. In the control condition the bankers were actually quite honest, and the distribution of heads they had tossed closely followed the binomial theorem, or what would be expected purely by chance. The workplace primed bankers, on the other hand, reported significantly more heads than expected by chance, and more than the control condition reported. Fehr and colleagues interpreted these results as support for their ‘situated identity’ theory in which different goals, values, and behavior tendencies are automatically activated when one enters into these various life settings.

Organizational psychologist Gary Latham and his colleagues have now conducted a decade of research on goal priming in organizations, in both laboratory and field settings – showing priming effects on achievement and performance, creativity, learning, and cooperation in the actual workplace. A recent meta-analysis of this research (Chen et al., 2020) concluded that the goal priming effects were robust and reliable across both settings, and if anything, they were stronger in real-world corporate and nonprofit organizational settings than in the laboratory. This research program has routinely included manipulation checks of their priming manipulations to show its effect on the internal concept (achievement, cooperation, creativity) such as word fragment completion and other standard laboratory ‘social priming’ studies. In the field studies, performance primes embedded in a CEO’s regular Monday morning email significantly increased productivity in employees randomly assigned to that condition, over the entire subsequent work week, compared to the other employees randomly assigned to the control condition. In large customer service call centers, performance priming increased customer satisfaction ratings with the employees’ calls and also the rate at which the customer’s problems were solved in the initial call about the problem.

Health scientist Esther Papies (Papies & Veling, 2013; Papies et al., 2014) whose research focuses on obesity and eating disorders in real life settings, has now conducted many field studies of recipe and poster priming of healthy eating and dieting, in Dutch grocery stores, butcher shops, and university cafeterias. These diet primes dramatically decrease the both purchase and consumption of unhealthy fatty foods by consumers in real life, despite these consumers denying that the flyers or posters had any influence on their behavior, and even in consumers who had no memory for seeing the flyer or poster (see J. Harris et al., 2009, for similar priming effects on food consumption via television advertising).

Organizational psychologist Trevor Foulk and colleagues (2016) performed several field and laboratory studies of the contagion of rudeness in business school classrooms as well as other organizational settings, showing that observing a rude behavior had the same effect on the accessibility of the internal concept of 'rude' as did rude related verbal primes (see Bargh et al., 1996, Study 1). As had Weber and Latham, Foulk conceptualized his real world priming influence in the same way that 'social priming' researchers do, and showed that the primed internal behavioral concept of rudeness had equivalent effects as the perception of actual rude behavior in the classroom setting. Exposure to words related to rudeness or politeness influence the participant's reactions in a subsequent situation where they can act in either a rude or polite way (Bargh et al., 1996, Study 1) in the same way that exposure to actual rude behavior by the classroom instructor increases the probability that students will subsequently act in a ruder way when answering customer emails (Foulk et al., 2016).

Importantly, Foulk and colleagues also showed experimentally that perceiving the behavior of others activates an internal trait-type representation of that type of behavior, and that this internal priming of the behavior concept is responsible for the contagion effects they

demonstrated. Similar laboratory ‘social priming’ studies had already shown that verbal priming of the concept of helpfulness increases helpful behavior by participants who thought the study was over, in an elevator leaving the building (Macrae & Johnston, 1998); the same effect in reverse was found by Kawada et al. (2004) in which participants induced to be helpful, or nosy, subsequently rated a target person as having more of that personality trait (in an ostensibly unrelated experiment). Witnessing the actual behavior of others, or engaging in that actual behavior oneself, primes the relevant internal concept and makes it more likely to be used in forming impressions of target persons; and priming the relevant internal concept increases the person’s own likelihood of behaving that way (Bargh et al., 1996; Chartrand & Lakin, 2013; Dijksterhuis & Bargh, 2001).

All of these are studies conducted by researchers in diverse domains of behavioral science. In each case, the experimental design was generated from a theoretical model identical to those in ‘social priming’ studies, in which incidental contextual and situational cues are hypothesized to activate internal goals, interpersonal behavioral tendencies, health behaviors, standards of morality, personal identities, and public-policy attitudes. These internal mental constructs are then predicted to exert an implicit influence on an important real-world phenomenon. This is absolutely the same method, the same theoretical model, the same prediction, as in ‘social priming’ research. But these studies have not faced the same skepticism as have ‘social priming’ studies, presumably because they were not conducted by social psychologists. Yet of course they *are* social priming effects, and the tendency to place them in a separate category involves a double standard that is not defensible.

3. Replicability and the relative power of between- versus within-participants designs

I've personally been involved in many different research programs over the years; some of these have involved between-participants designs and others used within-participants designs. As Sherman and Rivers point out, within-subjects manipulations are higher powered than between-subjects manipulations. And accordingly, the lines of my own research that have easily replicated are those that involved within-participants designs, such as the (1) automatic-evaluation (automatic attitude activation) research with Shelly Chaiken (e.g., Bargh et al., 1992, 1996), in which the generalized, unconditional affective priming effect was repeatedly found over the next 20 years of research in labs around the world (see meta-analysis by Herring et al., 2013); (2) the goal-driven implicit evaluation effect (e.g., Ferguson & Bargh, 2004; Ferguson, 2008; Huang & Bargh, 2014; Melnikoff & Strohminger, 2020), and (3) the automatic behavioral approach-avoidance effect (Chen & Bargh, 1999; see Aubé et al., 2019; Rougier et al., 2020). Moreover, as Sherman and Rivers also note that power should increase the less time elapses between priming manipulation and dependent variable measurement, it is relevant that behavioral mimicry effects (Chartrand & Bargh, 1999) which involve zero delay -- where the prime is present *during* the behavior -- also easily and consistently replicate (e.g., Chartrand & Lakin, 2013; Foulk et al., 2016).

So if you look at the fate of these various research programs -- which span the range of design options from between- to within-participants, and with a variety of temporal delays between independent and dependent variables -- the ones that were more easily replicated were the ones that used the most powerful designs. This is hardly surprising. But it highlights Sherman and Rivers' conclusion that it is not the 'social' nature of social priming studies, or even that they are done by social psychologists that makes them less consistently replicated, but that many of

these studies have used between-participants designs and relatively long delays between priming and dependent variable measurement. Consider my own career, then, as a kind of within-participants test of this idea: the lines of research that stood up well to the test of time were those that involved within-participants designs and zero temporal delays, the ones that were more difficult for others to replicate involved between-participants designs and longer delays.

Of course, this means we should put our money where our mouth is: the conclusion that the small N and between-participants social priming studies involving goal and behavior priming are also real effects, which would become replicable as well if greater power were used to detect them, is a prediction that can be tested. And it now has been, and supported through experiments involving large N and within-participants designs (behavior priming: Payne, Brown-Iannuzzi, & Loersch, 2016) and through meta-analytic reviews that have aggregated data across hundreds of experiments (goal priming: Weingarten et al., 2016; Chen et al., 2020; religious value priming of moral and prosocial behavior: Shariff et al., 2016). But again, these successes should not be surprising. These large N experiments and meta-analytic reviews obtained robust ‘social’ priming effects precisely because these effects are qualitatively the same, and involve the same underlying mechanisms, as those in the well-established (and noncontroversial) verbal priming and implicit memory literatures (Schacter, 1987; Reber, 1993).

In sum, as Sherman and Rivers point out, the distinction between social and nonsocial priming is arbitrary and without foundation. This means that those who do not believe ‘social’ priming effects are real can take one of two possible paths. They can be skeptical of these nonsocial priming effects as well – that is, all priming in general -- or else adopt the position that any failures to replicate ‘social’ priming studies merely speak to particular effects and not the deeper question of whether priming itself is real or not. There is no legitimate third path.

Author Note

John A. Bargh, Department of Psychology, Yale University, 2 Hillhouse Avenue, New Haven CT 06511. E-mail: john.bargh@yale.edu. Thanks Ran Hassin and David Melnikoff for their feedback on earlier drafts.

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