

Romance, Risk, and Replication:  
Can Consumer Choices and Risk-Taking be Primed by Mating Motives?

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**Abstract**

Interventions aimed at influencing spending behavior and risk-taking have considerable practical importance. A number of studies motivated by the costly signaling theory within evolutionary psychology have reported that priming inductions (such as looking at pictures of attractive opposite-sex members) designed to trigger mating motives increase males' stated willingness to purchase conspicuous consumption items and to engage in risk-taking behaviors, and reduce loss aversion. However a meta-analysis of this literature reveals strong evidence of either publication bias or *p*-hacking (or both). We then report 8 studies with a total sample of over 1,600 participants which sought to reproduce these effects. None of the studies, including one which was fully preregistered, was successful. The results question the claim that romantic primes can influence risk-taking and other potentially harmful behaviors.

Extensive efforts have been made in several areas of psychology to develop interventions for influencing people's willingness to engage in potentially harmful behaviors such as gambling, addictions, other forms of risk-taking, and excessive spending. Although there have been some notable successes, such as the development of a range of techniques within the cognitive 'debiasing' field (e.g., Gigerenzer, 1991; Larrick, 2004), new interventions would have both practical and theoretical utility. Recently it has been claimed that risk-taking and spending behavior may be triggered in part by evolutionarily-driven motives (Kenrick & Griskevicius, 2013). Studies designed to support this hypothesis have suggested that the subtle priming of mating motives can affect these behaviors. Certainly, the ubiquitous employment of attractive models and sexual cues in advertising product categories such as casinos, fashion, jewelry, cosmetic surgery, cars, cigarettes, and alcohol, and the evidence for their effectiveness (King, McClelland, & Furnham, 2015; Reichert, 2002), suggests that controlling such primes in real-world settings might constitute a valuable intervention.

Evolutionary psychologists have argued that male risk-taking and conspicuous consumption are costly sexual signals intended to attract potential mates (Miller, 2000). In brief, a man's readiness to tolerate risks and to bear a high cost for certain purchases is a reliable indicator of his wealth and status. These behaviors are costly in terms of economic resources and hence exclusive, are easily perceived by others, and because females are assumed to place high importance on affluence and prestige in their potential mates, should thus increase the prospect of attracting a female mate. Researchers (see Kenrick & Griskevicius, 2013) have therefore suggested that men's willingness to pay elevated prices for particular purchases and to engage in risky behaviors ('young male syndrome') is the result of a psychological mechanism designed by

sexual selection as an adaptation to women's evolved preference for prosperous and high-status mates.

Several laboratory experiments have been conducted to test this viewpoint. More specifically, they examined whether males' risk-taking and expenditure on publicly consumed goods and services could be increased by the subtle activation of mating motives. The results provide what appears to be compelling support for the evolutionary psychology hypothesis: For instance, priming of mating motives significantly increased male but not female participants' stated willingness to engage in risky behaviors (Greitemeyer, Kastenmüller, & Fischer, 2013) and to pay for conspicuous but not inconspicuous goods and services (Griskevicius et al., 2007; Sundie et al., 2011). Consistent with the general theoretical framework within which they are interpreted (Kenrick & Griskevicius, 2013), these effects are not always restricted to males. Festjens, Bruyneel, and Dewitte (2014, Study 3) obtained a priming effect in both males and females on willingness to pay for 'reward' items such as chocolates and wine, and Hill and Durante (2011) found an effect of mating primes on females' willingness to take health-related risks in the service of increasing their attractiveness, specifically their desire to get a tan or take dangerous dieting pills.

Since the seminal and highly-cited (>300 Google Scholar citations) research of Wilson and Daly (2004), it has been reported that several decision-making behaviors can be influenced by mating primes, using a variety of different priming manipulations (the specific methods of many of these studies will be described in more detail below). Table 1 summarizes the large space of such demonstrations. In addition to studies on risk-taking and conspicuous consumption, Table 1 lists studies that have explored temporal discounting, loss aversion, and cooperation in the ultimatum game, all capturing important aspects of decision making. For

example, Wilson and Daly (2004) and subsequent studies found that male participants discounted monetary rewards more steeply after viewing pictures of attractive females, and Van den Bergh, Dewitte, and Warlop (2008) obtained the same result when male participants physically examined bras compared to t-shirts. Festjens et al. (2014) obtained a similar effect in both temporal discounting and loss aversion but with female participants who physically examined a pair of boxer shorts compared to a t-shirt. Van den Bergh and Dewitte (2006) observed that male participants accepted less fair offers in the ultimatum game after viewing pictures of attractive females.

It is important to emphasize that this space is itself just a small part of an even larger space in which (a) mating primes have been reported to influence a range of other behaviors such as creativity, aggression, the likelihood of noticing conspicuous consumption products, and the stated importance of wealth (Janssens et al., 2011; Kenrick & Griskevicius, 2013; Roney, 2003), and (b) the behaviors listed in the Table have been associated with other types of evolutionary prime. For instance, Li, Kenrick, Griskevicius, and Neuberg (2012) found that loss aversion can be affected by ‘self-protection’ primes. Other research not included here has studied the effects of the presence of opposite-sex individuals on risk-taking. As an illustration, Ronay and von Hippel (2010) found that male skateboarders took greater risks in the presence of a female than of a male observer. Although priming may contribute to such effects, other processes such as attention oriented to the observer are likely to play a role.

Here we evaluate whether decision making behaviors can be influenced by mating primes. In addition to the growing recognition that psychology must generally devote more efforts towards replication (e.g., Asendorpf et al., 2013; Simons, 2014), these particular studies merit attention for several further and more specific reasons. First, they are motivated by and

conceptualized within a theoretical framework whose plausibility has been questioned (Newell & Shanks, 2014a, 2014b). In this framework, primes are assumed to have broad and long-term effects, influencing a wide range of possible downstream behaviors, and these influences are largely automatic. They assume, for instance, that a priming induction designed to trigger mating motives, such as looking at pictures of attractive opposite-sex members, can increase an individual's stated willingness to engage in risky driving behaviors (Greitemeyer et al., 2013). Transfer and generalization from the prime induction to the measured behavior would have to be very broad for such an influence to occur, and a considerable amount of research suggests that such broad transfer is very much the exception rather than the rule (Newell & Shanks, 2014a). They also assume that primes can influence behavior unconsciously, and again the evidence for such influences is debatable at best (Newell & Shanks, 2014a, 2014b).

Secondly, these priming effects might be mediated by processes that are rather different from – and less theoretically novel than – those envisaged by their proponents. Specifically, the design of many behavior priming studies leaves open the possibility that compliant participants are able to infer, and hence behave in accordance with, the experimenter's hypothesis (Durgin et al., 2009; O. Klein et al., 2012; Orne, 1962; Rosenthal & Rubin, 1978). It does not seem far-fetched to imagine that a male participant first asked to look at pictures of attractive opposite-sex members, and then to state his willingness to engage in risk-taking behaviors, might intuit and conform to the experimenter's hypothesis. Indeed it is even possible, particularly in laboratory experiments, that the experimenter might subtly and unintentionally convey this expectancy (O. Klein et al., 2012).

Finally, many previous claims of subtle behavior priming effects, some quite similar to those described above, have failed to withstand close scrutiny and been identified as possible

false positives (e.g., R. A. Klein, others, & Nosek, 2014; Pashler, Coburn, & Harris, 2012; Shanks et al., 2013). After completing some of the initial studies reported below, which were designed primarily to explore the generality and boundary conditions on these priming effects, we commenced a meta-analysis of the extant studies to try to understand the discrepant findings (our later experiments, particularly Studies 3, 6, 7 and 8, are more exact replications). This meta-analysis speaks to the issue of whether some published romantic priming effects might be false positives.

### **Meta-analysis of romantic priming effects on decision making**

The 15 studies included (those listed in Table 1) are all the reports, whether published or unpublished (e.g., dissertations), identified by searching ProQuest, PsycINFO, and Web of Science (search terminated in April 2015) using combinations of the terms ‘priming’, ‘mating/romantic motives/goals’, and ‘sex(ual) cue’, which examination revealed employed a mating prime-induction method and a decision-making dependent variable. The reference lists of those articles were examined and descendancy searches were conducted via Google Scholar to examine all articles subsequently citing them. We also contacted the authors of all identified reports requesting information about any additional unpublished data they possessed or knew about. For each publication we extracted the test statistic, means, standard deviations and sample size for the key experimental prediction, and calculated effect sizes from these. In some cases the authors kindly provided additional details. The complete set of data is available at <https://osf.io/zubfs/>.

In total these articles reported 43 independent effects, all but one of which were originally described as statistically significant. The results are depicted in the forest plot in Figure 1 and the black circles in the funnel plot in Figure 2<sup>1</sup>. In the latter, effect size is plotted against the standard

error of the effect size. Studies with larger samples have lower standard errors. The meta-analysis fails to find evidence of significant heterogeneity amongst the effects,  $Q(42) = 53.7$ ,  $p = 0.11$ ,  $I^2 = 19.6\%$ . Although it would be wrong to regard the different dependent measures as psychologically interchangeable, they appear to be statistically similar in terms of the effect size estimates they yield. Given the absence of substantial between-study heterogeneity, we have not attempted to identify moderator variables or effect modifiers.

The meta-analysis, using a random-effects model, yields an effect size of  $d = 0.57$ , 95% confidence interval (CI) [0.49, 0.65], a medium-sized effect, marked by the rightmost vertical line in Figure 2. The triangle around this line (the ‘funnel’) marks the region where the individual studies are expected to be distributed. Near to the top of the figure, studies should yield effect size estimates close to the mean effect size. These large studies are unlikely to yield estimates that diverge far from the mean effect, because their sampling error will be low, in the same way that estimates of the proportion of heads based on 1,000 coin-tosses will be close to 50%. Towards the bottom of the figure, studies are expected to be more widely distributed on either side of the mean effect size, because their sampling error will be high, in the same way that estimates of the proportion of heads based on 4 coin-tosses may be a long way above or below 50%. Importantly, the scatter of points should be symmetrical: studies should be as likely to deviate below as above the mean effect size.

This is clearly not what Figure 2 reveals. Studies with lower error do not converge on the mean effect size: they yield estimates that are consistently lower than the mean, while studies with more error yield estimates that are consistently greater than the mean (in other words, the datapoints are not symmetrically distributed). This asymmetry (captured by the red regression line) is statistically significant by the Egger test (Egger, Smith, Schneider, & Minder, 1997),

$t(41) = 6.24, p < .0001$ . Moreover, there is a striking lack of studies falling in the part of the funnel that is shaded gray. This is the region in which nonsignificant ( $p > .05$ ) results fall (the darker portion of this area depicts the region of marginal significance,  $0.10 > p > .05$ ).

If this pattern is not consistent with how a funnel plot is expected to appear, then what does it mean? Two obvious possibilities (albeit not the only ones: see Sterne et al., 2011, for discussion of the range of possible causes of funnel plot asymmetry) are that the published studies are affected by publication bias (selection) or  $p$ -hacking (inflation). On this interpretation, it is either the case that studies falling in the shaded area to the left of the vertical line have been conducted but – because they yielded nonsignificant results – not published, or studies have been artificially shifted in the funnel plot as a result of  $p$ -hacking (or some combination of both of these). Examples of  $p$ -hacking include continuing to test additional participants until a significant result is achieved (Yu, Sprenger, Thomas, & Dougherty, 2014), or removing outliers post hoc to attain significance, or collecting several dependent measures and only reporting the one(s) that yield significant results (John, Loewenstein, & Prelec, 2012; Simmons, Nelson, & Simonsohn, 2011). Whatever the cause (publication bias or  $p$ -hacking), the conclusion is the same: despite 42/43 statistically significant results, the published literature cannot be taken as providing a sound basis for estimating the true size of the effect of romantic priming on decision making. The published studies are consistent with a true effect much smaller than 0.5. Indeed, extrapolating from the datapoints to a study with zero standard error (Stanley & Doucouliagos, 2014), the effect size estimate is close to zero.

Another way of appreciating the extreme bias evident amongst these studies is to note that for every additional 30 participants added per group (prime/control), the effect size is expected to decrease by approximately 0.1 effect size (Cohen's  $d$ ) units. Thus while a study with

30 participants per group is expected to yield  $d \approx 0.7$ , the same study with 180 per group yields  $d \approx 0.2$ . (This estimate comes not from the funnel plot in Figure 2 but rather from a simple linear regression of effect size onto study sample sizes.)

It is important to stress that we are not concluding from this analysis that the true effect size is small (or zero). Nor are we suggesting that selection or inflation are any more prevalent in this literature than anywhere else in behavioral research. Indeed, many other examples of publication bias/*p*-hacking in psychology have been documented recently (e.g., Bakker, van Dijk, & Wicherts, 2012; Carter & McCullough, 2014; Flore & Wicherts, 2015), including in a meta-analysis of priming studies in another domain (religious priming; Shariff, Willard, Andersen, & Norenzayan, in press). Ferguson and Brannick (2012) estimated that amongst meta-analyses that tested for funnel plot asymmetry, it was present in around 20-40%. Rather, our more modest conclusion is that any firm inference about the true effect size based on the published research is unjustified. We simply do not know what studies may have been conducted but not published, nor do we know for certain that *p*-hacking has taken place. What we do know is that the funnel plot is irregular and that therefore the published research does not validly support any conclusions about the true effect of romantic priming on decision making.

### **Overview of Present Studies**

The 8 studies reported here employed combinations of two prime induction methods and eight dependent measures. The former involved either a text-based or a pictorial procedure for inducing a mating motive. In the text-based version, participants read about a romantic episode or about a neutral event. In the pictorial version, they viewed attractive opposite-sex members, or neutral people/scenes. Some studies (Festjens et al., 2014; Van den Bergh et al., 2008) have employed other methods which are not explored here (though they are noted in Table 1 for

completeness): in these studies, participants physically examined an item of mating-related clothing (e.g., a bikini or a pair of boxer shorts).

The dependent measures, based on previous studies in this field, involved stated willingness either to pay for various conspicuous consumption goods (Studies 1-5), to engage in risky behaviors in the domains of sexual or social behavior, substance abuse, driving, or gambling (Studies 5-7), or to pay for gains or to avoid losses (Study 8). In Studies 3 and 5 additional dependent measures (benevolence and nonconspicuous consumption, respectively) were included in order to match the procedures of previous studies as closely as possible. Although all the dependent measures were based on self-report, such measures appear to be a reliable and valid method to assess many risk-taking behaviors (see Brener, Billy, & Grady, 2003).

While some of the studies reported here combined prime inductions and dependent measures in combinations different from those employed in the published experiments on which they are modeled, others were close replications using identical combinations. For reference, Table 2 provides details of major differences between the studies reported in this article and the original studies on which they are based. As described below, past research has found both prime induction methods to be effective and has used them interchangeably.

### **Study 1**

The first experiment employed a text prime induction method, as used by Griskevicius et al. (2007, Studies 2-4) and Sundie et al. (2011, Study 2). Spending patterns were measured using a method similar to that employed in Griskevicius et al.'s (2007) Study 1. The key hypothesis is that priming male participants with mating motives will lead them (but not female participants) to increase their willingness to pay for publicly visible goods and services.

## Method

### Participants

In all experiments reported here we aimed to collect approximately 40 completed surveys per group for each gender, slightly above the median sample size (34) in previous studies.

Although our primary approach to sample size was a Bayesian one (see below), we note that this sample size is adequate to detect an effect of size  $d = 0.57$  (the meta-analytic effect size) with power  $(1 - \beta) > 0.80$ .

One hundred and seventy-three participants, recruited through the psychology participant panel at University College London (UCL), completed the study. Participants filled out a self-administered online survey for the chance to win one of four £20 online retail vouchers. Sixteen of the submitted surveys were excluded from subsequent analysis either because they took an inappropriate amount of time (less than 5 min or more than 1 hr) to complete the survey or because of unreflective responding such as giving the same response to all willingness-to-pay (WTP) questions (note that the statistical inferences below are not altered by including these datasets). This resulted in a final sample of 157 participants, of whom 83 were female and 74 were male. The mean age of these participants was 27.8 years ( $SD = 8.8$ ). The survey was administered online using Qualtrics survey software ([www.qualtrics.com](http://www.qualtrics.com)).

### Procedure

The study employed a 2 (Gender) x 2 (Priming Condition: Control vs. Experimental) between-subject factorial design. Participants were randomly assigned to one of the two priming conditions. Seventy-nine participants were assigned to each condition, with the gender split reported in Table 3.

Participants were first presented with introductory information about the aim of the study and gave their informed consent. The introductory information stated that the survey had been designed to ‘investigate consumer preferences and memory recall’. Participants were told that they were being asked to read a short text as part of a memory recollection exercise. The consumer preference questions were placed between the text and the memory questions so as to appear as a distractor allowing for memory decay.

**Priming.** The priming materials were identical to those used by Griskevicius et al. (2007, Studies 2-4). Participants in the experimental condition were requested to read a romantic scenario involving meeting a highly desirable person of the opposite sex during a holiday on a tropical island. Participants imagined spending a romantic afternoon as well as evening with this person and being strongly motivated to romantically pursue this relationship. The text ended with a kiss with the person and a profound feeling of excitement as to what the rest of the night may bring. Participants in the control condition read an emotion-laden scenario about getting ready to go to a concert with a same-sex friend. Participants imagined not being able to find the tickets for the show and frantically searching for them everywhere. The text finished with the friend showing up, tickets in hand, and an elated mood in anticipation of the entertaining evening ahead. The supplemental materials report a manipulation check confirming that the prime text significantly activates mating intentions.

**Willingness to pay.** Participants were presented with a randomized list of 10 items for each of which they had to state the maximum amount of money they would be willing to pay. The items on the list consisted of publicly visible goods and services purchased by women as well as men: a new watch, a dinner with friends at a restaurant (per person), a new mobile phone (not part of a contract), a short vacation abroad (transportation and accommodation, per person),

a new pair of shoes, a new pair of jeans, a new pair of sunglasses, a new fragrance, a new laptop, and a new portable media (e.g., MP3) player.

The first four of these items corresponded to goods and services utilized in Study 1 of Griskevicius et al. (2007). The fifth item (a new car) from that study was not included in the present research as it was presumed that most of the survey respondents would be in their twenties and not able to afford a new automobile. Participants provided numerical responses for each item. Average UK market prices for the goods and services were listed in brackets next to each. These averages were calculated on the basis of the frequency distribution of prices on online retail sites ([www.amazon.co.uk](http://www.amazon.co.uk)). Participants were given the option to indicate that they would not purchase a specific item, in which case they were instructed to enter a value of zero (this was rarely chosen).

After stating their willingness to pay for the target items, participants completed questions assessing their purchase motivation (stating to what extent their decisions to purchase goods and services depended generally on factors such as price, quality, opportunity to display wealth, etc), and their memory for the priming stories. For the sake of brevity analyses of these responses are not reported. As described in the supplemental materials, participants in this and all subsequent experiments also gave demographic information and reported their awareness of the purpose of the experiment via a series of funnel debriefing questions.

## **Results**

All statistical analyses reported in this article were computed in JASP (Love et al., 2015). The complete set of data for this and all subsequent studies is available at <https://osf.io/ytvj7/>.

**Willingness to pay.** The willingness to pay amounts were aggregated for each participant by summing across the 10 items. The sums were calculated on the basis of scaled values calculated as the ratio between the values entered and the associated anchor value.

The mean of participants' scaled values was used to measure their willingness to pay for conspicuous goods and services. Descriptive statistics are presented in Table 3. A 2 (Condition: Prime vs. Control) x 2 (Gender) between-subjects ANOVA revealed no significant interaction of gender and priming condition on the amount of money participants indicated they would spend,  $F(1, 153) = 0.17, p = .68, \eta^2_p = .001$ . In addition, neither the main effect of prime condition,  $F(1, 153) = 0.51, p = .48, \eta^2_p = .003$ , nor of gender,  $F(1, 154) = 0.01, p = .91, \eta^2_p = .00$ , was significant. To examine the specific experimental hypotheses, two planned contrasts were computed. Priming, critically, did not have a significant effect on male participants' willingness to pay,  $t(72) = 0.63$ , one-tailed  $p = .27$  (effect sizes for this and all subsequent critical contrasts are reported in Figure 3). Female participants in the experimental condition also did not pay more than those in the control condition,  $t(81) = 0.30$ , one-tailed  $p = .38$ . Table 3 reports the confidence intervals on the differences.

### **Bayes factor analysis**

Bayes factors were calculated for the simple contrast analysis on male and female participants' willingness to pay (see Table 3). Bayes factors ( $BF_{01}$ ) represent the probability of the data given the null hypothesis versus the probability of the data given the experimental hypothesis. Put more simply, the Bayes factor provides an indication of whether, given the data, the null hypothesis or the experimental hypothesis is more likely. The method used to quantify the Bayes factor and the relevant probabilities was the one developed by Rouder, Speckman,

Sun, Morey, and Iverson (2009) (using the Cauchy distribution, default scale  $r$  on effect size = 1.0).

A common yardstick is to interpret  $BF_{01}$  Bayes factors between 1 and 3 as ‘barely worth a mention’, ones between 3 and 10 as providing substantial support for the null hypothesis, and ones greater than 10 as providing strong support (Jeffreys, 1961; Wetzels et al., 2011). The Bayes factors for both female and, more importantly, male participants in Study 1 were both greater than 4 (all reported Bayes factors are two-sided). These findings suggest that the data are at least four times as likely under the null hypothesis compared to the experimental hypothesis. In light of the data obtained, the hypothesis that priming condition had an effect on male participants’ willingness to pay is much less likely than the hypothesis that priming did *not* have an effect.

## Study 2

In Study 1 participants’ willingness to pay was measured by open-ended, numerical responses for each item, with average UK market prices provided for each. This is slightly different from any of the methods employed by Griskevicius and colleagues. In Study 2 the method for measuring willingness to pay was identical to that of Griskevicius et al.’s (2007, Study 1) original experiment.

### Method

#### Participants

One hundred and twenty-eight UCL participants completed the survey, for a chance to win one of four £20 online retail vouchers. Data from 3 participants were excluded on the same basis as in Study 1. Seventy-one of the participants were female and 54 male, and their mean age was 26.2 years ( $SD = 7.2$ ).

#### Procedure

Except where mentioned, the procedure was identical to that of Study 1. The introductory instructions were made more similar to those employed by Griskevicius et al. (2007, p. 91) and stated that the goal of the research was to ‘investigate consumer behavior and decision making’. Participants were told they would all be asked to read the same standard scenario before filling out the survey in order to ensure that all participants were in ‘the same frame of mind’ when answering the following questions and to thus reduce extraneous bias in the research. The respondents were thereby ostensibly ‘led to believe that everyone was reading the same scenario and that the nature of the scenario was irrelevant to the study as long as it served to focus everyone on the same thing’.

**Priming.** The method used to prime the participants was identical to that of Study 1. However, the instruction preceding both of the primes was adapted to the revised cover story: ‘Please read the following text. All participants are requested to read the same standard scenario before completing the rest of the survey. This is to ensure that everyone is in the same “frame of mind” and to thus reduce extraneous bias in the study’.

**Willingness to pay.** Participants were presented with the same set of goods and services as in Study 1 to ascertain their willingness to pay for conspicuous purchases. The order of the goods and services was no longer randomized and the first four items in the set — that is, a new watch, a dinner with friends, a new mobile phone and a short vacation abroad — corresponded to the four goods and services of Griskevicius et al. (Study 1, 2007) that were included in Study 1 of the present research. To eliminate potential order effects, the arrangement of these first four items was reversed roughly half way through data collection and the same was separately done for the remaining six goods and services.

In line with the response format of Griskevicius et al.'s first experiment (Study 1, 2007), participants indicated how much money they would be willing to spend on the various items using an 11-point scale for each item. Each point on the scales represented a specific monetary value and the range of values was separately predefined for each item on the basis of the frequency distributions of prices on online retail websites. The middle value (6) on each scale was set to the respective average price used as the anchor value in Study 1. The minimum and maximum scale points were set as round integer values allowing for simple integer increments between the scale points given the average price as the middle scale point, and the scale increments between points was constant for each item. Participants were no longer given the option to not spend money on individual items.

## Results

**Willingness to pay.** The scale point values (not the monetary values associated with the scale points) were summed individually for each participant across the four items taken from Griskevicius et al.'s research (Study 1, 2007) and also across the larger set of ten items from Study 1 above.

Across the 10-item set, and replicating the findings of Study 1, gender did not significantly moderate the effect of priming condition on the amount of money participants indicated they would pay for goods and services,  $F(1, 121) = 1.27, p = .26, \eta^2_p = .01$ . In addition, neither the main effect of prime condition,  $F(1, 121) = 0.02, p = .88, \eta^2_p = .00$ , nor of gender,  $F(1, 121) = 2.22, p = .14, \eta^2_p = .018$ , was significant. Contrast analyses also did not support the costly signaling interpretation of male luxury good consumption: Men in the experimental condition did not spend more on conspicuous purchases than those in the control condition, and indeed the effect was in the wrong direction,  $t(52) = -0.65$ , one-tailed  $p = .74$ . The willingness to

pay of women primed with mating motives also did not significantly differ from those who were presented with the neutral prime,  $t(69) = 0.97$ , two-tailed  $p = .34$ .

Limiting the statistical analysis to the subset taken from Griskevicius et al.'s experiment ([Study 1, 2007](#)) did not change the pattern of results. There was no significant interaction between gender and priming condition on expenditure for the reduced set of goods and services,  $F(1, 121) = 0.77$ ,  $p = .38$ ,  $\eta^2_p = .006$ . Neither the main effect of prime condition,  $F(1, 121) = 0.00$ ,  $p = .99$ ,  $\eta^2_p = .00$ , nor of gender,  $F(1, 121) = 2.96$ ,  $p = .09$ ,  $\eta^2_p = .024$ , was significant. Men primed with the romantic scenario did not have a significantly higher willingness to pay in comparison to those in the control condition,  $t(52) = -0.60$ , one-tailed  $p = .73$ . The same result emerged with respect to the effect of priming condition on expenditure levels for female participants,  $t(69) = 0.65$ , two-tailed  $p = .52$ .

Bayes factors are reported in Table 3. These are again in excess of 4 for male participants for both the complete and the reduced item sets, indicating clear support for the null hypothesis.

## Discussion

Studies 1 and 2 failed to replicate the pattern of results of previous studies, in particular the findings of Study 1 of Griskevicius et al. (2007). Contrary to what was expected, priming male participants with mating motives did not lead them (in contrast to female participants) to increase their willingness to pay for publicly visible goods and services. The spending behavior of both male and female participants in the experimental conditions did not significantly differ from that in the control conditions.

## Study 3

Study 3 is a further attempt to obtain a priming effect, again making the procedure closer to that of the previously published studies. Specifically, the study adopts the same design as

Griskevicius et al.'s (2007) Study 2, with participants responding not only to consumption but also to benevolence questions, and using the same response format that they used. Griskevicius et al. observed a reliable tendency for romantic primes to increase males' conspicuous consumption judgments and to increase females' conspicuous benevolence judgments.

## **Method**

### **Participants**

One hundred and sixty-three UCL students completed the survey. They were given the option of receiving a £2 Amazon voucher or partial course credit for filling out a self-administered online survey. Data from 12 participants were excluded on the same basis as in the previous studies. This resulted in a final sample of 151 participants, of whom 87 were female and 64 were male. Their mean age was 27.7 ( $SD = 10.9$ ).

**Design.** The study employed a 2 (Participant Gender: Male vs. Female) x 2 (Priming Condition: Prime vs. Control) x 2 (Behavior: Consumption vs. Benevolence) x 2 (Conspicuousness: Conspicuous vs Inconspicuous) mixed-factorial design. Gender and priming condition were both between-subjects factors, and behavior and conspicuousness were both within-subjects factors. Participants were randomly assigned to one of the two priming conditions and then all answered questions on spending and helping.

### **Procedure**

Except where specifically mentioned, the procedure was identical to that of Study 2. The text prime was again used.

**Willingness to Pay.** The items, presented one at a time in randomized order, were the same goods and services used by Griskevicius et al. (2007, Study 2). The response format was also the same as employed in that study. Five items were conspicuous goods and five

inconspicuous. The 5 conspicuous consumption items were: a new car, a new watch, taking a group of friends out for dinner, a new mobile phone, and a nice holiday somewhere in Europe. The five inconspicuous consumption items were: basic toiletries (e.g., tissues), household medication (e.g., headache medication), a bedroom alarm clock, kitchen staples (e.g., salt), and household cleaning products (e.g., tile cleaner). Participants indicated how much they would be willing to pay on a 9-point scale: 1 (much less than the average person), 5 (about average), and 9 (much more than the average person).

**Motivational Booster.** As in Griskevicius et al.'s (2007) Study 2, a motivational booster was given after the randomly assigned first block of either consumption or benevolence questions. Participants were told they were completing a recall task and that they were to imagine themselves in the scenario they had read at the start of the experiment. In the experimental condition participants were told they had up to 3 minutes to describe their ideal mate. In the control condition participants were asked to spend up to 3 minutes describing the anticipated concert venue.

**Willingness to Help.** Participants were asked to indicate their willingness to help in 10 randomized situations. Five were conspicuous benevolence situations: volunteering at a homeless shelter, helping to build houses for poor families, teaching underprivileged youths how to read, mentoring a young person, and volunteering in a children's hospital. The other 5 were inconspicuous benevolence situations: spending an afternoon each weekend picking up rubbish alone in a park, taking much shorter showers in order to conserve water, putting money into a stranger's parking meter when time had expired, posting a letter someone had dropped on the way to the post office, and going to the library to drop off a found library book in the drop box. Responses were given on the same 9-point scale used for the consumption items above.

## Results

**Willingness to Pay and Help.** The conspicuous and inconspicuous consumption and benevolence scores were aggregated for each participant by averaging across each set of 5 questions. A full statistical analysis, revealing a pattern of strategic sex-specific displays similar to that observed by Griskevicius et al. (2007, Study 2), is provided as supplemental materials. In brief, it demonstrates that our measurement instrument is sensitive to variation in gender, domain, and conspicuousness. For example participants rated themselves as more willing than the average person to pay for acts of benevolence but less willing than the average person to pay for consumption items; and rated themselves as relatively more likely to engage in conspicuous compared to inconspicuous activities.

However whether participants were in the experimental condition viewing the romantic prime or in the control condition viewing the neutral prime did not influence how likely they were to engage in conspicuous or inconspicuous consumption or benevolence. The relevant means are presented in Table 3 together with the confidence intervals on the differences. In contradiction to Griskevicius et al.'s (2007) findings, there was no reliable tendency for romantic primes to increase males' conspicuous consumption WTP judgments nor to increase females' conspicuous benevolence WTP judgments. Table 3 also reports the relevant Bayes factors which are again in excess of 4 for male participants, supporting the null hypothesis.

## Study 4

This study attempts to see if an increase in conspicuous consumption can also be found using a different cover story and prime (pictures of attractive opposite-sex members). These were adapted from Study 1 of Griskevicius et al. (2007) which also used picture primes.

## Method

Except where noted this study was identical to Study 3.

### **Participants**

One hundred and fifty-two participants, recruited through the UCL psychology participant panel, completed the survey. Twelve datasets were excluded on the same basis as previously. This resulted in a sample of 140 participants of whom 68 were males and 72 female. Their mean age was 22.3 ( $SD = 6.1$ ).

### **Procedure**

A 2 (Gender: Male vs. Female) x 2 (Priming Condition: Prime vs. Control) factorial design was employed.

The introductory information simply stated that participants would have to do ‘Two brief unrelated studies to provide materials for future experiments.’ The first survey was said to be a ‘Picture Preference’ task, rating photographs for a future experiment, and the second survey was said to be a market research questionnaire on consumer preferences.

**Priming.** The primes were adapted from Griskevicius et al. (2007, Study 1). In the experimental condition participants viewed photos of three attractive opposite sex faces and were told that each individual was interested in pursuing a relationship with them. They were then asked to rate each face’s attractiveness on a scale from 0 (very unattractive) to 10 (very attractive). Participants were subsequently asked ‘Who would you like to go on a first date with the most?’ and were again shown the images. For the chosen individual participants then spent up to 3 min describing their perfect date with that person.

Participants in the control condition viewed three photos of streets and rated how much they liked each on a scale from 0 (dislike extremely) to 10 (like extremely). They then selected which street they liked the most out of the three, and spent up to 3 min describing “the most

pleasant weather conditions in which to walk around and look at the buildings.” This is identical to the control task used by Griskevicius et al. (2007, Study 1) except that their participants only viewed one street photo. By requiring a selection from amongst 3 photos, the present control condition more closely matches the task in the experimental condition.

**Conspicuous Consumption.** Participants were then told they were starting study two, which required them to indicate how much they would be willing to pay for various goods and services in comparison to the average person, again using a 9-point scale in relation to the average person. As this study was only looking at the influence of primes on conspicuous consumption, only 5 questions were included.

## Results

**Conspicuous Consumption.** An aggregate conspicuous consumption score was generated by averaging the 5 willingness-to-pay scores. Then a 2 (Gender) x 2 (Condition: Prime vs. Control) ANOVA was conducted. This revealed no main effect of gender,  $F(1, 136) = .20, p = .65, \eta^2_p = .001$ , or condition,  $F(1, 136) = .12, p = .73, \eta^2_p = .001$ , and no interaction,  $F(1, 136) = 0.65, p = .42, \eta^2_p = .005$ . Planned contrasts found no priming effect in either male,  $t(66) = 0.30$ , one-tailed  $p = .38$ , or female,  $t(70) = -0.89$ , two-tailed  $p = .38$ , participants. Means are listed in Table 3. The Bayes factor for males is once again in excess of 4.

## Study 5

In Study 5 we again employed the picture prime procedure but extended the dependent measure to include risk-taking behaviour (Greitemeyer et al., 2013) as well as inconspicuous consumption (Griskevicius et al., 2007).

The experiment compared risk-taking behaviour in two groups of male participants who were either exposed to the experimental prime or a control one. The priming procedure closely

matched that used by Griskevicius et al. (2007, Study 1) and in the present Study 4. In the experimental condition participants were asked to rate the attractiveness of women and to imagine a perfect date with one of them. In the control condition participants rated the competence of male managers and imagined what perfect team-working with one of them would be like. The effect of these primes on risk-taking behaviour and consumption was measured in three different questionnaires. One focused on social situations, one on reckless driving, and one on conspicuous and inconspicuous consumption. The effect of priming on risky driving behavior was assessed by Greitemeyer et al. (2013, Experiment 3). Greitemeyer et al. (2013, Experiment 4) included questions on social behaviour in a larger set of risk-propensity questions (the DOSPERT Scale; Blais & Weber, 2006). Here we assess it as a separate domain.

The major difference between the design of this study and those of Griskevicius et al. (2007) and Greitemeyer et al. (2013) was that a more appropriate control prime was employed in this experiment: it involved pictures of people and hence was socially-oriented (though not mating-oriented), whilst the previous research used images of streets.

## **Method**

### **Participants**

For this experiment, 85 males were recruited from the UCL psychology participant panel. Six datasets were excluded on the same basis as previously. The remaining participants had a mean age of 22.9 ( $SD = 3.5$ ) with 39 assigned to the experimental group and 40 to the control group.

**Apparatus and Design.** The picture primes for the experimental condition were taken from *HotnessRater.com*. This website allows users to rate the attractiveness of pictures of women from 0-10. All pictures used in this study had at least 1,500 user votes and an average rating of

9.5. The original source used by Greitemeyer et al., *binichsexy.de*, is no longer active.

Greitemeyer et al. (2013) had used a similar source and the same criteria of a minimum user vote and rating to obtain their mating primes. Primes for the control condition were pictures of smartly dressed men who might come from an office-type working environment.

Three questionnaires were designed to assess risk-taking behavior in everyday situations as well as consumption. Driving behavior was assessed using the same 10-item questionnaire as Greitemeyer et al. (2013), originally designed by Ben-Ari, Florian, and Mikulincer (1999). Participants were given a scenario, such as “You are on your way to a weekend vacation. A very slow lorry is driving just in front of you. A continuous white line separates you and the other direction of the road. What do you think are the chances that you will go for an overtake?” They then had to indicate the likelihood of taking the suggested action on a scale of 1-100.

Social risk-taking was measured in 5 different scenarios of possible confrontation. Again, participants were given a scenario and this time two possible actions. One was the ‘risky’ option, which included confronting another person, and the other the ‘safe’ one, which did not. The following is an example scenario: “You are shopping at your local store and in queue behind a rather attractive woman. As she approaches the till to pay for her groceries the shop assistant loudly makes an insulting comment towards her. What do you think the chances are that you A. step forward and tell him to apologize, B. keep queuing and let her deal with it?” Here, A is the risky option and B the safe one. Participants were asked to indicate how likely they were to take each of the suggested actions on a scale of 1-100.

Finally, participants’ conspicuous and inconspicuous spending behavior was examined using tests similar to those used previously (but with only 5 items each), with responses made on

scales similar to those employed in Study 2 (for conspicuous items) and Study 3 (inconspicuous items).

### **Procedure**

The experiment consisted of a priming phase followed by three blocks of questions. During the priming stage, participants in the experimental condition initially rated the three pictures of women individually, from 1 ('not at all attractive') to 10 ('extremely attractive'). They were then asked to choose which of the women they would like to take out on a date and spent about 3 min writing about what the perfect date with that woman would be like. In the control condition participants rated the three pictures of businessmen individually, from 1 ('not at all competent') to 10 ('extremely competent'). They then had to choose which of them they would like to work with most and write a short text about what a perfect team-working experience with that person would be like. After the priming stage, both groups were given the same three blocks of questions in a randomized order.

### **Results**

**Risk-taking.** For each participant, scores from the individual items on the risky driving and social risk-taking questionnaires were averaged, with responses reverse coded where relevant. High scores indicate greater risk-taking. Due to a programming error data for 2 of the social risk-taking questions were not correctly recorded for 22 participants and their mean scores are therefore based on the remaining 8 questions.

Means and Bayes factors are listed in Table 3. There was no significant priming effect on mean risky driving scores,  $t(77) = -0.85$ , one-tailed  $p = .80$ , or on mean social risk-taking,  $t(77) = .59$ ,  $p = .28$ . Hence, the results from Greitemeyer et al. (2013) were not replicated. Mating primes did not increase willingness to take risks while driving, which is especially surprising

considering the unusually large effect reported by Greitemeyer et al. (2013),  $d_s = 1.62$ . Similarly, mating primes were not found to increase men's willingness to engage in risk-taking or confrontation in social situations.

**Conspicuous and inconspicuous consumption.** There was no effect of priming condition on participants' spending behavior for conspicuous,  $t(77) = .14$ , one-tailed  $p = .44$ , or inconspicuous items,  $t(77) = .08$ ,  $p = .93$ .

### Study 6

Study 5 once again failed to detect any effect of priming mating motives on males' consumption behavior, and extended this null effect to risk-taking. In Study 6 we undertook another evaluation of priming effects on risk-taking, again following the procedure of Greitemeyer et al. (2013), and incorporating measures of sexual, substance, and gambling risk-taking. The present study is a replication of Greitemeyer et al.'s (2013) Experiments 1 and 2 in which sexual and gambling risk-taking, respectively, were measured. Baker and Maner (2008) reported a similar study in which the primes were attractive (versus unattractive) opposite-sex faces and the gambling dependent measure was risky choice in a simulated blackjack game.

### Methods

#### Participants

Unlike Study 5, both male and female participants were included in the sample to allow us to determine whether our questionnaires are sensitive to gender differences. One hundred and eight UCL students completed the study online, with 11 datasets being excluded on the same basis as previously. This resulted in a final sample of 97 participants of whom 56 were males and 41 female. The mean age was 22.0 ( $SD = 5.5$ ). Approximately half (43/97) the participants

were tested in person via a laptop computer in a quiet room whereas the remainder were tested online.

### **Procedure and materials**

Except where noted, the procedure was identical to that of Study 5 with the following changes. Participants were asked their gender at the start of the experiment so that appropriate opposite-sex pictures could be presented. For the control group we reverted to the pictures of street scenes to make the experiment more similar to that of Greitemeyer et al. (2013).

Presentation of the three risk-taking question sets (sexual risk-taking, gambling, and substance abuse) was randomized. Four gambling questions (two from Greitemeyer et al., 2013, plus two similar new items) were constructed to measure gambling risk-taking. For example, one question asked participants to choose between a lottery ticket to win £100 where 1 million people enter the lottery and 50,000 winners are chosen (the conservative option), and a ticket to win £500 where 1 million people enter the lottery and 5,000 winners are chosen (the risky option). The sexual risk-taking questionnaire incorporated 8 items from the same source as Greitemeyer et al. (2013), such as “If I find someone attractive, I would agree to sexual intercourse even if it is unprotected” and scored on an 11-point scale from 0 (“strongly disagree”) to 10 (“strongly agree”). A 14-item substance risk-taking questionnaire was constructed asking participants to judge how likely (0 = “very unlikely”, 10 = “very likely”) they were, for instance, to consume a Class A drug (e.g., cocaine) in their lifetime.

### **Results**

For each participant, scores from the individual items on the substance and sexual risk-taking questionnaires were averaged, with responses reverse coded where relevant. High scores indicate greater risk-taking. Responses on the gambling questions were scored as 0 if the

conservative option was selected and 1 if the risky option was selected, and then summed, yielding a score between 0 and 4. Means scores on each measure for males and females are reported in Table 3 together with the 95% CIs on the priming effects, and the ensuing Bayes factors. Mode of testing (experimenter present vs. online) had no effect on the results.

**Sexual risk-taking.** A 2 (Gender) x 2 (Condition: Prime versus Control) ANOVA revealed a significant main effect of gender,  $F(1, 93) = 7.34, p < 0.01, \eta^2_p = .072$ . In the domain of sexual risk-taking, males were substantially more risk-seeking than females, as found by Greitemeyer et al. (2013, Experiment 1). However there was no effect of priming condition,  $F(1, 93) = 1.15, p = 0.29, \eta^2_p = .011$ , and no interaction,  $F < 1$ .

**Gambling risk-taking.** A comparable ANOVA revealed no main effects and no interaction,  $F < 1$  in each case. The absence of a significant gender effect is consistent with the results of Greitemeyer et al. (2013, Experiment 2).

**Substance risk-taking.** A comparable ANOVA revealed no main effects and no interaction,  $F < 1$  in each case.

Study 6 once again fails to obtain any evidence for priming effects on measures of risk-taking, in this case in the domains of sexual, gambling, and substance-abuse behavior. There was no hint that risk-taking is affected by romantic primes more in males than females, as Greitemeyer et al. (2013) reported for sexual (Experiment 1) and gambling (Experiment 2) behavior (indeed Greitemeyer et al.'s Experiment 2 found significantly less risk taking in females in the mating prime compared to the control condition). Importantly, these null effects do not simply reflect complete insensitivity with our participants using these measures, as a robust gender effect on sexual risk-taking was observed. There was no gender effect on the gambling questionnaire. Both of these patterns align with what Greitemeyer et al. (2013) found. For the

domains common to this study and the experiments of Greitemeyer et al. (sexual and gambling behavior) the overall mean risk-taking scores were very comparable.

### **Studies 7a and 7b**

In Study 7a we undertook an even closer replication of Greitemeyer et al.'s (2013) Experiment 1 measuring sexual risk-taking. In Study 7b we did the same for their Experiment 2 measuring gambling. To test the possibility that participants might respond differently in laboratory and online conditions, participants in Study 7a were tested in both contexts.

## **Methods**

### **Participants**

Only male participants were included in the sample. 126 participants completed Study 7a, with 6 datasets being excluded on the same basis as previously. This resulted in a final sample of 120 participants with a mean age of 21.7 ( $SD = 3.7$ ). Thirty-seven UCL participants were tested in laboratory cubicles (this is larger than Greitemeyer et al.'s sample,  $n = 31$ ), whereas the remainder, recruited via Prolific Academic ([www.prolific.ac](http://www.prolific.ac)) or the UCL participant pool, were tested online. 109 participants, all recruited via Prolific Academic, completed Study 7b, with 3 datasets being excluded. This resulted in a final sample of 106 participants with a mean age of 27.3 ( $SD = 10.3$ ). Here, all participants were tested online.

### **Procedure and materials**

The procedure was identical to that of Study 6 with the following changes. In Study 7a participants completed the sexual risk-taking questionnaire. In Study 7b they answered four gambling questions, identical to those used by Greitemeyer et al. (2013, Exp. 2). In both studies, a final set of questions asked all participants to provide attractiveness ratings of the 3 pictures of women and the street scene. Combining data across both experiments, the former received

significantly higher ratings:  $M = 6.58$  ( $SD = 1.38$ ) and  $M = 3.99$  ( $SD = 1.87$ ) for the faces and street picture, respectively,  $t(218) = 17.40$ , two-tailed  $p < .001$ .

## Results

Means scores are reported in Table 3 together with the 95% CIs on the priming effects and Bayes factors.

**Study 7a: Sexual risk-taking.** There was no effect of priming condition. Men primed with mating motives showed no tendency towards more risky behavior in comparison to those in the control condition,  $t(118) = 0.07$ , one-tailed  $p = .47$ . The subset of participants tested under laboratory conditions showed the same absence of a priming effect [ $M = 3.30$  ( $SD = 1.73$ ) and  $M = 3.62$  ( $SD = 2.04$ ) for the priming and control groups, respectively],  $t(35) = -0.52$ , one-tailed  $p = .70$ .

**Study 7b: Gambling risk-taking.** There was again no effect of priming condition. Men primed with mating motives did not have a significantly higher willingness to choose risky gambles in comparison to those in the control condition,  $t(104) = -0.35$ , one-tailed  $p = .64$ .

## Study 8

In the final study we examine the reproducibility of the effect of mating and self-protection primes on another dependent measure from the domain of decision making, loss aversion, employing a method closely modelled on that of Li et al. (2012, Study 1). Self-protection primes activate thoughts of fear and the need to protect oneself against danger, an evolutionary motivation assumed to be gender nonspecific. Li et al.'s key findings were that, relative to the neutral control condition, a mating prime rendered males but not females *less* averse to losses, while a self-protection prime *increased* loss aversion, regardless of gender.

Study 8 was preregistered and employed a large sample, comprising (like Study 7a) samples tested in the laboratory and online. Preregistration virtually eliminates the possibility of publication bias and *p*-hacking (Chambers, Feredoes, Muthukumaraswamy, & Etchells, 2014), because the study protocol and analysis plan are specified ahead of time. In a large preregistered experiment, Gomes and McCullough (in press) failed to reproduce a different priming effect, of religious primes on prosocial behavior.

## **Methods**

### **Participants**

The total sample comprised 670 participants of both genders, with 20 datasets being excluded on the same basis as previously. This resulted in a final sample of 650 participants (325 males) with a mean age of 27.9 ( $SD = 9.3$ ). Seventy UCL participants were tested in laboratory cubicles, whereas the remainder were recruited via Prolific Academic and tested online. Participants were allocated at random to one of 3 groups, control, mating prime, and self-protection prime.

### **Procedure and materials**

The study was pre-registered in detail at <https://osf.io/g2nek/>. The priming stage was similar to previous experiments but also included a self-protection text, as used by Li et al. (2012, Study 3). The test phase included 3 measures, presented in a randomized order. Participants responded to a set of questions, identical to those used by Li et al., designed to measure loss aversion. Participants were presented with 7 different attribute items (being liked, being respected, providing for their family, safety from physical danger, safety from contagious disease, dating ability and romantic relationship stability) and asked to imagine they were on the 50th percentile on each of the attributes. They then indicated how much of £1000 they would pay

to improve (gain) or avoid a decrease (loss) on each of the attributes by 10% and 30%. The order of the 28 questions [7 attributes x 2 percentiles (30%, 50%) x 2 outcomes (gain/loss)] was randomized.

Li et al. (2012, Study 1) elicited willingness-to-pay judgments with an 11-point scale, from \$0 to \$1000 in increments of \$100. The mean effect of the mating prime on male participants' loss aversion was a reduction of only about \$7, which is equivalent to a change of one point on the 11-point scale for 2 of the 28 items rated by a participant in the prime condition compared to a participant in the control condition. Given this very small influence, we deemed it appropriate to replace the 11-point scale with a continuous scale where participants could enter any monetary amount between £0 and £1000.

Participants also completed another loss aversion test, taken from Tom, Fox, Trepel, and Poldrack (2007). In this test, participants were presented with 20 gambles, in randomized order. Each gamble offered a 50% chance of winning an amount shown in green and a 50% chance of losing an amount shown in red. The wins ranged from £10-£40 (in increments of £2) and the losses from £5-£20 (in increments of £1), and half the gambles displayed greater losses than gains. Participants decided whether to accept or reject each gamble.

A single gamble similar to those used by Greitemeyer et al. (2013) and in Studies 6 and 7b was also included in the test. Participants chose between a conservative option (a 1 in 4 chance of winning £20) and a riskier one (a 1 in 25 chance of winning £50).

## **Results**

Data collection proceeded in accordance with the preregistration plan with no noteworthy deviations. A full description of the results is provided as supplemental material. We computed a loss aversion measure in the same way as Li et al. (2012), comprising a single score calculated

for each participant by subtracting the amounts paid to avoid a loss from the amounts paid to achieve a gain, aggregated across all attributes and both percentile changes. Negative scores thus imply loss aversion. Mean scores in the mating prime and control conditions are reported in Table 3 together with the 95% CIs on the priming effects and Bayes factors.

Participants were loss-averse on average,  $-\pounds 9.19$ , 95% CI  $[-15.22, -3.17]$ , meaning that they were willing to pay more to avoid a loss than to acquire a comparable gain. Those tested online were significantly more loss averse than those tested in the laboratory. However mode of testing did not interact with any other factors. Combined across this factor, there were no effects of priming condition. Males primed with mating motives were no less averse to losses than those in the control condition,  $t(216) = 0.02$ , one-tailed  $p = .49$ , and the Bayes factor in support of the null is nearly 10. The effect for females was also nonsignificant,  $t(215) = -0.06$ , two-tailed  $p = .95$ . Also contrary to the predictions, the self-protection prime did not make either males,  $t(214) = 0.16$ , one-tailed  $p = .44$ , or females,  $t(214) = -0.83$ , one-tailed  $p = .80$ , more averse to losses.

### General Discussion

The studies reported here can be readily summarized: They have failed to detect any effects of mating primes on risk-taking, expenditure on publicly consumed goods and services, or loss aversion. Indeed, as indicated by the Bayes factor analyses, their results strongly support the null hypothesis of no effect. Together with the asymmetric funnel plot shown in Figure 2, which implies the existence either of  $p$ -hacking in previously published studies or selective publication of results (or both), our results suggest the real possibility that romantic primes have no meaningful effect on decision-making behaviors.

Although the major findings comprise null results, our experiments were able to replicate other anticipated effects unrelated to priming, including quite subtle ones. For example we confirmed in Study 3 that males and females differ in their judgments concerning consumption and benevolence, depending on whether the behaviors in question are conspicuous or inconspicuous, mirroring the pattern reported by Griskevicius et al. (2007, Study 2); in Study 6, we confirmed that males were substantially more risk-seeking than females but only in the domain of sexual risk-taking and not gambling, exactly as found by Greitemeyer et al. (2013); and in Study 8 participants were significantly loss averse.

There is no such thing as an ‘exact’ replication (Stroebe & Strack, 2014) and hence it must be acknowledged that the published studies (notwithstanding the evidence for *p*-hacking and/or publication bias) may have obtained genuine effects and that undetected moderator variables explain why the present studies failed to obtain priming. Some of the experiments reported here differed in important ways from those on which they were modeled (although others were closer replications and even these failed to obtain evidence of reliable romantic priming). As Stroebe and Strack (2014) point out, what is crucial is not so much exact surface replication but rather identical operationalization of the theoretically-relevant variables. In the present case, the crucial factors are the activation of romantic motives and the appropriate assessment of consumption, risk-taking and other measures. For instance testing the same participant population as an original study but with different pictures of attractive individuals is likely to be a closer and more valid replication than using the same pictures but in participants from a different culture with different views of attractiveness. In the former but not the latter, a theoretically-relevant factor – activation of romantic motives – will be reproduced.

Is it possible that the present studies failed to achieve these requirements? It seems unlikely. Published studies have employed a range of priming methods with no hint that these methods differ in efficacy. For instance, both picture and text primes have been used, with and without participants writing about their thoughts. Indeed published studies have found priming effects with minimal inductions such as that employed by Kim and Zauberan (2013, Study 2) in which participants rated the attractiveness of each of 7 photographs, shown for 7 sec each, a task which presumably took little more than 1 min. Our participants come from a very similar language and cultural population as those previously studied (themselves quite varied), and the priming materials surely activated romantic concepts effectively in our participants (as explicitly confirmed in Studies 7a and 7b and in the manipulation check reported in the supplemental materials). Our measures of consumption, risk-taking and other decisions employed similar or identical methods to those used previously.

One potential moderator variable is study format (laboratory or online) with the majority of published studies using laboratory data collection and the majority of our studies using online collection. Is it likely that this difference is crucial? Some of the previous studies (Chan, 2015; Kim & Zauberan, 2013, Study 5; Sundie et al., 2011, Study 2) were conducted online (as were other studies within this general field, e.g., Wang & Griskevicius, 2014) and obtained priming effects similar to those found in their companion laboratory experiments. At the same time, two of the experiments reported here (Studies 7a and 8) specifically asked whether priming effects differed between the two formats and obtained no evidence of this, consistent with a growing literature from a range of domains demonstrating a similar conclusion (Germine et al., 2012; R. A. Klein et al., 2014). Indeed in their meta-analysis of religious priming studies, Shariff et al. (in press) tested whether the results varied between online and laboratory studies and found no

evidence that they did. This is certainly something to be explored further in future research, but the available evidence provides no strong support for this being an important moderating factor. One of the clear advantages of online experiments is that direct human interaction between researcher and participant is limited thus leaving less room for experimenters to bias participants' responses.

A second potential moderator is whether participants are primed with or have a bias towards short- versus long-term mating motives. This refers to the fact that individuals differ in their preference for uncommitted, low-investment, unrestricted sexual strategies in contrast to committed, high-investment, restricted, ones. As well as this intrinsic preference, participants can be presented with inductions designed to trigger either short- or long-term mating motives, for example via texts that emphasize either the low- or high-commitment interests of the protagonists in a romantic encounter. Some research (Sundie et al., 2011) has suggested that romantic priming of male conspicuous consumption is limited to those following a low-investment strategy and to short-term mating primes. Although this moderator should continue to be evaluated in future research, it is unlikely that it is an important factor, for several reasons: (a) the evidence reported by Sundie et al. (2011) is inconclusive: they found significant priming in participants presented with a short-term prime but not in those presented with a long-term prime, and likewise they found significant priming in unrestricted but not in restricted males; however in neither case did they report that these factors significantly moderated the degree of priming; (b) other studies (Greitemeyer et al., 2013, Experiments 3 & 4; Griskevicius et al., 2007, Study 2, see Footnote 1, p. 90) found no difference between short- and long-term mating primes, while Griskevicius et al. (2007, Study 3) found no effect of male participants' preference for pursuing a short-term compared to a long-term mating strategy on the extent to which consumption

behavior was primed; (c) the effects obtained in published studies on other aspects of decision-making (Table 1) did not depend on (and indeed took no account of) short- versus long-term mating motives.

The conclusion we drew in the Introduction from the funnel plot asymmetry revealed in Figure 2 was that the published studies do not license any firm statement about the true magnitude of the effect of mating primes on decision making. Despite the fact that 43 contrasts on data from 3252 participants yielded 42 significant priming effects and a meta-analytic effect size of  $d = 0.57$ , those studies do not allow the true effect size to be determined unambiguously because of the correlation they manifest between effect size and sample size. But in light of the results of the additional data we have collected, this conclusion can now be considerably strengthened: We infer that the true effect size for those 43 contrasts is very close to zero. Egger's test yields an estimated intercept of  $d = -0.06$   $[-0.27, 0.14]$ , and for the main experimental tests in Studies 1-8 the meta-analytic effect size is  $d = 0.00$   $[-0.12, 0.11]$ . The individual effect sizes<sup>2</sup> contributing to the latter are illustrated in the open triangles in Figure 2 and in the forest plot in Figure 3. Strikingly, the two datasets (previously-published studies represented by the black circles in Figure 2, and the main results across Studies 1-8 reported here, represented by the open triangles) fall into complete alignment. The previous studies show evidence of publication bias and the correction for this yields an estimated effect size close to zero, similar to that of the studies reported here (for which a publication bias correction is not required).

Our conclusion may appear surprising but it is consistent with meta-analyses elsewhere in decision-making research. For example, Nieuwenstein et al. (2015) described a meta-analysis of studies on the 'unconscious thought' effect and concluded that, despite many positive findings

(e.g., Dijksterhuis, Bos, Nordgren, & van Baaren, 2006), the true effect size after correcting for publication bias is negligible. Renkewitz, Fuchs, and Fiedler (2011) reanalyzed a meta-analysis by Dato-on and Dahlstrom (2003) on priming effects in decision making and found evidence of publication bias, leading them to conclude that Dato-on and Dahlstrom had overestimated the true effect size. And Carter and McCullough (2014) reported a meta-analysis which implied that – after correcting for publication bias – the tendency for acts of self-control to cause ‘depletion’ of a common resource has an effect size that is no greater than zero. In all of these cases substantial bodies of evidence are severely compromised by publication bias.

Although the present results cast some doubt on the claim that aspects of decision-making behavior can be primed by the activation of mating motives, there are other phenomena which, though superficially similar, are not at all challenged by the null results obtained here. For example, it has been demonstrated that the presence of an opposite-sex observer may alter an individual’s willingness to take risks (e.g., Ronay & von Hippel, 2010). Such effects may be mediated by direct physiological arousal and indeed Ariely and Loewenstein (2006) showed that sexual arousal rendered males more willing to take risks. The concept of behavior priming (Molden, 2014) does not need to be invoked to explain such effects.

Behavior priming is both theoretically and empirically questionable (Newell & Shanks, 2014a, 2014b). It is theoretically problematic because the claim that subtle cues can unconsciously activate the “mental representations of social targets, events, or situations that then influences subsequent evaluations, judgments, or actions” (Molden, 2014, p.4) runs counter to well-established theories in which behavior is understood in relation to the conscious, cognitive appraisal of cues and situations (Baumeister, Masicampo, & Vohs, 2011; Lovibond & Shanks, 2002; Newell & Shanks, 2014b). It is empirically problematic because – consistent with

the findings reported here – many of the most influential demonstrations of behavior priming have proved to be very difficult to replicate (e.g., Gomes & McCullough, in press; R. A. Klein et al., 2014; Pashler et al., 2012; Shanks et al., 2013).

As noted in the Introduction, research on the effects of mating primes on decision-making behaviors is connected to a larger body of work in which (a) these primes have been paired with other dependent measures such as creativity and aggression, and (b) decision-making behaviors have been paired with other types of primes (see Kenrick & Griskevicius, 2013, for a review). Our findings of course must not be over-generalized, but future research should take seriously the possibility that publication bias and/or *p*-hacking are present in these associated domains too and should take steps to minimize that risk. Preregistration represents an important new method that future studies could profitably employ to enable firmer conclusions about these priming effects to be reached. This refers to the practice of specifying the study protocol and analytic strategy ahead of data collection. Preregistration, with its attendant minimization of the possibility of publication bias and *p*-hacking (Chambers et al., 2014), offers the possibility of placing romantic priming of decision-making behaviors (and related forms of priming) on a firmer footing than has been achieved to date.

**Footnotes**

1. Despite only 1 of the 43 contrasts being originally reported as nonsignificant (Griskevicius et al., 2007, Study 1, men), in Figure 1 there are 10 whose lower 95% confidence interval includes zero (and these fall inside the shaded dark gray area in Figure 2). The reason for this discrepancy is that for some studies the effect sizes could be computed in different ways (e.g., from within-versus between-subjects contrasts; from test statistics versus the authors' own reported effect size estimates). Our aim was to maximize consistency across the meta-analysis but this sometimes yielded a result that was nonsignificant even though an alternative method did yield a significant result. This discrepancy has no bearing on the meta-analysis, however, which is based on the effect size estimates themselves and not on their statistical significance.

2. The dependent variables in the meta-analysis of Studies 1-8 are not all statistically independent: Within-study measures come from the same participants. We include all those data points for illustrative purposes. The results do not change substantially if all non-independent data points from a study are collated into a single composite effect size.

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**Table 1**

Studies of the influence of mating primes on various aspects of decision making. **Bold** indicates studies reported in this article.

|                           | Prime Method   |  |  |
|---------------------------|--|--|--|
| Decision-making domain    | Opposite-sex pictures  | Romantic text  | Other formats                                      |
| Conspicuous consumption   | Griskevicius et al. (2007, Study 1)<br>Sundie et al. (2011, Study 1)<br><b>Studies 4 &amp; 5</b>                       | Griskevicius et al. (2007, Studies 2 & 3)<br>Sundie et al. (2011, Studies 2 & 3)<br><b>Studies 1-3</b> | Festjens et al. (2014, Study 3)<br>Chan (in press) |
| Benevolence               | Griskevicius et al. (2007, Study 1)  | Griskevicius et al. (2007, Studies 2-4)<br><b>Study 3</b>  |  |
| Gambling                  | Baker & Maner (2008)<br>Greitemeyer et al. (2013, Exp 2)<br>Li (2012)<br>McAlvanah (2009)<br><b>Studies 6 &amp; 7b</b> |  |  |
| Social risk-taking        | <b>Studies 5 &amp; 6</b>   |  |  |
| Sexual/health risk-taking | Greitemeyer et al. (2013, Exp 1)<br>Hill & Durante (2009, Study 1)<br><b>Studies 6 &amp; 7a</b>                        | Hill & Durante (2009, Study 2)   |  |
| Driving risk-taking       | Greitemeyer et al. (2013, Exp 3)<br><b>Study 5</b>   |  |  |
| Substance risk-taking     | <b>Study 6</b>   |  |  |
| Physical risk-taking      |  |  | Baker and Maner (2009)                             |
| Loss aversion             |  | Li et al. (2012, Studies 1-3)<br><b>Study 8</b>  | Festjens et al. (2014, Study 2)                    |

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|                              |  |  |  |
|------------------------------|--|--|--|
| Temporal discounting         | Kim & Zauberma<br>(2013)<br>Van den Bergh et al.<br>(2008, Study 1A)<br>Wilson & Daly (2004) |  | Festjens et al. (2014,<br>Study 1)<br>Van den Bergh et al.<br>(2008, Study 1B) |
| Cooperation (ultimatum game) | Van den Bergh &<br>Dewitte (2006)  |  |  |

**Table 2**

Major differences (features present in the replication compared to the replicated experiment) between the studies reported in this article and the original studies on which they are modeled.

| Study | Replicated experiment(s)   | Major Differences  |
|-------|--|--|
| 1     | Prime induction: Griskevicius et al. (2007), Studies 2-4<br>Consumption measure: Griskevicius et al. (2007), Study 1 | Online sample<br>Consumption measure (monetary amount)   |
| 2     | Prime induction: Griskevicius et al. (2007), Studies 2-4<br>Consumption measure: Griskevicius et al. (2007), Study 1 | Online sample  |
| 3     | Griskevicius et al. (2007) Study 2   | Online sample  |
| 4     | Prime induction: Griskevicius et al. (2007) Study 1<br>Consumption measure: Griskevicius et al. (2007) Studies 2-4   | Online sample  |
| 5     | Griskevicius et al. (2007) Study 1, male participants<br>Greitemeyer et al. (2013) Exps. 3 & 4, male participants    | Online sample<br>Control stimuli (people)<br>Risk attitudes and consumption behaviour measured within-subjects                                 |
| 6     | Greitemeyer et al. (2013) Exps. 1 & 2  | Online sample<br>Risk attitudes measured within-subjects   |
| 7a    | Greitemeyer et al. (2013) Exp. 1, male participants  |  |
| 7b    | Greitemeyer et al. (2013) Exp. 2, male participants  | Online sample  |
| 8     | Li et al. (2012), Study 1  | Continuous willingness-to-pay scale<br>Loss aversion (gambling) test added<br>Gambling question added<br>Self-protection prime condition added |

**Table 3**

Sample size ( $N$ ) and descriptive statistics for the experimental (prime) and control groups in each study, and Bayes factors.

|                                    | $N$ | Prime $M$ ( $SD$ ) | $N$ | Control $M$ ( $SD$ ) | Difference [CI]     | Bayes factor $BF_{01}$ |
|------------------------------------|-----|--------------------|-----|----------------------|---------------------|------------------------|
| Study 1                            |     |                    |     |                      |                     |                        |
| Conspicuous consumption            |     |                    |     |                      |                     |                        |
| Male                               | 40  | 10.69 (8.83)       | 34  | 9.61 (5.21)          | 1.08 [-2.36, 4.52]  | 4.70                   |
| Female                             | 39  | 10.18 (4.20)       | 44  | 9.89 (4.60)          | 0.29 [-1.64, 2.22]  | 5.71                   |
| Study 2                            |     |                    |     |                      |                     |                        |
| Conspicuous consumption (10 items) |     |                    |     |                      |                     |                        |
| Male                               | 27  | 4.67 (1.37)        | 27  | 4.97 (1.91)          | -0.30 [-1.20, 0.61] | 4.05                   |
| Female                             | 32  | 4.57 (1.74)        | 39  | 4.18 (1.60)          | 0.38 [-0.41, 1.17]  | 3.60                   |
| Conspicuous consumption (4 items)  |     |                    |     |                      |                     |                        |
| Male                               | 27  | 5.25 (1.81)        | 27  | 5.56 (1.92)          | -0.31 [-1.32, 0.71] | 4.17                   |
| Female                             | 32  | 4.96 (1.89)        | 39  | 4.67 (1.93)          | 0.29 [-0.62, 1.20]  | 4.56                   |
| Study 3                            |     |                    |     |                      |                     |                        |
| Conspicuous consumption            |     |                    |     |                      |                     |                        |
| Male                               | 32  | 5.49 (1.39)        | 32  | 5.29 (1.10)          | 0.21 [-0.42, 0.83]  | 4.34                   |
| Female                             | 40  | 4.68 (1.27)        | 47  | 4.74 (1.08)          | -0.06 [-0.56, 0.44] | 5.91                   |
| Inconspicuous consumption          |     |                    |     |                      |                     |                        |
| Male                               | 32  | 4.74 (1.56)        | 32  | 4.72 (1.20)          | 0.03 [-0.67, 0.72]  | 5.28                   |
| Female                             | 40  | 4.61 (0.93)        | 47  | 4.65 (0.98)          | -0.04 [-0.45, 0.37] | 5.99                   |
| Conspicuous benevolence            |     |                    |     |                      |                     |                        |
| Male                               | 32  | 5.46 (1.59)        | 32  | 5.16 (1.56)          | 0.30 [-0.49, 1.09]  | 4.07                   |
| Female                             | 40  | 6.14 (1.39)        | 47  | 5.87 (1.33)          | 0.26 [-0.32, 0.85]  | 4.17                   |
| Inconspicuous benevolence          |     |                    |     |                      |                     |                        |
| Male                               | 32  | 5.00 (1.32)        | 32  | 5.12 (1.45)          | -0.12 [-0.81, 0.57] | 5.02                   |
| Female                             | 40  | 5.28 (1.06)        | 47  | 4.99 (1.25)          | 0.29 [-0.21, 0.79]  | 3.23                   |
| Study 4                            |     |                    |     |                      |                     |                        |
| Conspicuous consumption            |     |                    |     |                      |                     |                        |
| Male                               | 33  | 4.65 (1.40)        | 35  | 4.56 (1.19)          | 0.09 [-0.53, 0.72]  | 5.22                   |
| Female                             | 34  | 4.58 (1.13)        | 38  | 4.82 (1.10)          | -0.23 [-0.76, 0.29] | 3.88                   |

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|                           |     |               |     |               |                      |      |
|---------------------------|-----|---------------|-----|---------------|----------------------|------|
| Study 5                   |     |               |     |               |                      |      |
| Risk-taking (driving)     |     |               |     |               |                      |      |
| Male                      | 39  | 34.19 (15.81) | 40  | 37.19 (15.71) | -3.00 [-10.06, 4.06] | 4.19 |
| Risk-taking (social)      |     |               |     |               |                      |      |
| Male                      | 39  | 45.49 (12.71) | 40  | 43.50 (17.00) | 1.98 [-4.75, 8.72]   | 4.97 |
| Conspicuous consumption   |     |               |     |               |                      |      |
| Male                      | 39  | 4.56 (2.02)   | 40  | 4.51 (1.63)   | 0.06 [-0.76, 0.88]   | 5.77 |
| Inconspicuous consumption |     |               |     |               |                      |      |
| Male                      | 39  | 4.29 (1.24)   | 40  | 4.27 (1.12)   | 0.02 [-0.51, 0.55]   | 5.81 |
| Study 6                   |     |               |     |               |                      |      |
| Risk-taking (sexual)      |     |               |     |               |                      |      |
| Male                      | 25  | 2.73 (1.51)   | 31  | 3.25 (1.62)   | -0.53 [-1.37, 0.32]  | 2.48 |
| Female                    | 19  | 2.01 (1.71)   | 22  | 2.19 (1.54)   | -0.18 [-1.21, 0.85]  | 4.10 |
| Risk-taking (gambling)    |     |               |     |               |                      |      |
| Male                      | 25  | 1.16 (1.11)   | 31  | 1.45 (1.46)   | -0.29 [-1.0, 0.42]   | 3.65 |
| Female                    | 19  | 1.26 (1.33)   | 22  | 1.50 (1.34)   | -0.24 [-1.08, 0.61]  | 3.77 |
| Risk-taking (substance)   |     |               |     |               |                      |      |
| Male                      | 25  | 3.12 (2.25)   | 31  | 3.06 (2.36)   | 0.06 [-1.19, 1.31]   | 4.94 |
| Female                    | 19  | 2.72 (2.08)   | 22  | 2.85 (2.01)   | -0.13 [-1.42, 1.17]  | 4.26 |
| Study 7a                  |     |               |     |               |                      |      |
| Risk-taking (sexual)      |     |               |     |               |                      |      |
| Male                      | 61  | 3.91 (1.76)   | 59  | 3.89 (1.86)   | 0.02 [-0.63, 0.68]   | 7.06 |
| Study 7b                  |     |               |     |               |                      |      |
| Risk-taking (gambling)    |     |               |     |               |                      |      |
| Male                      | 53  | 0.76 (1.11)   | 53  | 0.83 (1.09)   | -0.08 [-0.50, 0.35]  | 6.30 |
| Study 8                   |     |               |     |               |                      |      |
| Loss aversion             |     |               |     |               |                      |      |
| Male                      | 109 | -3.92 (85.4)  | 109 | -4.19 (80.4)  | 0.27 [-21.9, 22.4]   | 9.41 |
| Female                    | 109 | -16.83 (75.9) | 108 | -16.21 (79.4) | -0.61 [-21.4, 20.2]  | 9.38 |

Note: CI = 95% confidence interval.

### Figure Captions

Figure 1. Forest plot from the meta-analysis showing the effect size (ES, Cohen's  $d$ ) and 95% confidence interval (CI) of each of 43 independent studies (from the reports listed in Table 1) and the meta-analytic effect size from a random-effects analysis. For each study the marker size is proportional to the sample size.

Figure 2. Funnel plot from the meta-analysis. Black circles represent the effect sizes (Cohen's  $d$ ) of each of the 43 independent studies from the reports listed in Table 1 plotted against the inverse of that study's standard error (SE). The rightmost vertical line is the effect size estimate from a random-effects model of these 43 studies, and the red line is the regression line from the Egger test. Open triangles denote the effect sizes and SEs of the new studies reported in the present article. The shaded gray area depicts the region in which  $p > .05$  for individual studies. The darker portion of this area depicts the region of marginal significance,  $0.10 > p > .05$ .

Figure 3. Forest plot for the main experimental tests in Studies 1-8, showing the effect size (ES, Cohen's  $d$ ) and 95% confidence interval (CI) of each as well as the meta-analytic effect size from a random-effects analysis. Except where otherwise noted, the tests are all for male participants. For each study the marker size is proportional to the sample size.

Figure 1

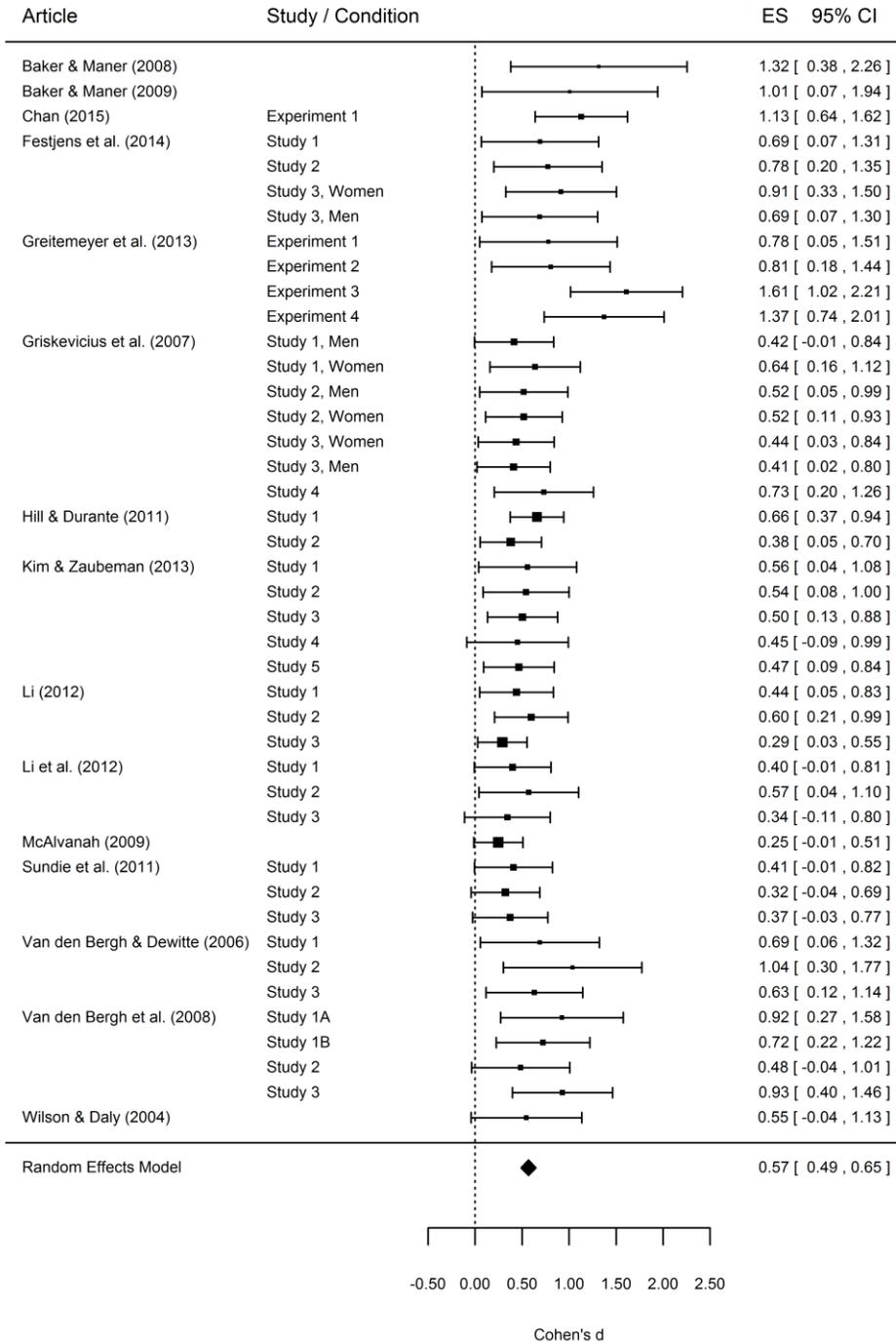


Figure 2

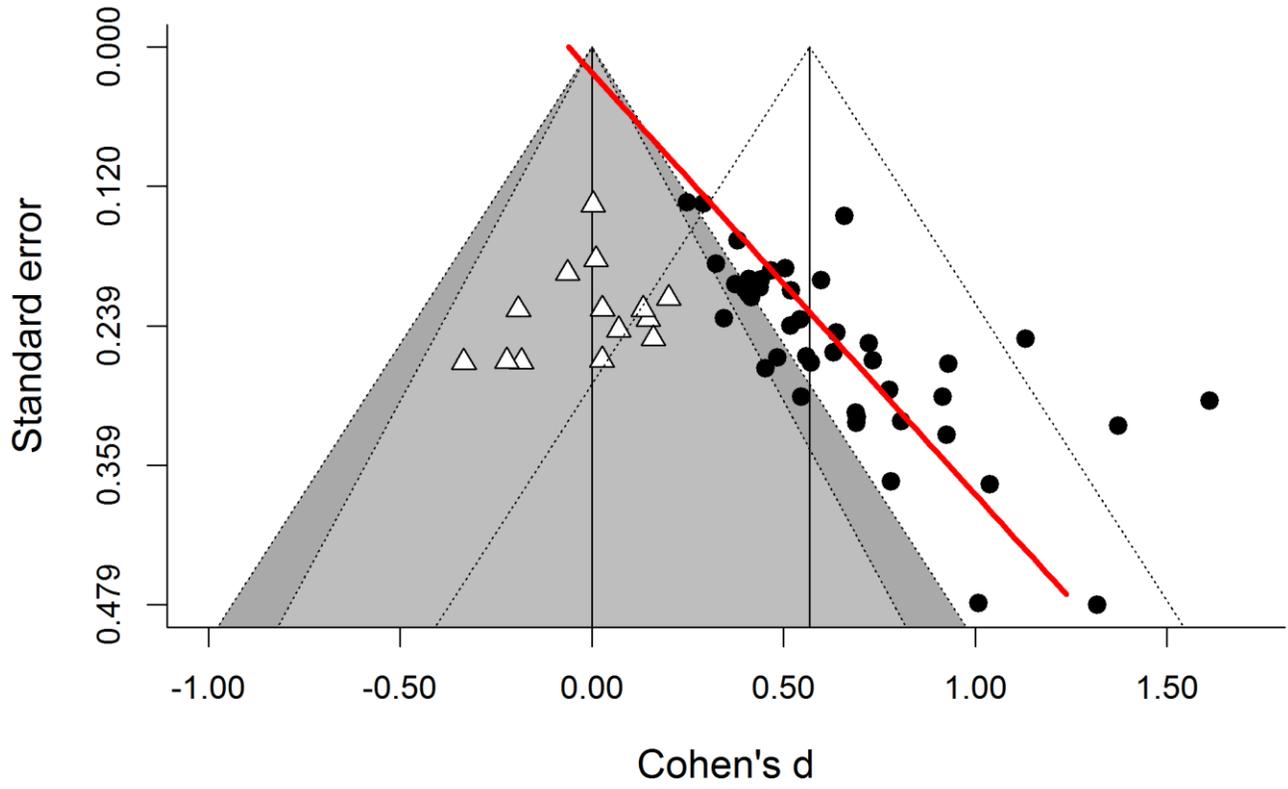
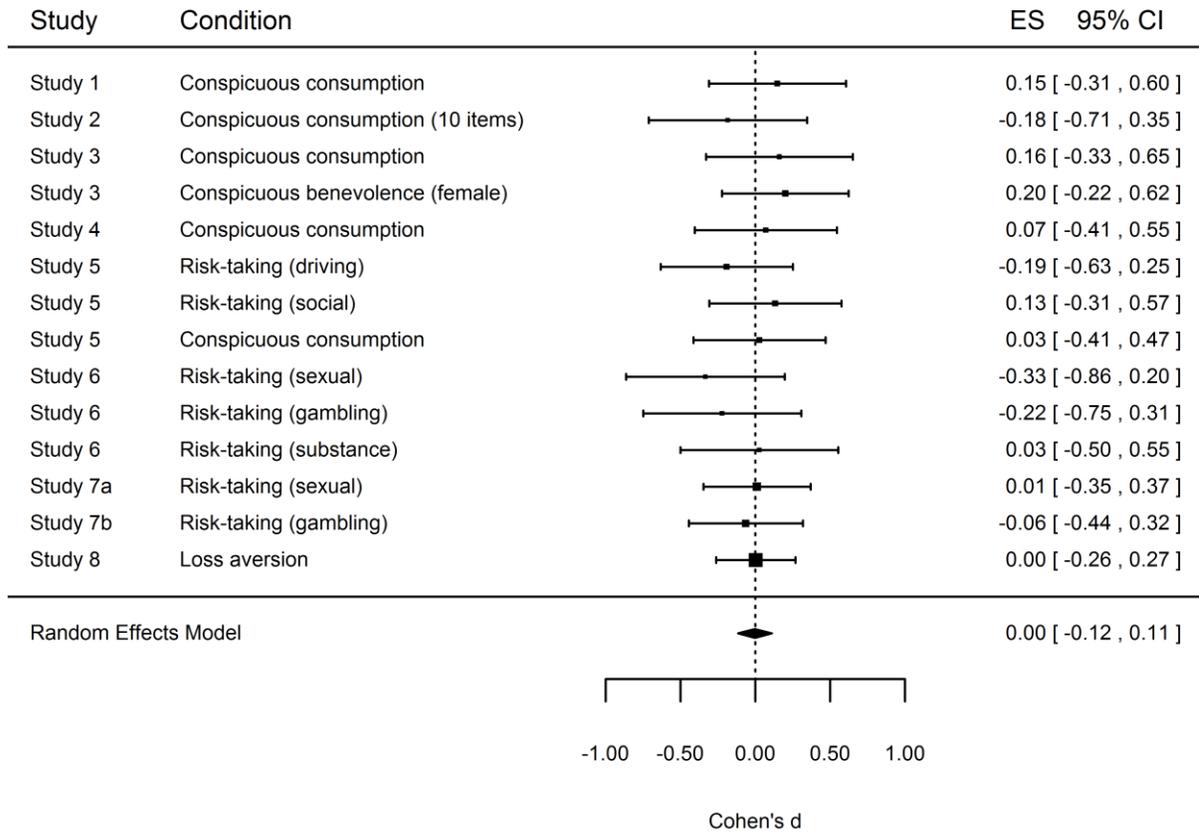


Figure 3



**Supplemental Material**

Romance, Risk, and Replication:

Can Consumer Choices and Risk-Taking be Primed by Mating Motives?

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In all experiments participants gave demographic and other information including their relationship status (e.g., single, in a relationship, married) and also reported their awareness of the purpose of the experiment via a series of funnel debriefing questions. These began very generally (e.g., “what do you think the survey you just completed was designed to investigate?”) with participants answering in their own words, but eventually became highly specific (e.g., “Do you think the text at the beginning of the survey had an effect on the maximum prices you said you would be willing to pay for the various goods and services?”) and answered via a yes/no response. Even by the most specific question only a minority of participants reported believing that the text affected their responses. Of these only some gave a justification indicative of awareness of the experimental hypothesis (e.g., “feelings of contentment and excitement may compel one to spend higher than normal on certain items”) and such reports were given by participants in both priming and control groups. Importantly, for all experiments the overall pattern of results is unaffected by removal of such participants from the analyses.

To confirm that the priming materials activate priming motives and intentions, we carried out a manipulation check on 106 additional male participants, tested online. After reading the romantic prime text, participants rated themselves on a scale from 0-100 as more a strongly desiring a romantic partner ( $M = 74.3$ ,  $SD = 24.8$ ) than ones who read the control text ( $M = 62.6$ ,  $SD = 31.2$ ),  $t(104) = 2.13$ ,  $p = .018$  (1-tailed). This is consistent with several of the published studies (Baker & Maner, 2008, 2009; Griskevicius et al., 2007; Hill & Durante, 2011) which have similarly demonstrated an effect of the prime materials (both text and pictures) on mating intentions.

*Study 3 results*

A 2 (Gender) x 2 (Priming Condition: Prime vs. Control) x 2 (Domain: Consumption vs. Benevolence) x 2 (Conspicuousness: Conspicuous vs Inconspicuous) mixed ANOVA was conducted. This yielded a main effect of domain,  $F(1, 147) = 18.85, p < .001, \eta^2_p = .114$ , indicating that participants rated themselves as more willing than the average person to pay for acts of benevolence ( $M = 5.38$ ) but less willing than the average person to pay for consumption items ( $M = 4.87$ ), which was moderated by a significant domain x gender interaction,  $F(1, 147) = 10.79, p = .001, \eta^2_p = .068$ , reflecting the fact that the effect of domain was gender-specific: while females were much more willing to pay for benevolence ( $M = 5.57$ ) than for consumption ( $M = 4.67$ ), males were indifferent between benevolence ( $M = 5.19$ ) and consumption ( $M = 5.06$ ). There was a main effect of conspicuousness,  $F(1, 147) = 33.54, p < .001, \eta^2_p = .186$ , indicating that individuals rated themselves as relatively more likely to engage in conspicuous ( $M = 5.35$ ) compared to inconspicuous ( $M = 4.89$ ) activities.

Lastly there was a highly significant domain x conspicuousness x gender interaction,  $F(1, 147) = 13.12, p < .001, \eta^2_p = .082$ . This reflects the fact that while men were more likely to engage in conspicuous ( $M = 5.39$ ) compared to inconspicuous *consumption* ( $M = 4.73$ ), females were indifferent (conspicuous  $M = 4.71$ ; inconspicuous  $M = 4.63$ ); in contrast females were more likely to engage in conspicuous ( $M = 6.00$ ) compared to inconspicuous *benevolence* ( $M = 5.14$ ), whereas males were indifferent (conspicuous  $M = 5.31$ ; inconspicuous  $M = 5.06$ ). However crucially the 4-way domain x conspicuousness x prime x gender interaction was nonsignificant,  $F(1, 147) = 0.14, p = .71, \eta^2_p = .001$ , as was the main effect of prime condition,  $F(1, 147) = 0.69, p = .41, \eta^2_p = .005$ , and all other interactions involving condition,  $F < 1.04$  in each case.

*Study 8 results*

On the primary (i.e., Li et al.) loss aversion measure, the observed degree of loss aversion was slightly greater than in Li et al.'s (2012, Study 1) experiment, which was approximately \$1.50 (estimated from their Figure 1). An analysis of variance with gender, testing format (laboratory or online), and priming condition (control, mating prime, self-protection prime) as between-subjects factors yielded a main effect of format,  $F(1, 638) = 7.07, p = .008, \eta^2_p = .011$ . Participants tested online,  $M = -12.1, 95\% \text{ CI } [-18.6, -5.7]$ , were significantly loss averse whereas those tested in the laboratory,  $M = 15.2, 95\% \text{ CI } [-1.0, 31.4]$ , were not. No other effects or interactions were significant, including the important gender x priming condition interaction,  $F(2, 638) = 1.85, p = .16, \eta^2_p = .006$ . Descriptive statistics are reported in Table S1.

For the second loss aversion test (based on Tom et al., 2007), we calculated the number of gambles accepted by each participant. Despite the fact that the wins were larger on average than the losses and that 10 of the gambles displayed greater gains than losses, participants opted to play significantly fewer than half the gambles,  $M = 9.19, 95\% \text{ CI } [8.90, 9.47]$ , indicating loss aversion (participants were on average indifferent between accepting and rejecting a gamble when the gain/loss ratio was 1.34). An analysis of variance identical to the one above yielded no significant main effects or interactions, including no main effect of testing format,  $F(1, 638) = 0.08, p = .78, \eta^2_p = .00$ , and no gender x priming condition interaction,  $F(2, 638) = 1.49, p = .23, \eta^2_p = .005$ . Descriptive statistics are reported in Table S1.

For the gambling question, a large majority of participants chose the less risky option. A slightly (but not significantly) smaller proportion of males chose the risky option in the mating prime (5/109) versus the control condition (8/109).

**Table S1**

Sample size ( $N$ ) and descriptive statistics for the prime (mating and self-protection) and control groups in Study 8 for the two loss aversion measures.

|                                  | $N$ | Mating $M$ ( $SD$ ) | $N$ | Self-protection<br>$M$ ( $SD$ ) | $N$ | Control $M$<br>( $SD$ ) |
|----------------------------------|-----|---------------------|-----|---------------------------------|-----|-------------------------|
| Li et al. loss aversion measure  |     |                     |     |                                 |     |                         |
| Male                             |     |                     |     |                                 |     |                         |
| online                           | 97  | -2.41 (87.21)       | 96  | -5.50 (85.87)                   | 96  | -13.44 (77.51)          |
| laboratory                       | 12  | -16.11 (70.92)      | 11  | -9.87 (38.47)                   | 13  | 64.07 (69.76)           |
| Female                           |     |                     |     |                                 |     |                         |
| online                           | 97  | -22.16 (75.34)      | 97  | -10.14 (66.91)                  | 97  | -19.12 (79.31)          |
| laboratory                       | 12  | 26.28 (68.71)       | 11  | 10.41 (49.75)                   | 11  | 9.44 (78.82)            |
| Tom et al. loss aversion measure |     |                     |     |                                 |     |                         |
| Male                             |     |                     |     |                                 |     |                         |
| online                           | 97  | 9.43 (3.53)         | 96  | 9.73 (4.03)                     | 96  | 9.08 (3.43)             |
| laboratory                       | 12  | 10.17 (5.67)        | 11  | 8.91 (2.77)                     | 13  | 8.85 (1.72)             |
| Female                           |     |                     |     |                                 |     |                         |
| online                           | 97  | 9.37 (3.64)         | 97  | 8.62 (3.73)                     | 97  | 8.80 (3.48)             |
| laboratory                       | 12  | 8.83 (3.93)         | 11  | 8.18 (4.79)                     | 11  | 10.91 (4.99)            |