Misunderstanding the Behavior Priming Controversy:

Comment on Payne, Brown-Iannuzzi, and Loersch (2016)

David R. Shanks

Division of Psychology and Language Sciences, University College London, 26 Bedford Way, London WC1H 0AP, England

d.shanks@ucl.ac.uk

Abstract

There has been considerable controversy around the limits and reproducibility of so-called 'behavior' priming effects. Payne, Brown-Iannuzzi, and Loersch (2016) reported a series of 6 experiments on the effects of primes on participants' bets in a simulated blackjack game, and claimed that their findings not only establish the reality of behavior priming beyond dispute, but also demonstrate that this form of priming has the crucial hallmark of occurring outside participants' awareness and control. I describe a statistical model which does not distinguish automatic and controlled processes, but which nonetheless reproduces Payne et al.'s results and hence shows that their conclusions are unwarranted. Payne et al.'s experimental task and within-subjects design provide little insight into why some behavior priming studies have proven difficult to replicate.

Keywords:

Automaticity; Priming; Process dissociation procedure; Replication; Unconscious

The psychological and behavioral sciences are going through a period of deep reflection about their research methods and capacity for revealing the truth about Nature. Although this reflection has grown to encompass many elements including questionable research practices, statistical methods, and publishing models, one of its key drivers is disagreement and debate about a simple phenomenon which has become known, for better or worse, as 'behavior' or 'social' priming.

This phenomenon has taken centre stage because, as Molden (2014, p. 1) observes, "it is now virtually axiomatic among social psychologists that the mere exposure to socially relevant stimuli can facilitate, or prime, a host of impressions, judgments, goals, and actions, often even outside of people's intention or awareness." Unfortunately, many of the most influential examples of behavior priming[1] have proven very difficult to replicate (see Cesario, 2014; Kahneman, 2012)(see also special issue of *Social Cognition*, June 2014). A recent meta-analysis of 352 effect sizes from studies of the effects of incidentally-presented action-related prime words on task performance (Weingarten et al., 2016) serves to highlight this concern, despite the fact that it obtained an overall statistically significant (albeit small) priming effect ($d \approx 0.3$-0.4). Two of the apparently-successful studies included in the meta-analysis (Albarracín et al., 2008; Eitam, Hassin, & Schul, 2008) were re-examined in the Reproducibility Project (Open Science Collaboration, 2015). High-powered, pre-registered replication attempts were unsuccessful in both cases. Although these two studies represent a small proportion of the significant effects in the meta-analysis, they can be regarded as selected at random. When combined with the statistically significant evidence of publication bias in the meta-analysis (Weingarten et al., 2016), the replication failures cast considerable doubt on the reality and reproducibility of this form of behavior priming.

The article by Payne, Brown-Iannuzzi, and Loersch (2016; henceforth PBL) is presented as a contribution to this debate. These researchers reported a series of 6 experiments on the effects of primes on participants' bets in a simulated blackjack game, and argued that their findings establish the reality of behavior priming beyond dispute. On the basis of their findings, PBL suggested that the reason some prominent behavior priming studies have proven hard to replicate is that they (and the replications) employed weak and under-powered between-subjects designs. In addition to these two theoretical claims, PBL also claimed that this form of priming has the crucial hallmark of occurring outside participants' awareness and control. This assertion is the focus of the present Comment. I argue here that this empirical claim, which is crucial to the framing of behaviour priming, can be challenged on both conceptual and methodological grounds. In the final discussion I also comment briefly on PBL's two theoretical inferences.

The experimental task employed in PBL's experiments is simple and involves participants choosing to bet or pass on each trial of a blackjack-like gambling game. On critical trials where the participants' two cards had intermediate values (defined as 8-17 or 11-14, depending on the experiment) and where the outcome (win/lose) was arranged to be completely random, the decision to bet was influenced by a brief (300 ms) but clearly visible prime (e.g., the facilitating word 'gamble' or the inhibiting word 'stay') presented prior to each choice but uncorrelated with the outcome. This effect was replicated across all of their experiments. PBL's experimental results establish very firmly that gambling decisions can be primed, and (with the exception of the results of Experiment 4, as noted below) the present Comment does not question this aspect of their findings.

Awareness and the process dissociation procedure

PBL note that "Behavior priming effects are important for psychological theory because they provide evidence about the influence of automatic or unconscious processes on behavior" (p. 1269). Their theoretical interpretation of the priming effects observed in their experiments, based on the Situated Inference Model (Loersch & Payne, 2014), proposes that participants mistake the source of mental contents activated by the prime. Instead of recognizing that the thought of betting on a given trial has been caused by the prime, the participant misattributes this thought to her own internal evaluation of the situation and believes that it is self-generated. In turn this misattribution leads to lack of awareness of the prime's true influence. PBL's Experiments 4-6 investigated this important property.

In Experiment 4 the primes were presented briefly for 12 ms and pre- and post-masked by neutral words each presented for 100 ms to suppress awareness. Because a priming effect (albeit somewhat reduced) was again obtained, the authors concluded that the effects observed were therefore indicative of unconscious or automatic processes.

When assessing awareness, the devil is usually in the details. PBL measured awareness in a test administered after the main gambling phase of the experiment. On each trial a sequence was presented that was identical to a gambling trial (premask word → prime → postmask word) but were asked to type in the 3 words in three dialog boxes labelled *first word*, *second word*, and *third word*. They entered '*99'* if they did not see a word. PBL observed that one participant correctly reported a single prime word.

For decades researchers have grappled with the problem of possible response bias in awareness tests because it is known that such tests can underestimate awareness if participants adopt a conservative reporting criterion (Eriksen, 1960; Kunimoto, Miller, & Pashler, 2001). Participants' confidence in their identification of the prime word may have been so low that they simply decided to type *99* (which also requires less effort). A further issue is that participants' task in the awareness test was not at all the same as in the main gambling task. Instead of clicking a button to indicate their choice (bet or pass), they had to write down 3 words, a difference that could easily have contributed to a reduction in sensitivity. The first (premask) and third (postmask) words were fully visible and easy to perceive relative to the prime, hence it is also likely that these "easy" words were recalled and typed first, attenuating accurate reporting of the "difficult" prime word.

It is also important to emphasize that the priming effect in this study was fragile. Although there was a significant priming × hand value interaction, the main effect of priming was far from significant (as PBL noted). Moreover only 53/115 participants showed a numerically positive priming effect across the critical hands against 46/115 who showed a negative effect (and 16 had a zero effect), a distribution which is not significant by a sign test, one-tailed $p = 0.273$. It is easy to see that the priming × hand value interaction could be attributable to just a small number of participants who consciously detected and responded in accordance with some of the primes.

In Experiments 5 and 6 PBL attempted to separate controlled/conscious from automatic/unconscious influences of the prime by employing the process dissociation procedure (PDP; Jacoby, 1991) to analyze performance in Inclusion and Exclusion versions of the gambling task. The Exclusion version was the standard task from the preceding experiments in which participants were instructed to ignore the uninformative prime word.

In the Inclusion version, by contrast, they were instructed that the prime was "a hint and is informative on more trials than not, so you may want to consider this word in your decision" (Payne et al., 2016, p. 1275). A substantially larger priming effect was obtained in the Inclusion condition. This, on its own, demonstrates that at least part of the effect must be controllable; the question is whether in addition to this controllable component there is also a contribution of uncontrollable/automatic processes to the priming effect? The data for Experiment 5 are shown in the top row of Figure 1.

To apply the process dissociation procedure, PBL calculated the proportion of prime-consistent responses (by summing the number of bet responses given a bet prime and the number of pass responses given a pass prime), and then calculated the PDP parameters for controlled/conscious processes (*C*) and automatic (unconscious) processes (*A*) from the formulae:

$$C = P(\text{Prime-consistent}|\text{Inclusion}) - P(\text{Prime-consistent}|\text{Exclusion}) \quad (1)$$

$$A = P(\text{Prime-consistent}|\text{Exclusion})/(1 - C) \quad (2)$$

PBL asserted that if there are no automatic effects then $A = 0.5$, while if there are no controlled effects then $C = 0$ (I return to this point below). The results for Experiments 5 (see Figure 2) and 6 showed that *C* was greater than zero but did not differ significantly when comparing ambiguous hands (with total card values 11-14) versus unambiguous hands (values 8-10 and 15-17). *A* was greater than 0.5 but did differ significantly, albeit by a very small amount numerically, between ambiguous and unambiguous hands. PBL interpreted this pattern as evidence not only of the controlled/automatic distinction, but also as supporting their hypothesis (derived from the Situated Inference Model) that the automatic effect of the prime via misattribution would be greater when the participant is more uncertain – that is, on ambiguous hands.

There are several problems with this analysis. First, the PDP requires contrasting a condition in which participants are motivated to include some information in their response with one in which they are motivated to exclude it. But in the Exclusion instructions of these experiments participants were merely told that the prime was a distractor and that they should try not to let it influence their decision. No incentives were provided for doing this and it seems possible that at least some participants may have paid little heed to the instruction and hence not made much effort to inhibit the prime's influence (of which they may have been fully aware). It is important to realise that there is nothing at all irrational about participants' use of the prime to guide their choices. Given that the payoff on each critical trial was completely random, there was nothing the participant could do to either increase or decrease her chances of winning (this does not apply of course to the noncritical trials). Making a choice on each critical trial that is identical to the prime neither reduces nor increases the payoff probability. Indeed responding in alignment with the prime could be seen as wholly rational. For example, it requires cognitive effort to make a binary choice across many trials. By simply responding on the basis of the prime, cognitive effort is reduced at no cost in terms of financial payoff.

Secondly, PBL calculated *C* and *A* parameters separately for ambiguous and unambiguous hands. While this may make sense from the perspective of the Situated Inference Model, it is hard to see its justification within the theoretical framework of the process dissociation procedure, and indeed is at variance with PBL's own description of the PDP:

> "In our task, participants could be influenced by the prime either intentionally (e.g., by using the prime as a hint) or automatically (e.g., making prime-consistent responses despite trying to ignore the prime). Note that the model does not take the hand value into account when estimating automatic and controlled influences." (p. 1275).

Ambiguous and unambiguous hands differ only in their points value, so calculating separate parameter estimates implies a process whereby the magnitude of controlled and automatic influences on a given trial is determined *after* each card pair is viewed by the participant, in contradiction of the statement above. This is not in keeping with the PDP's assumption that *C* and *A* are governed by systematic cognitive factors such as attention or depth of processing. It is analogous to applying the PDP separately to high- and low-frequency words in a memory experiment, for example. PBL offered no justification for applying the PDP in this way. Indeed, their application of the PDP to the data they collected in their Experiments 5 and 6 failed to take heed of the known boundary conditions of the procedure. It is well-established for example that the independence assumption needs careful validation and that the precise wording of the test instructions can be important (see Yonelinas & Jacoby, 2012). The procedure "cannot be "taken off the shelf" with no regard for meeting boundary conditions" (Yonelinas & Jacoby, 2012, p. 676). PBL reported no analyses of the appropriateness of their application of the PDP to their data.

Finally, and more fundamentally, the PDP is not always a reliable method for teasing apart conscious and unconscious processes. Ratcliff, McKoon, and Van Zandt (1995) showed that the PDP identifies two dissociable processes even when the data are known to have been generated by a single process. The simulation I report now applies the same reasoning to PBL's procedure.

A model of Inclusion/Exclusion performance

In this simple statistical model, each trial begins with the independent selection of two cards, each drawn with probability 1/13. Their values are summed and denoted by *T* (total). Next, the prime is selected at random to be either a bet or pass word, assigned the values +1 and -1, respectively, and denoted *V* (value). This prime value is weighted by amount *W*. The total evidence *E* on which each choice is based is therefore

$$E = T + W.V - 12.5 \qquad (3)$$

where 12.5 is subtracted to center the output at that value, the mean hand value. To translate this into an actual choice, *E* is transformed by a sigmoid function:

$$P(bet) = \frac{1}{1+e^{-S.E}} \qquad (4)$$

where *S* is a slope parameter (set to 0.7 in the simulation).

The crucial feature of the model is that the weight *W*, which can be interpreted as an attentional or decision weight, is permitted to vary between the Inclusion and Exclusion conditions. In the standard Exclusion condition in which participants are instructed to pay no heed to the prime, we assume that *W* is very small (0.3 in the simulation), whereas in the Inclusion condition, the instruction to consider the prime in their decision causes participants to assign a higher value to *W* (1.0 in the simulation). Although restricted to being nonnegative, this decision weight is not assumed to have a lower bound: A value of

0.0 – a complete lack of any priming effect, and hence no automatic influence – would apply if the participant gave the prime no weight.

This model is more parsimonious than PBL's in terms of numbers of free parameters: 3 parameters (*W* with 2 values, and *S*) to predict 4 dependent measures (the proportions of prime-consistent responses in the Inclusion and Exclusion conditions, for ambiguous and unambiguous hands), whereas PBL's model requires 4 parameters (*A* and *C* values for each hand type) each with one value to predict the same 4 measures[2]. The statistical model embodied in Equations 3 and 4 is therefore both broader in scope (as it makes predictions for prime-consistent responding on each hand) and more parsimonious.

The model is able to simulate the major findings without difficulty. The bottom row of Figure 1 shows the predicted proportion of betting choices for the Inclusion (left panel) and Exclusion (right panel) conditions, respectively. Clearly, the model captures the overall increase in betting choices as hand value increases, as well as the fact that the priming effect is greater for ambiguous than for unambiguous hands, which arises simply as a result of the sigmoidal squashing function (Equation 3) that transforms evidence into choice probabilities. More importantly, the model generates a greater influence of the prime under Inclusion conditions. In the Inclusion condition, the weight given to the prime biases betting responses upwards when the prime is a bet word and downwards when it is a pass word, and is equivalent to changing the total card value by more than one point. Of course this depends on the *W* parameter. When $W = 0$, there is no priming effect at all.

Not only does the model account for choice behavior in the Inclusion and Exclusion conditions, it also yields good approximations to the *C* and *A* parameters when its predictions are taken as the input to the process dissociation procedure of Equations 1 and 2. As shown in Figure 2, *C* is estimated to be small but greater than zero while *A* is larger than 0.5. The contrast between unambiguous and ambiguous hands is larger for *A* than *C*[3]. Thus the model's predictions, which all fall within the 95% confidence intervals of the empirical estimates, match the observed results closely.

In sum, an obvious interpretation of the success of this straightforward statistical model is simply that the Inclusion instructions encouraged participants to pay more attention to the prime. The experimental results and their interpretation through the lens of the process dissociation procedure do not establish that the priming effect is driven in part by automatic/unconscious processes, because a model which makes no distinction between controlled and automatic processes is equally compatible with the data.

The simulation provides further evidence (Ratcliff et al., 1995) that the PDP is, if misapplied, a flawed technique for isolating mental processes. The PDP misdiagnoses the simulation results as arising from independent automatic and controlled processes. By extension, we can also conclude that it similarly misdiagnoses the behavior of PBL's participants. Indeed this can be illustrated even more directly by a simple thought experiment. Imagine that participants exert complete control over their responding and that there are no automatic influences on their behavior. In the Inclusion condition they deliberately respond in accordance with the prime on some trials, while on other trials they ignore it completely. Their overall concordance probability might be 0.62, for example, which is the level of prime-consistent responses in the Inclusion condition of Experiment 5. Suppose also that they completely avoid any influence of the prime in the Exclusion condition – perhaps they close their eyes briefly on each trial when the prime is displayed. Assuming participants bet and pass with equal overall probability, then their proportion of prime-consistent responses in this condition will be 0.5[4]. From Equation 1 we obtain $C = 0.62 - 0.5 = 0.12$ (despite the fact that participants have complete control), and from Equation 2 we obtain $A = 0.5/(1 - 0.12) = 0.57$. Thus the PDP yields the nonsensical and false conclusion that there are reliable automatic influences ($A > 0.5$) in this set of circumstances. In fact from Equation 2 it is clear that whenever $C > 0$, the PDP yields $A > 0.5$ if $P(\text{Prime-consistent}|\text{Exclusion}) = 0.5$.

It is, in essence, a basic property of the PDP as applied by PBL to their task that automatic influences can only be completely absent ($A = 0.5$) when $C = 0$, that is, when the prime has no detectable influence whatsoever. PBL's finding of A values greater than 0.5 is not a psychological discovery but an arithmetic inevitability.

The points of difference between PBL's account and the one offered here are worth emphasizing. In PBL's explanation, there are distinct automatic and controlled mental processes. The prime automatically activates thoughts of betting or passing, and these are misattributed to internally-generated evaluations. This misattribution, and hence the automatic priming effect, is larger on ambiguous than on unambiguous hands. In the account offered here, there is no automatic/controlled distinction, no distinction between the processes operating on ambiguous and unambiguous hands, and no misattribution process. Instead, the observed data patterns for both the probability of prime-consistent responding and the PDP parameters are emergent properties of a mechanism which integrates all relevant sources of information, modulated by a decision weight.

Concluding comments

Payne et al. (2016) present evidence that a brief prime can influence participants' decisions in a betting game. However, because of the limitations of the PDP method and analysis in Experiments 5 and 6, their conclusion that this effect has an automatic component is unjustified. Contrary to what they claim, their experiments do not therefore establish the existence of behaviour priming, defined as the automatic influence of a prime on behaviour.

As noted at the beginning of this Comment, PBL's article seeks to do substantially more than demonstrate unconscious priming. It aims to establish the reality of behavior priming beyond dispute by providing a novel experimental tool, and to make the case that the employment of under-powered between-subjects designs explains why some prominent behavior priming studies have proven hard to replicate. Are these claims warranted?

PBL acknowledge that priming effects on evaluation of and judgments about stimuli have previously been established beyond dispute, but suggest that "like semantic priming, these paradigms focus on processing of subsequent stimuli rather than on decisions or actions that are traditionally considered to be "behaviors"" (Payne et al., 2016, p. 1271). They suggest that their results are novel because they "demonstrate, most simply, that primes can affect subsequent behavior, not just the ability to process subsequent stimuli" (p. 1271) and "provide evidence that primes can reliably affect behavior, under at least some conditions" (p. 1269). Is this evidence truly novel? In fact it is not difficult to find prior demonstrations that decisions and actions can be biased by some priming cue. A study by Ludvig, Madan, and Spetch (2015) is particularly relevant because it reported a priming effect very similar to PBL's. Participants in this experiment saw 2 stimuli (images of distinctive doors) on each critical trial, one a risky choice and the other a safe choice. If a prime, comprising an image previously associated with reward, was presented prior to the choice stimuli then risk-taking was increased by about 15%, even though (as in the key trials in PBL's procedure) the prime was not predictive of the trial outcome. PBL did not cite this study despite its conceptual similarity to their own experimental method and outcome. The only major difference is the use of a prime picture whose association with reward was learned during the experiment (Ludvig et al., 2015) versus a prime word semantically related to reward (PBL).

Whatever the merits of drawing a distinction between priming as the facilitation of processing subsequent stimuli, on the one hand, and priming of decisions or actions on the other, there can be little dispute that both forms of priming are well-established.

A second reason to doubt the relevance of PBL's findings to the wider behavior priming debate is the substantial difference in timescale (see Wentura & Rothermund, 2014, for a detailed discussion of this issue). In PBL's procedure the choice options were presented 100 ms after the prime. Although participants were under no time pressure, their responses were presumably made within a matter of a few seconds of the prime at most. In a typical behavior priming study, in contrast, the prime-behavior interval might be two orders of magnitude greater than this. For example, in the studies included in Weingarten et al.'s (2016) meta-analysis the interval ranged from around 3-8 min. The controversial 'flag' priming effect of Carter, Ferguson, and Hassin (2011) was claimed to endure across an 8-month interval. PBL themselves noted in passing this timescale difference but nonetheless drew conclusions from their studies about behaviour priming more generally. Such conclusions, I argue, are unjustified.

The fact that risky choices were affected by subtle primes in PBL's experiments is an important result of considerable relevance to theories of decision making. But PBL's claim that their experiments – by demonstrating that choices can be primed – make a contribution to the behavior priming controversy and "provide important evidence for debates about the reality and replicability of priming phenomena" (p. 1277) rests on inadequate acknowledgement of past research and glossing over a substantial difference in timescale.

What about PBL's other theoretical conclusion, that the employment of under-powered between-subjects designs explains why some prominent behavior priming studies have proven hard to replicate? If decisions and actions can be primed, then why (PBL ask) has it proven so difficult to establish such priming effects beyond dispute? PBL point to experimental design combined with statistical power as a key factor. They observe that "behavior priming studies typically use a between-subjects design… Semantic priming, in contrast, uses within-subjects designs in which each subject responds to all prime-target combinations over dozens or hundreds of trials" (p. 1271). Other things being equal, within-subjects designs will usually be more powerful, regardless of the topic under study. But many demonstrations of priming of actions and decisions have employed within-subjects designs (e.g., Griskevicius et al., 2007; Ludvig et al., 2015).

PBL suggest that their use of a within-subjects design sheds light on past replication failures, but here again their reasoning seems flawed. They ask whether

> "In studies where failed null replications have been reported, is it the psychological effect of primes on behavior that is in question, or is it mundane methodological factors like statistical power? Our results suggest that methodological factors play an important role. If so, we may revise our beliefs about effect sizes and variability of previously reported effects, but not about the basic psychological effect in question."

Yet they provide no direct evidence that priming is more consistent in within-subjects designs, and they make no mention that statistical power was explicitly considered at the time of deciding on sample size in almost every priming replication failure in the past few years (e.g., Gomes & McCullough, 2015; Klein, others, & Nosek, 2014; Rohrer, Pashler, & Harris, 2015). For instance, Klein et al.'s (2014) Many Labs replication achieved power of 1.000 to detect 2 behavior priming results, yet failed to do so. When so much of the recent debate has concerned power, sample sizes, confidence intervals and so on, to suggest that many or at least some of the prominent replication failures have arisen merely as a result of low power seems unconvincing.

The key issue is that priming 'skeptics' do not question all examples of behavior priming, only some. PBL's position is that the crucial domain is priming effects on actions and decisions that cannot be simply explained as arising from enhancement of stimulus processing. To paraphrase their viewpoint, past studies in this domain have been predominantly studied in fragile between-subjects experiments and hence false negatives have occurred when low-powered replication studies have been undertaken. But considering experimental design, I contend, sheds minimal light on the priming controversy.

There is a rather different explanation for why some behavior priming effects have proven so difficult to replicate, namely that they are false positives emerging from a mistaken view of the human mind. Perhaps the mind is not an iceberg much of whose modular structure is below the waterline of awareness, and perhaps the unconscious cannot in reality perform all of the high-level functions that the conscious mind can (Bargh, 2014b; Hassin, 2013)? There is much evidence that unconscious processes, if they exist at all, are narrow, fleeting, and insufficient to provide the level of *transfer* from the prime to the target behavior that the controversial studies uniformly assume (see Newell & Shanks, 2014). To activate the stereotype 'professor' and observe a downstream effect on an individual's ability to answer the question "What is the total number of spots on a die?" (Dijksterhuis & van Knippenberg, 1998) would require a rich pathway of interconnecting concepts and processes which is orders of magnitude beyond what is typically revealed in more conventional (e.g., repetition) priming experiments, where priming is commonly found to transfer remarkably narrowly across tasks and time (e.g., Franks, Bilbrey, Lien, & McNamara, 2000).

References

Albarracín, D., Handley, I. M., Noguchi, K., McCulloch, K. C., Li, H., Leeper, J., . . . Hart, W. P. (2008). Increasing and decreasing motor and cognitive output: A model of general action and inaction goals. *Journal of Personality and Social Psychology, 95*, 510-523. doi:10.1037/a0012833

Bargh, J. A. (2014a). The historical origins of priming as the preparation of behavioral responses: Unconscious carryover and contextual influences of real-world importance. *Social Cognition, 32*, 209-224.

Bargh, J. A. (2014b). Our unconscious mind. *Scientific American*, 30-37.

Carter, T. J., Ferguson, M. J., & Hassin, R. R. (2011). A single exposure to the American flag shifts support toward Republicanism up to 8 months later. *Psychological Science, 22*, 1011-1018.

Cesario, J. (2014). Priming, replication, and the hardest science. *Perspectives on Psychological Science, 9*, 40-48. doi:10.1177/1745691613513470

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.

Eitam, B., Hassin, R. R., & Schul, Y. (2008). Nonconscious goal pursuit in novel environments: The case of implicit learning. *Psychological Science, 19*, 261-267.

Eriksen, C. W. (1960). Discrimination and learning without awareness: A methodological survey and evaluation. *Psychological Review, 67*, 279-300.

Franks, J. J., Bilbrey, C. W., Lien, K. G., & McNamara, T. P. (2000). Transfer-appropriate processing (TAP) and repetition priming. *Memory & Cognition, 28*, 1140-1151.

Gomes, C. M., & McCullough, M. E. (2015). The effects of implicit religious primes on dictator game allocations: A preregistered replication experiment. *Journal of Experimental Psychology: General, 144*, e94–e104.

Griskevicius, V., Tybur, J. M., Sundie, J. M., Cialdini, R. B., Miller, G. F., & Kenrick, D. T. (2007). Blatant benevolence and conspicuous consumption: When romantic motives elicit strategic costly signals. *Journal of Personality and Social Psychology, 93*, 85-102.

Hassin, R. R. (2013). Yes it can: On the functional abilities of the human unconscious. *Perspectives on Psychological Science, 8*, 195-207. doi:10.1177/1745691612460684

Jacoby, L. L. (1991). A process dissociation framework: Separating automatic from intentional uses of memory. *Journal of Memory and Language, 30*, 513-541.

Kahneman, D. (2012). A proposal to deal with questions about priming effects. Retrieved from http://www.nature.com/polopoly_fs/7.6716.1349271308!/suppinfoFile/Kahneman%20Letter.pdf

Klein, R. A., others, & Nosek, B. A. (2014). Investigating variation in replicability: A “Many Labs” replication project. *Social Psychology, 45*, 142-152.

Kunimoto, C., Miller, J., & Pashler, H. (2001). Confidence and accuracy of near-threshold discrimination responses. *Consciousness and Cognition, 10*, 294-340.

Loersch, C., & Payne, B. K. (2014). Situated inferences and the what, who, and where of priming. *Social Cognition, 32*, 137-151.

Ludvig, E. A., Madan, C. R., & Spetch, M. L. (2015). Priming memories of past wins induces risk seeking. *Journal of Experimental Psychology: General, 144*, 24-29. doi:dx.doi.org/10.1037/xge0000046

Molden, D. C. (2014). Understanding priming effects in social psychology: What is "social priming" and how does it occur? *Social Cognition, 32*, 1-11.

Newell, B. R., & Shanks, D. R. (2014). Prime numbers: Anchoring and its implications for theories of behavior priming. *Social Cognition, 32*, 88-108.

Open Science Collaboration. (2015). Estimating the reproducibility of psychological science. *Science, 349*. doi:10.1126/science.aac4716

Payne, B. K., Brown-Iannuzzi, J. L., & Loersch, C. (2016). Replicable effects of primes on human behavior. *Journal of Experimental Psychology: General, 145*, 1269–1279. doi:10.1037/xge0000201

Ratcliff, R., McKoon, G., & Van Zandt, T. (1995). Process dissociation, single-process theories, and recognition memory. *Journal of Experimental Psychology: General, 124*, 352-374.

Rohrer, D., Pashler, H., & Harris, C. R. (2015). Do subtle reminders of money change people’s political views? *Journal of Experimental Psychology: General, 144*, e73-e85.

Weingarten, E., Chen, Q., McAdams, M., Yi, J., Hepler, J., & Albarracín, D. (2016). From primed concepts to action: A meta-analysis of the behavioral effects of incidentally presented words. *Psychological Bulletin, 142*, 472-497.

Wentura, D., & Rothermund, K. (2014). Priming is not priming is not priming. *Social Cognition, 32*, 47-67.

Yonelinas, A. P., & Jacoby, L. L. (2012). The process-dissociation approach two decades later: Convergence, boundary conditions, and new directions. *Memory & Cognition, 40*, 663-680. doi:10.3758/s13421-012-0205-5

Footnotes

1. Behavior priming as characterised by Molden (2014) has to be clearly distinguished from the phenomenon of priming as typically studied by experimental cognitive psychologists, often interpreted as the spread of activation from one mental representation to a closely related one. Although the precise demarcation between 'behavior' and 'cognitive' priming is a matter of debate (Bargh, 2014a), there is no controversy over the reality of the priming effects obtained in hundreds of studies on masking, repetition priming, lexical decision, and so on. For present purposes behavior priming is characterized as the priming of overt behaviors including decisions/actions, rather than simply as facilitation in the processing of subsequent stimuli.
2. Given that the PDP model has as many free parameters as there are datapoints, it is of course unfalsifiable in the sense that it can be fitted perfectly to any pattern of values of P(Prime-consistent|Inclusion) and P(Prime-consistent|Exclusion) (except when the former is 1 and the latter is zero, in which case A is undefined). However an additional meta-theoretical constraint is that experimental manipulations (such as instructions that the prime is a useful hint) will have systematic effects on the model parameters, for instance affecting C but not A or *vice versa*.
3. The model can generate quite substantial differences between the effect of hand type (Ambiguous/Unambiguous) on estimates of A compared to C. However I do not pursue this aspect of the experiment in depth because the empirical results are equivocal. PBL reported that hand type had a significant effect on A in both Experiments 5 and 6, and that its effect on C was not significant in either experiment. Yet analysis of variance reveals that the measure (A vs. C) × hand type interaction was only significant in Experiment 5, $F(1, 208) = 11.88$, $p = .001$, and not in Experiment 6, $F(1, 178) = 2.03$, $p = .16$.
4. It is important to note that the terms P(Prime-consistent|Inclusion) and P(Prime-consistent|Exclusion) in Equations 1 and 2 have minimum values of 0.5. Given that the prime is chosen at random on each trial, 50% of responses will be consistent with the prime by chance alone. The only exception to this would arise if a participant chose to deliberately respond in opposition to the prime (that is, chose to bet

whenever the prime was a pass word and *vice versa*) but this would lead to negative values of *C* and render the PDP uninterpretable.

Disclosures and Acknowledgments

The author declares no conflict of interest.

Programs for running the simulation reported in this article are available in both Excel and R versions at osf.io/q9rk7. I am indebted to Tom Hardwicke, Ben Newell, and Miguel Vadillo for helpful advice and comments.

Figure Captions

Figure 1: Proportion of betting choices as a function of hand value. Left column: Inclusion condition. Right column: Exclusion condition. Top row: Data from Payne et al.'s Experiment 5. Bottom row: Model results.

Figure 2: Mean estimates of the parameters for controlled (*C*) and automatic (*A*) responding for ambiguous (values 11-14) and unambiguous (values 8-10 and 15-17) hands derived from applying the process dissociation procedure of Equations 1 and 2, for Payne et al.'s Experiment 5, Experiment 6, and the model described in the text. Amb = ambiguous, Unamb = unambiguous. Error bars are 95% confidence intervals.

Figure 1.

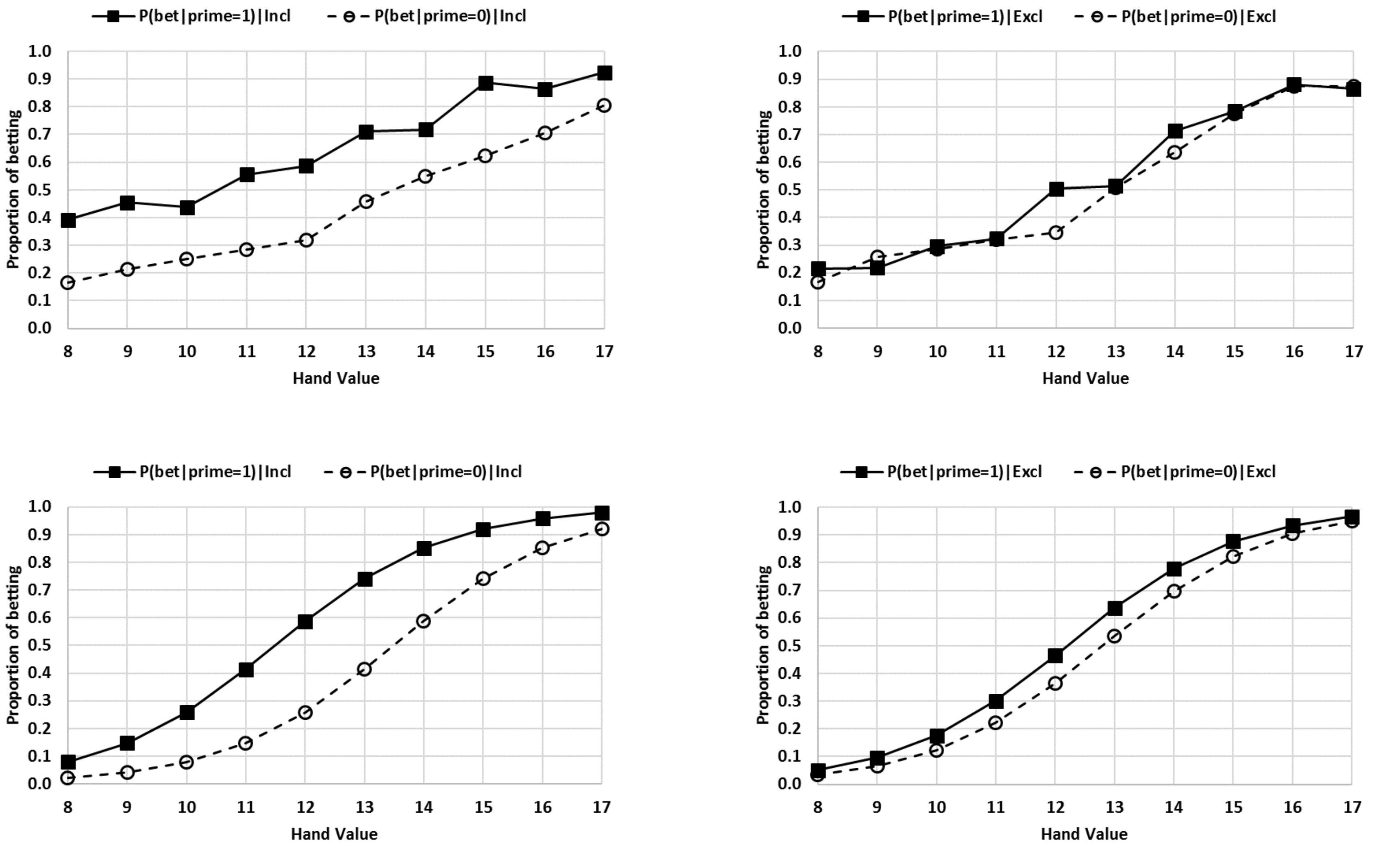
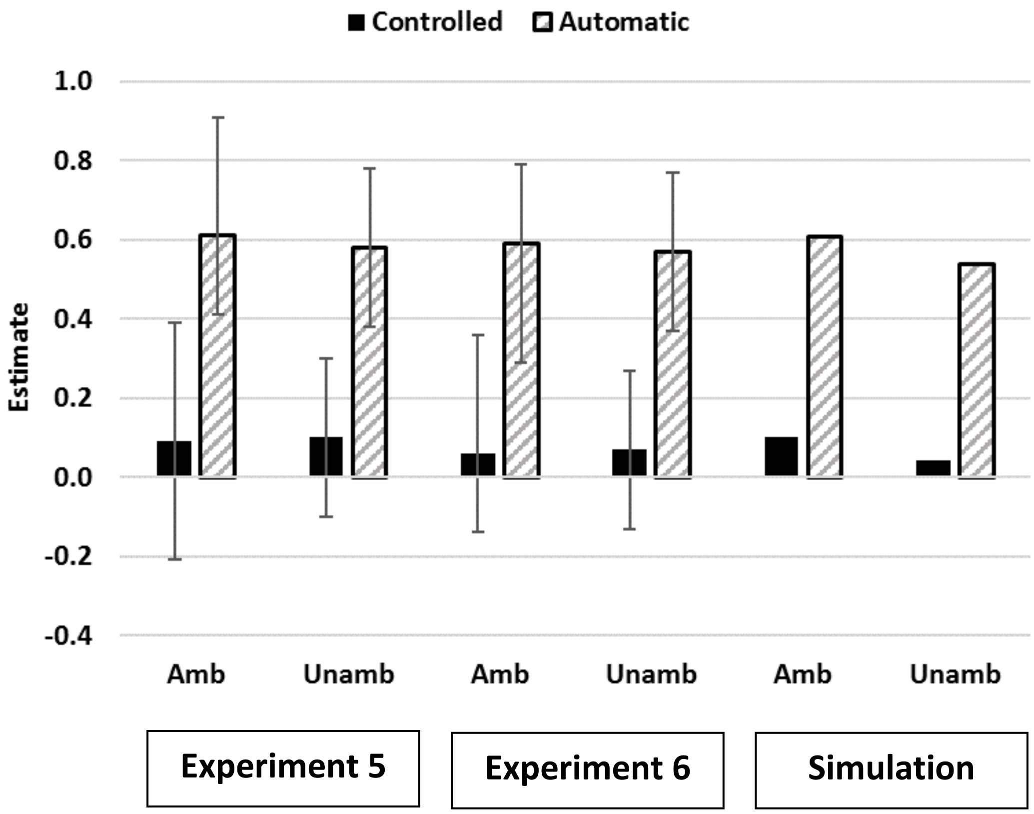

P(bet|prime=1)|Incl
P(bet|prime=0)|Incl
P(bet|prime=1)|Excl
P(bet|prime=0)|Excl
Proportion of betting
Hand Value
0.0
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0.9
1.0
8
9
10
11
12
13
14
15
16
17

Figure 2.



Controlled
Automatic
1.0
0.8
0.6
0.4
0.2
0.0
-0.2
-0.4
Estimate
Amb
Unamb
Amb
Unamb
Amb
Unamb
Experiment 5
Experiment 6
Simulation