

# The Hidden Effects of Recalling Secrets: Assimilation, Contrast, and the Burdens of Secrecy

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Three high-power studies ( $N = 3,000$  total) demonstrated that asking participants to recall an experience as a manipulation can have unintended consequences. Participants who recalled preoccupying secrets made more extreme judgments of an external environment, supporting the notion that secrecy is burdensome. This influence was found, however, only among a subset of participants (i.e., participants who successfully recalled secrets that corresponded to their condition). We introduce the concept of manipulation correspondence to understand these patterns of results. Without taking into account whether participants' recalled secrets corresponded to their manipulation, there was no main effect of the recall manipulation on hill slant judgments. Among participants whose secrets did not correspond with the manipulation, a contrast effect emerged (i.e., influences on perceptual judgments opposite to the intention of the recall prompts). Moreover, the very process of recalling a secret in response to a prompt can lead to contrast from that prompt. Exposing participants to extreme exemplar secrets can experimentally produce, or counteract, this contrast effect. Preoccupying secrets are burdensome but tests of this phenomenon must take into account whether participants are actually preoccupied with their secrets (i.e., whether their recalled secrets correspond with the experimental manipulation), or experimentally ensure that participants judge their secrets as in line with the manipulation. More broadly, the current research speaks to a fundamental principle of recall manipulations; when recalling a particular experience, correspondence with the manipulation will determine its effects, and the process of recalling an experience (and comparing it to a prompt) might change how one perceives that experience.

*Keywords:* secrecy, assimilation and contrast, perceptual judgments, replication

Secrets are a ubiquitous feature of social life. People keep secrets from friends, colleagues, family members, and significant others. Such concealment is associated with a wide range of negative outcomes, including anxiety, depression, and physical health complaints (Kelly & Yip, 2006; Larson & Chastain, 1990). Recent work suggests that some of the negative consequences of secrets may stem from secrets being experienced as psychologically burdensome (Slepian, Masicampo, Toosi, & Ambady, 2012). The burden of secrets can lead individuals to feel their resources are compromised, making the environment seem more forbidding and further exertions of effort seem more onerous. The apparent burdens of secrecy, however, are not yet well understood. This is due partly to failures to replicate an influence on perceptual judgments (LeBel & Wilbur, 2013; Pecher, van Mierlo, Cañal-Bruland, & Zeelenberg, 2015), which may stem from confusion about the precise mechanism by which secrets are experienced as burdensome (see Slepian, Camp, & Masicampo, 2015). The aim of

the present studies is to address these issues, providing a refined understanding of the burdens of secrecy, and providing new insights into how thinking about secrets can influence perceptual judgments.

Beyond the current domain of secrecy, the current work offers new insights into (a) the dynamics of recalling a personal experience as the source of a manipulation and (b) executing and evaluating replication attempts, more generally. We discuss an often-unappreciated feature of statistical power; within-group variability decreases statistical power. Specifically, asking participants to recall an experience as a manipulation (of secrecy, power, creativity, morality, etc.) should produce wide variability within a single condition—relative to presenting participants with a standardized stimulus—given the diversity of experiences participants have had and that they draw from as a source for the manipulation. The content of those recalled experiences will determine their influence on the outcome of interest. This variability in the content of recalled experiences presents challenges for using a recall task as a manipulation. An additional challenge with using recall tasks arises from the process by which participants recall their experiences. As participants search their memory for experiences that conform to a study prompt, participants will compare those experiences to the recall prompt. This can unintentionally lead participants to realize how their experience differs from the recall prompt, promoting contrast away from (rather than assimilation to)

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the prompt. We demonstrate this in the domain of secrecy but discuss implications for psychological effects more broadly.

The present work serves as a high-powered replication of past work, specifically testing the idea that highly preoccupying secrets are experienced as burdensome. The present experiments tested whether some participants fail to recall preoccupying secrets when instructed to do so, thereby masking the burdening effects of secrecy. We also explored whether this lack of correspondence to one's manipulated condition (a) promotes a contrast effect and (b) if this contrast effect can be counteracted. The results hold implications not only for the psychology of secrets, but for any study that uses a recall manipulation, and as a result, they speak more broadly to the replication of psychological effects. We discuss how prompts that ask participants to recall an experience as a manipulation can unintentionally change how participants view those experiences in a manner that opposes the intent of the original prompt.

### The Burdening Effect of Preoccupying Secrets

When people feel that they have diminished resources (e.g., cognitive, physiological, motivational), they feel that more effort is required to interact with the external environment (Cole & Balcettis, 2013; Eves, 2014; Eves, Thorpe, Lewis, & Taylor-Covill, 2014; Gross & Proffitt, 2014; Proffitt, 2006; Witt, Proffitt, & Epstein, 2004; Witt, 2011). Perceiving the external environment as requiring additional effort to interact with, then, makes that environment seem more challenging, which leads to the judgment that the environment is forbidding. For example, when wearing a heavy and burdensome backpack, one has fewer perceived resources to scale a hill. This makes the hill seem more challenging to scale, and therefore more steep (Proffitt, 2006).<sup>1</sup>

Secrets may have a similar burdening effect. By becoming preoccupied with a secret, people are devoting resources toward that secret, which leads the secret keeper to feel that he or she has fewer available resources for other pursuits (Slepian et al., 2015) and leads those pursuits to seem more challenging and therefore forbidding. As a consequence, Slepian and colleagues (2015, Studies 3 and 4) found that randomly assigning participants to recall secrets with which they were preoccupied (vs. those with which they were not preoccupied) led them to feel that more effort was required to keep their secret, which in turn predicted judging an external environment as more challenging and forbidding. Overall, recent work has revealed the level of preoccupation one has with a secret as a main determinant of whether holding that secret will be burdensome (Slepian et al., 2012, Study 3; Slepian et al., 2015).

Although multiple studies across multiple papers have found that secrets are burdensome (Goncalo, Vincent, & Krause, 2015; Slepian et al., 2012, 2015), other recent findings have questioned the reliability and robustness of these effects (LeBel & Wilbur, 2014; Pecher et al., 2015). It is important to distinguish conceptual hypotheses from results generated from specific methods in making conceptual claims. That is, without clear construct validity, a failure to replicate any phenomenon is difficult to interpret beyond that a specific independent variable did not influence a specific dependent variable (i.e., construct validity is needed to interpret the meaning behind those variables and their relationship). One possibility is that secrecy is indeed burdensome (consistent with what people say it is like to keep a secret; Slepian et al., 2012) but

that not all manipulations of secrecy are created equal. One reason that some studies have failed to find support for a burdening effect of secrets is that prior work has not focused on preoccupation. Indeed, prior failed replications have relied on a manipulation that asks participants to recall "big" versus "small" secrets, with the prediction that participants recalling "big" secrets will be more burdened than those recalling "small" secrets (LeBel & Wilbur, 2014; Pecher et al., 2015).

Asking participants to recall "big" versus "small" secrets might lead participants to recall secrets that are normatively treated as "big" versus "small" (e.g., infidelity vs. a white lie) but may not consistently lead participants to recall secrets with which they are personally preoccupied. Indeed, randomly assigning participants to recall "big" secrets does not seem to consistently lead participants to recall secrets that are more personally preoccupying than the secrets recalled by participants asked to think of "small" secrets (Slepian et al., 2015). Moreover, having a "big" secret like infidelity predicts burden-consistent outcomes only to the extent one is preoccupied with that secret (Slepian et al., 2012, Study 3). This may explain why a manipulation of "big" versus "small" secrets does not consistently have downstream consequences. For example, Goncalo, Vincent, and Krause (2015) and Slepian and colleagues (2012) found an influence of the "big" versus "small" manipulation, whereas LeBel and Wilbur (2014), Pecher and colleagues (2015), and Slepian and colleagues (2015) did not. In contrast, directly manipulating the recall of preoccupying versus nonpreoccupying secrets led participants recalling preoccupying secrets to exhibit burden-consistent effects (Slepian et al., 2015).

We suggest that replication failures of the burdening effects of secrecy have occurred, in part, due to an overreliance on an imprecise manipulation, the recall of "big" versus "small" secrets. That is, recent work (Slepian et al., 2015) reveals that it is how preoccupied one is with a secret, not just how conventionally "big" or "small it seems, that makes a secret burdensome. In the present work, we used three high-powered studies to test whether preoccupying secrets produce burden-consistent effects.

We also introduce a novel methodological consideration: manipulation correspondence. We suspect that some participants may recall secrets that do not correspond to the types of secrets that their experimental manipulation asks them to recall. This lack of manipulation correspondence may then mask any link between preoccupying secrets and burden-consistent outcomes.

<sup>1</sup> We refer here specifically to perceptual judgments and do not make claims in the current work about visual perception. There is ongoing debate about whether influences on judged hill slant are judgment-based or visually based (cf. Firestone, 2013; Proffitt, 2013), but the current work does not make strong claims that vision itself is influenced. There is also a debate about whether wearing a heavy backpack influences judgments of hill slant through reductions in perceived resources, or through demand effects (Durgin et al., 2009; Proffitt, 2006). This debate is orthogonal to the "judgment-versus-vision debate," but it is not relevant to the current work, as the manipulations used herein do not include a backpack or any interaction with experimenters, and in no study did participants guess the experimental hypotheses during debriefing. Many influences upon judgments of physical space, other than the debated backpack manipulation and demand-based explanations, have been found (Bhalla & Proffitt, 1999; Cole & Balcettis, 2013; Eves, 2014; Sugovic & Witt, 2013; Witt et al., 2009; Witt, Proffitt, & Epstein, 2004; but see Durgin, Klein, Spiegel, Strawser, & Williams, 2012).

## Manipulation Correspondence

We propose that participants do not always recall secrets that correspond with their experimental instructions. When participants are asked to recall a preoccupying versus nonpreoccupying secret, the assumption is that participants will indeed recall preoccupying and nonpreoccupying secrets. Crucially, if people do not recall secrets that correspond with the manipulation, then the effects of that manipulation will be difficult to observe.

In the present work, we tested whether the influence of secret recall hinges on manipulation correspondence. We expected that the effect of recalling preoccupying versus nonpreoccupying secrets would be moderated by manipulation correspondence. The issue regarding recalling secrets that do not correspond with the manipulation is even more important to consider because participants who do not recall a secret that corresponds to their manipulation may show the reverse pattern. We propose that recalling secrets that do not correspond to the manipulation can produce a contrast effect for two reasons.

First, a large literature suggests that the ease with which information is retrieved determines its effects on judgments (e.g., Schwarz, 1998; Schwarz, Bless, Strack, Klumpp, Rittenauer-Schatka, & Simons, 1991; Schwarz & Clore, 1996; Tversky & Kahneman, 1973; Wänke, Schwarz, & Bless, 1995; Winkielman, Schwarz, & Belli, 1998). For example, Schwarz and colleagues (1991) asked participants to recall either 6 or 12 examples of assertiveness. They found that participants experienced greater difficulty in recalling 12 examples than recalling 6 examples. As a result of this increased difficulty in recalling these experiences, participants in the 12 examples condition concluded that they lacked assertiveness. Similarly, Lammers et al. (2015) found that ease of retrieval moderated the effects of a power recall manipulation. When the experience of power was difficult to retrieve, participants actually felt less powerful and showed reverse effects than the typical influence of recalling power experiences increasing unethical behavior and decreasing conformity.

Second, it is well known that when individuals compare their mental contents with extreme exemplars, they contrast away from those exemplars (Bless & Schwarz, 2010; Herr, Sherman, & Fazio, 1983; Moskowitz & Skurnick, 1999; Strack & Mussweiler, 1997). Applied to secret recall manipulations, we suspected that for participants who are asked to recall a preoccupying secret but instead recall a nonpreoccupying secret, then that secret would seem even more nonpreoccupying than if they were assigned to the nonpreoccupying condition. Similarly, we suspected that participants in the nonpreoccupying condition who recall a preoccupying secret would find those secrets to be more preoccupying than if they had been assigned to the preoccupying condition.

These counterintuitive effects of retrieval and contrasting away from exemplars suggests that participants in the preoccupying secrets condition would exhibit *less* burden-consistent outcomes (e.g., less steep judgments of hill slant) when they have difficulty recalling their assigned secret and instead recall a secret that does not correspond to the manipulation. To be clear, we expected that (a) participants in the preoccupying secrets condition would recall secrets that are more preoccupying on average than those in the nonpreoccupying secrets condition, and (b) recalling preoccupying secrets would increase hill slant judgments. However, we also expected that (c) when participants' recalled secrets that did not

correspond to their manipulation, recalling preoccupying secrets would lead to less steep hill slant judgments, due to our proposed contrast effect.

To test his idea, we controlled for the preoccupying nature of participants' secrets. To illustrate why this matters, consider Participant A, who in the preoccupying condition, recalled a secret at the midpoint of our preoccupation measure (i.e., a 4 out of 7), and Participant B who also recalled a secret at the midpoint of the preoccupation measure but was in the nonpreoccupying condition. Although both participants recalled an equally preoccupying secret (i.e., preoccupation is held constant), they were not asked to recall equally preoccupying secrets. Thus, Participant A has recalled a relatively less preoccupying secret than requested, and Participant B has recalled a relatively more preoccupying secret than requested. This would lead Participant A, who is in the preoccupying condition, to thereby feel *less* burdened, and Participant B, who is in the nonpreoccupying condition, to feel correspondingly *more* burdened. As a result, when accounting for self-reported preoccupation, we would predict that Participant A (preoccupation condition) would judge a hill as less steep than Participant B (nonpreoccupation condition). Thus, low manipulation correspondence could contribute to a contrast effect.

Finally, we predict that this kind of contrast effect can be experimentally produced or eliminated. In a final study, we introduced a second manipulation that involved exposing participants to either an extremely preoccupying or extremely nonpreoccupying secret. If we exposed Participant A (in the preoccupying condition) to another person's secret that is highly nonpreoccupying, this should lead the participant to feel that their secret is indeed relatively preoccupying. Likewise, if we exposed Participant B (in the nonpreoccupying condition) to another person's secret that is highly preoccupying, this should reanchor the participant to feel that their secret is indeed relatively nonpreoccupying.

## The Current Work

The current studies on the burdens of secrecy had three main aims. The first was to address the issue of replicability with a high-powered, direct replication of recent work. Specifically, we conducted three high-power studies ( $N = 1,000$  per study) employing a manipulation of preoccupation found to influence hill slant judgments in recent work (Slepian et al., 2015). The main dependent measure was judgments of hill slant, with the prediction that preoccupation with secrets (i.e., feeling that one's resources are compromised by one's secret) would lead other pursuits to seem more challenging (e.g., a hill is more forbidding).

The second aim of this work was to account for manipulation correspondence with the secrecy recall instructions. We measured manipulation correspondence by asking participants to report how preoccupying his or her recalled secret was. We predicted that manipulation correspondence would be a moderator such that burden-consistent effects would only occur at high levels of manipulation correspondence. Further, we predicted that the effect might even reverse at low levels of manipulation correspondence. Thus, by measuring manipulation correspondence, we can test whether what may appear to be a null effect of the secret recall manipulation is actually an effect of the secret recall manipulation moderated by manipulation correspondence.

The third aim of this work was to test whether a contextual manipulation could reduce this hypothesized contrast effect. In a final study, we included a second manipulation that exposed participants to either an extremely preoccupying or extremely nonpreoccupying secret. In one condition, after participants recalled their secret, we introduced an exemplar that extremely opposed participants' assigned prompts (e.g., after participants recalled a preoccupying secret, they were exposed to a highly nonpreoccupying secret). For participants in this condition, the nonpreoccupying exemplar should make their own secret seem to be relatively more preoccupying and thus tightly tether participants to the manipulation prompt (i.e., increase the perceived correspondence between the participant's recall and the intent of the recall prompt). In another condition, we exposed participants to secrets that were extreme exemplars in the direction of their assigned prompt. We predict that a secret even more extreme than their condition (e.g., showing participants an extremely preoccupying secret after they recalled their own preoccupying secret) will cause participants to see their own secrets as relatively less preoccupying, thereby promoting contrast from the intention of the original recall prompt. Our aim here is to directly produce or eliminate our hypothesized contrast effect, while at the same time testing a methodological intervention for increasing the internal validity of a recall manipulation.

Across three studies, we randomly assigned participants to recall either preoccupying or nonpreoccupying secrets. In Studies 1 and 2, we then measured self-reported preoccupation with recalled secrets. This measure served as a manipulation check that the secrecy recall manipulation had an overall effect on how preoccupying participants' secrets were. It also served as our measure of manipulation correspondence, with higher (or lower) preoccupation indicating greater correspondence with instructions to recall preoccupying (or nonpreoccupying) secrets. Last, we measured judgments of the steepness of a pictured hill. Given the high similarity between the first two studies, we report them together, reporting the analyses per study, grouped by analysis strategy. To be clear, Study 1 was an exploratory study. In that study we found that only among participants who recalled secrets that corresponded with their experimental manipulation, did recalling preoccupying (vs. nonpreoccupying) secrets increase judgments of hill slant. Study 2 was a confirmatory study that offered an exact replication of Study 1.

In Study 3, after participants were randomly assigned to recall preoccupying or nonpreoccupying secrets, we then randomly exposed them to either highly preoccupying or highly nonpreoccupying secrets (a  $2 \times 2$  design). We expected that when the exemplar was more extreme than the prompt, we would get a contrast effect (e.g., after recalling a preoccupying secret, exposure to an extremely preoccupying secret would lead participants to see their own secrets as less preoccupying). However, when the exemplar was less extreme than the prompt, we expected this to reinforce the original intent of the prompt (e.g., after recalling a preoccupying secret, exposure to an extremely nonpreoccupying secret would lead participants to see their own secrets as more preoccupying). Thus, our manipulation either bolstered the original manipulation or undermined it.

In the current work we report all studies conducted (i.e., we only conducted the current three highly powered studies,  $N = 1,000$  each), all measures taken, and all data exclusions. Additionally, we implemented a JavaScript code in the current studies that pre-

vented participants from both participating in multiple studies in the current work, and also from participating if they previously participated in a study on secrecy previously conducted by the authors.

## Study 1

### Participants and Design

Adopting methodology from Slepian and colleagues (2015), 1,000 participants (539 male, 459 female, 2 unreported;  $M_{\text{age}} = 31.62$  years,  $SD = 11.98$ ) were recruited on Mechanical Turk for a study ostensibly about the workplace. Participants were randomly assigned to recall either a preoccupying or nonpreoccupying secret. The sample size of 1,000 was chosen because we considered any effect that could not be uncovered with this sample size to be too small to be meaningful (with 80% power, this sample size can detect a Cohen's  $d = .1775$ , equivalent to an  $r$  effect size =  $.0884$  at  $\alpha = .05$ ; see Fritz, Morris, & Richler, 2012). We did not anticipate the effect size to be this small, however, as we also measured manipulation correspondence, which also increases statistical power to the extent that there is indeed some correspondence to begin with (e.g., Hansen & Collins, 1994).

Data exclusions were decided ahead of time (using the same exclusion criteria as in Slepian et al., 2015). Forty-two participants (4.2%) stated that they did not have a secret to recall ( $n = 22$  preoccupying;  $n = 20$  nonpreoccupying), and thus these participants were excluded from analysis. Additionally, 9 (0.9%) participants provided a hill slant judgment other than a number between 1 and 89 (e.g., "90" or "steep"), and thus these participants were also excluded from analysis.

### Procedure

**Secret recall manipulation.** Participants read, "Before we ask you to rate objects and places, we are also interested in the psychology of secrets." On the next line, they read, "We ask you to think about a secret that you have, one that you are purposefully keeping as a secret." In the preoccupying secret condition, they were asked to make sure the secret fits all three of the following qualifications: (a) "You think about it reasonably often," (b) "It really affects you," and (c) "It really bothers you." In the nonpreoccupied condition, the criteria were (a) "You almost never think about it," (b) "It doesn't really affect you," and (c) "You feel okay about it."

**Manipulation correspondence measure.** On a subsequent page, a measure of correspondence with the manipulation was taken. Participants were asked, "How much do you think about your secret?", "How much does it affect you?", and "How much does it bother you?" (ratings ranged from 1 [*not at all*] to 7 [*very much*];  $\alpha = .90$ ).

**Control numerical judgments.** Next, on subsequent page, participants judged a series of control items: (a) the sturdiness of a table (ratings ranged from 1 [*not at all sturdy*] to 7 [*very sturdy*]), (b) the durability of a water bottle (ratings ranged from 1 [*not at all durable*] to 7 [*very durable*]), and (c) the temperature in degrees Fahrenheit of a pictured outdoor park. These items were z-scored,

and an average was taken as an index of control numerical estimation.<sup>2</sup>

**Hill slant judgment.** Last, participants judged the slant of a pictured grassy hill. Participants were reminded that 0 degrees is a flat surface and 90 degrees is a vertical surface, and thus their estimation should be in between those two numbers. As described previously, participants whose responses did not fall in this range of values were excluded from analysis (as in Slepian et al., 2015).

## Study 2

Study 2 was an exact replication of Study 1, but with one modification. Again, 1,000 participants (532 male, 466 female, 2 unreported;  $M_{\text{age}} = 31.28$  years,  $SD = 10.56$ ) were recruited on Mechanical Turk for a study ostensibly about the workplace. Perhaps the placement of the manipulation correspondence measure in Study 1 (between the independent variable and the dependent variable) altered the relationship between the two variables. To test for this possibility, we randomly assigned participants in Study 2 to complete the manipulation correspondence measure ( $\alpha = .91$ ) between the secret recall manipulation and the dependent measure, or after the dependent measure.

Twenty-six participants (2.6%) stated that they did not have a secret to recall ( $n = 16$  preoccupying;  $n = 10$  nonpreoccupying), and thus these participants were excluded from analysis. Additionally, 8 (0.8%) participants provided a hill slant judgment other than a number between 1 and 89 (e.g., “90” or “steep”), and thus these participants were also excluded from analysis.

## Results

We first tested for a direct link between preoccupation with secrets and hill slant judgments. This involved testing whether the secret recall manipulation influenced hill slant judgments as well as whether self-reported preoccupation with secrets influenced hill slant judgments. We then tested the hypothesized effects that were due to variance in correspondence with the manipulation: (a) moderation of the burdening effects of secrets by manipulation correspondence and (b) a contrast effect that was due to low manipulation correspondence.

### Direct Effects of Preoccupation

#### Study 1.

**Manipulation check.** We first examined whether the preoccupation manipulation produced the predicted effect on preoccupation. This was the case; participants asked to recall preoccupying secrets ( $M = 5.11$ ,  $SD = 1.36$ ) recalled secrets with which they were more preoccupied as compared with participants asked to recall nonpreoccupying secrets ( $M = 2.34$ ,  $SD = 1.25$ ; 95% CI on the difference = [2.60, 2.93];  $t(948) = 32.56$ ,  $p < .0001$ ,  $r = .73$ ).

**Hill slant judgment.** We next conducted a  $t$ -test comparing hill slant judgments across the two conditions. Those recalling preoccupying secrets ( $M = 40.93$ ,  $SD = 16.22$ ) did not make steeper judgments of hill slant than did those recalling nonpreoccupying secrets ( $M = 40.26$ ,  $SD = 16.60$ ; 95% CI on the difference = [-1.42, 2.76],  $t(948) = 0.63$ ,  $p = .53$ ,  $r = .02$ ).

Next, we examined whether preoccupation with secrets predicted judgments of hill slant. Indeed, the more preoccupied par-

ticipants were with their secrets, the steeper they judged the hill ( $b = .80$ ,  $SE = 0.28$ ; 95% CI on  $b = [.26, 1.35]$ ,  $t(948) = 2.88$ ,  $p = .004$ ).

**Control judgments.** Finally, we conducted a  $t$ -test comparing control judgments across the two conditions. There was also no difference in control judgments ( $M_{\text{preoccupying}} = .005$ ,  $SD = .64$ ;  $M_{\text{nonpreoccupying}} = -.007$ ,  $SD = .64$ ; 95% CI on the difference [-.07, .09],  $t(948) = 0.29$ ,  $p = .77$ ,  $r = .01$ ). There was also no relationship between how preoccupied participants were with their secrets and their control judgments ( $b = .003$ ,  $SE = 0.01$ , 95% CI = [-.02, .02],  $t(948) = 0.29$ ,  $p = .77$ ).

#### Study 2.

**Manipulation check.** The same tests were conducted as in Study 1, but with the inclusion of whether self-reported preoccupation (i.e., the manipulation correspondence measure) was measured between the independent variable and the dependent variable as a predictor (coded as 0) or whether it was measured after the dependent variable (coded as 1). To parallel analyses for Study 1, an analysis of variance (ANOVA) tested whether preoccupation with recalled secrets differed across secret recall conditions (including manipulation correspondence measurement timing as a covariate). There was an effect of secret recall,  $F(1, 964) = 1238.61$ ,  $p < .0001$ ,  $\eta^2 = .56$ , whereby participants asked to recall preoccupying secrets ( $M = 5.19$ ,  $SD = 1.33$ ) recalled secrets with which they were more preoccupied compared with participants who were asked to recall nonpreoccupying secrets ( $M = 2.28$ ,  $SD = 1.24$ ; 95% CI on the difference [2.75, 3.08]). There was no effect of manipulation correspondence measurement timing ( $F = 0.09$ ,  $p = .76$ ).

**Hill slant judgment.** To parallel analyses for Study 1, an ANOVA tested whether there was an effect of the secret recall manipulation on hill slant judgments (including manipulation correspondence measurement timing as a covariate), of which there was no effect,  $F(1, 964) = 1.74$ ,  $p = .19$ ,  $\eta^2 = .002$  ( $M_{\text{preoccupying}} = 41.86$  degrees,  $SD = 16.69$ ;  $M_{\text{nonpreoccupying}} = 40.49$  degrees,  $SD = 15.76$ ). There was no effect of manipulation correspondence measurement timing ( $F = 1.19$ ,  $p = .27$ ).

Next, we examined whether preoccupation with secrets predicted judgments of hill slant. Indeed, the more preoccupied participants were with their secrets, the steeper they judged the hill ( $b = .94$ ,  $SE = 0.27$ ; 95% CI = [.42, 1.46];  $t(963) = 3.50$ ,  $p = .0005$ ).

**Control judgments.** Last, an ANOVA tested compared control judgments across the two conditions (including manipulation correspondence measurement timing as a covariate). There was no effect of the secret recall manipulation on control judgments,  $F(1, 964) = 0.37$ ,  $p = .54$ ,  $\eta^2 = .0004$ , ( $M_{\text{preoccupying}} = .01$ ,  $SD = .61$ ;  $M_{\text{nonpreoccupying}} = -.01$ ,  $SD = .62$ ). There was no effect of manipulation correspondence measurement timing ( $F = 1.21$ ,  $p = .27$ ). There was also no relationship between how preoccupied participants were with their secrets and their control judgments ( $b = .01$ ,  $SE = 0.01$ ; 95% CI = [-.01, .03];  $t(963) = 1.46$ ,  $p = .15$ ).

<sup>2</sup> If a participant did not provide a numerical judgment for the temperature estimation (Study 1: 6.1% of participants, Study 2: 3.9% of participants; e.g., wrote “warm”, “chilly,” etc.), the index averaged only their other two  $z$ -scored control judgments.

**Manipulation correspondence measurement timing.** In the preceding analyses, we conducted ANOVAs on preoccupation, hill slant, and control judgments to parallel analyses from Study 1, and we included the timing of the measurement of manipulation correspondence as a covariate in the analyses. Each of these analyses is equivalent to testing a regression with two simultaneous predictors (the recall prompt and manipulation correspondence measurement timing). Correspondingly, it is worth examining whether these two factors interact in predicting these variables. These two variables did not interact to predict reported preoccupation with the recalled secret ( $b = .06$ ,  $SE = 0.17$ ; 95% CI =  $[-0.27, 0.39]$ ;  $t(962) = 0.35$ ,  $p = .72$ ), hill slant judgments ( $b = .18$ ,  $SE = 2.10$ ; 95% CI =  $[-3.93, 4.29]$ ;  $t(962) = 0.09$ ,  $p = .93$ ), or control judgments ( $b = .07$ ,  $SE = .08$ ; 95% CI =  $[-0.08, 0.23]$ ;  $t(962) = 0.91$ ,  $p = .36$ ).

We also reported that preoccupation with secrets predicted judgments of hill slant, and it might be worth testing whether this was moderated by manipulation correspondence timing. Timing of measurement of manipulation correspondence also did not interact with preoccupation of recalled secrets to predict judged hill slant ( $b = .24$ ,  $SE = 0.54$ ; 95% CI =  $[-0.81, 1.29]$ ;  $t(962) = 0.44$ ,  $p = .66$ ). These additional analyses reveal that whether manipulation correspondence was measured between the independent and dependent variables or after the dependent variable did not moderate any effects upon the dependent measures. Thus, in the current domain it does not seem that assessing manipulation correspondence between the independent and dependent variables had any observable effect on the measured outcomes. That said, in other domains of study, measuring manipulation correspondence before the dependent measure could be problematic (e.g., Parrott & Hertel, 1999), and thus it may be preferable to measure after the dependent measure, or manipulate the timing of the measurement (as in Study 2) to directly assess the effects of manipulation correspondence measurement timing.

### Summary

In both studies, the secret recall manipulation did not directly influence judgments of hill slant. Yet, in both studies (a) the secret recall manipulation strongly predicted preoccupation with secrets (but not control judgments) and (b) preoccupation with secrets strongly predicted judgments of hill slant (but not control judgments). We suggest that the reason for this discrepancy is moderation by manipulation correspondence, driven by a hidden contrast effect among the participants who were low in manipulation correspondence.

### Moderation by Manipulation Correspondence

**Study 1.** We first assessed the degree of manipulation correspondence across the two secret recall conditions. In Figure 1, we plot a distribution of preoccupation with recalled secrets for the two secret recall manipulations (distributions are computed using a kernel density estimator, implemented with the R-software ggplot2 package; Wickham, 2009). This plot makes clear, as reported above, that participants asked to recall nonpreoccupying secrets tend to recall less preoccupying secrets, and that participants asked to recall preoccupying secrets tend to recall more preoccupying secrets. The plot, however, also demonstrates overlap between the

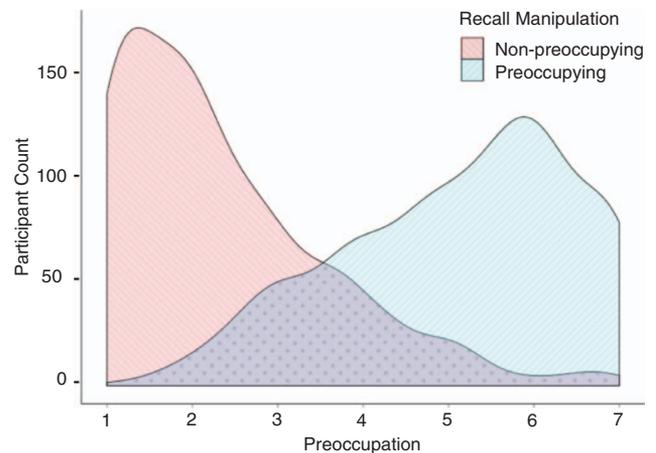


Figure 1. Density plot of manipulation correspondence when recalling preoccupying versus nonpreoccupying secrets in Study 1. See the online article for the color version of this figure.

distributions, with a number of participants recalling secrets less and more preoccupied than requested for by the prompt (32% overlap).

For participants who were prompted to recall nonpreoccupying secrets, we can reverse-score their level of preoccupation with their secrets. This leads to an index that for both groups of participants increases with manipulation correspondence (i.e., increasing preoccupation when asked to recall preoccupying secrets, and increasing nonpreoccupation when asked to recall nonpreoccupying secrets). Using this index of manipulation correspondence, we test whether there is an effect of the secret recall manipulation on judgments of hill slant, moderated by manipulation correspondence.

**Hill slant judgments.** In Step 1, we entered the secret recall condition and manipulation correspondence index in a regression predicting judgments of hill slant. Neither condition nor this measure predicted judgments of hill slant: secret recall condition ( $b = 0.75$ ,  $SE = 1.09$ ; 95% CI =  $[-1.38, 2.89]$ ;  $t(947) = 0.69$ ,  $p = .49$ ); manipulation correspondence ( $b = .15$ ,  $SE = 0.41$ ; 95% CI =  $[-.64, .95]$ ;  $t(947) = 0.38$ ,  $p = .70$ ).

In Step 2, we also entered the condition by manipulation correspondence interaction, which revealed a significant interaction between these factors ( $b = 2.85$ ,  $SE = 0.81$ ; 95% CI =  $[1.26, 4.45]$ ;  $t(946) = 3.51$ ,  $p = .0005$ ; see Figure 2). We examined the simple slopes of the secret recall manipulation at high (+1 SD) and low (-1 SD) manipulation correspondence to decompose the interaction (see Aiken & West, 1991).

At high manipulation correspondence, recalling a preoccupying (vs. nonpreoccupying) secret increased hill slant judgments ( $b = 4.51$ ,  $SE = 1.52$ ; 95% CI =  $[1.52, 7.50]$ ;  $t(946) = 2.96$ ,  $p = .003$ ). In contrast, at low manipulation correspondence, recalling a preoccupying (vs. nonpreoccupying) secret decreased hill slant judgments ( $b = -3.12$ ,  $SE = 1.54$ ; 95% CI =  $[-6.15, -0.09]$ ;  $t(946) = -2.02$ ,  $p = .04$ ).

Thus only when participants recalled a secret that corresponded highly with the manipulation prompt did recalling preoccupying (vs. nonpreoccupying) secrets increase judgments of hill slant. When participants, in contrast, did not recall a secret that corre-

sponded with the manipulation prompt (e.g., recalling nonpreoccupying secrets when asked to recall preoccupying secrets), they demonstrated the opposite pattern of results.

**Control judgments.** These results did not extend to control judgments. We conducted the same analyses with the control numerical judgments index as the outcome. In Step 1, secret recall condition and the manipulation correspondence were entered in a regression predicting control judgments, and neither predicted control judgments: secret recall condition ( $b = .03$ ,  $SE = .04$ ; 95% CI on  $b$   $[-.06, .11]$ ;  $t(947) = 0.60$ ,  $p = .54$ ); manipulation correspondence ( $b = .03$ ,  $SE = .02$ ; 95% CI on  $b$   $[-.01, .06]$ ;  $t(947) = 1.58$ ,  $p = .12$ ). In Step 2, we entered both factors and their interaction, of which there was none ( $b = -.0004$ ,  $SE = .03$ ; 95% CI on  $b$   $[-.06, .06]$ ;  $t(946) = -0.01$ ,  $p = .99$ ).

**Study 2.** As for Study 1, we plotted a distribution of preoccupation with recalled secrets for the two secret recall manipulations. Again, we see a tendency to recall preoccupying secrets in the preoccupying secrets condition, and nonpreoccupying secrets in the nonpreoccupying secrets condition, but also substantial overlap (33%; see Figure 3).

**Hill slant judgments.** We conducted the same two-step regression analysis as above for Study 1, but with the inclusion of timing of the measurement of manipulation correspondence as a predictor.<sup>3</sup> In Step 1, no variables predicted judgments of hill slant: secret recall condition ( $b = 1.57$ ,  $SE = 1.07$ ; 95% CI =  $[-0.52, 3.67]$ ;  $t(962) = 1.48$ ,  $p = .14$ ); manipulation correspondence ( $b = 0.38$ ,  $SE = 0.41$ ; 95% CI =  $[-0.41, 1.18]$ ;  $t(962) = 0.94$ ,  $p = .35$ ); manipulation correspondence measurement timing ( $b = -1.17$ ,  $SE = 1.05$ ; 95% CI =  $[-3.23, 0.88]$ ;  $t(962) = -1.12$ ,  $p = .26$ ).

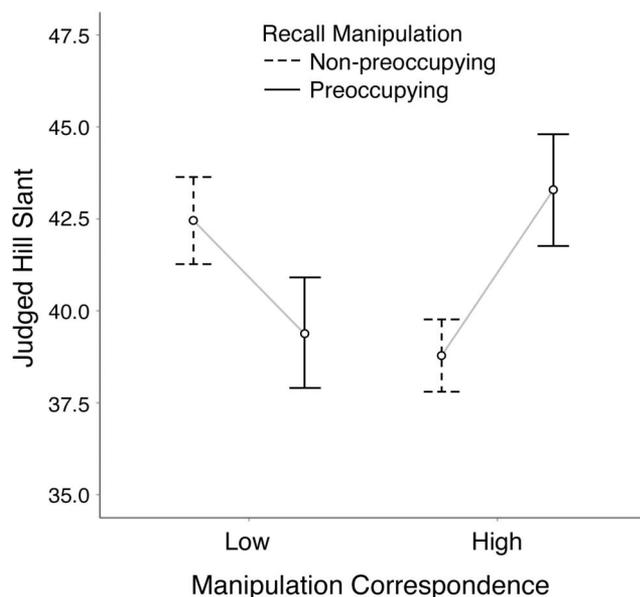


Figure 2. Study 1 interaction between the secret recall condition and the manipulation correspondence index. Only to the extent that participants recalled secrets that corresponded with the secret recall prompt, did recalling preoccupying (vs. nonpreoccupying) secrets increase judgments of hill slant. The plotted regression lines are simple effects of the recall manipulation on judged hill slant assessed at one standard deviation above and below the mean of manipulation correspondence.

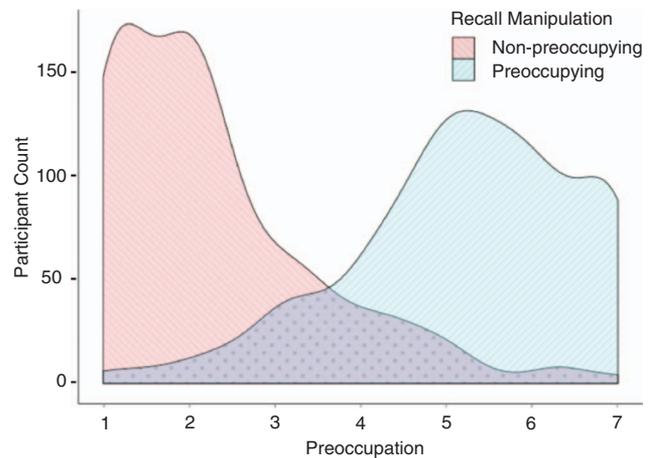


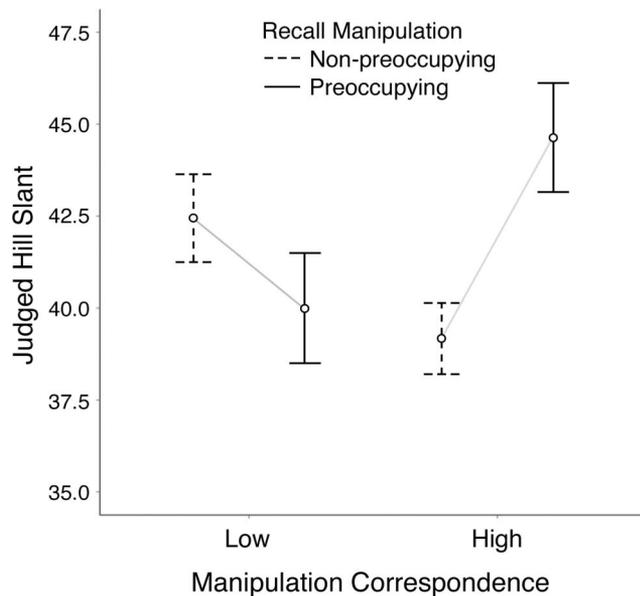
Figure 3. Density plot of manipulation correspondence when recalling preoccupying versus nonpreoccupying secrets in Study 2. See the online article for the color version of this figure.

In Step 2, as in Study 1, we also entered the secret recall by manipulation correspondence interaction term (with the inclusion of manipulation correspondence measurement timing). This revealed a Secret Recall  $\times$  Manipulation Correspondence interaction ( $b = 3.02$ ,  $SE = 0.81$ ; 95% CI =  $[1.44, 4.61]$ ;  $t(961) = 3.74$ ,  $p = .0002$ , see Figure 4). There was no three-way interaction ( $b = 0.68$ ,  $SE = 1.61$ ; 95% CI =  $[-2.49, 3.84]$ ;  $t(958) = 0.42$ ,  $p = .67$ ).

As in Study 1, we examined the simple slopes of the secret recall manipulation at high (+1  $SD$ ) and low (-1  $SD$ ) manipulation correspondence to decompose the interaction. At high manipulation correspondence, recalling a preoccupying (vs. nonpreoccupying) secret increased hill slant judgments ( $b = 5.46$ ,  $SE = 1.49$ ; 95% CI =  $[2.54, 8.38]$ ;  $t(962) = 3.67$ ,  $p = .003$ ). In contrast, at low manipulation correspondence, recalling a preoccupying (vs. nonpreoccupying) secret non-significantly decreased hill slant judgments ( $b = -2.46$ ,  $SE = 1.51$ ; 95% CI =  $[-5.43, 0.51]$ ;  $t(962) = -1.63$ ,  $p = .10$ ).

**Control judgments.** As in Study 1, these results did not extend to control judgments. We conducted the same analyses with the control numerical judgments index as the outcome. In Step 1, no variables predicted control judgments: secret recall ( $b = 0.03$ ,  $SE = 0.04$ ; 95% CI =  $[-0.05, 0.11]$ ;  $t(962) = 0.65$ ,  $p = .51$ ); manipulation correspondence ( $b = .004$ ,  $SE = 0.02$ ; 95% CI =  $[-0.03, 0.03]$ ;  $t(962) = 0.27$ ,  $p = .79$ ); manipulation correspondence measurement timing ( $b = -0.04$ ,  $SE = 0.04$ ; 95% CI =  $[-0.12, 0.03]$ ;  $t(962) = -1.10$ ,  $p = .27$ ). In Step 2, there was no Secret Recall  $\times$  Manipulation Correspondence interaction on control judgments ( $b = 0.05$ ,  $SE = 0.03$ ; 95% CI =  $[-0.01, 0.11]$ ;  $t(961) = 1.49$ ,  $p = .14$ ). There was no three-way interaction ( $b = 0.01$ ,  $SE = 0.06$ ; 95% CI =  $[-0.11, 0.14]$ ;  $t(958) = 0.24$ ,  $p = .81$ ).

<sup>3</sup> In all analyses, removing timing of the measurement of manipulation correspondence as a predictor does not alter the patterns of results or significance.



**Figure 4.** Study 2 interaction between the secret recall condition and the manipulation correspondence index. Only when participants recalled secrets that corresponded with the secret recall prompt, did recalling preoccupying (vs. nonpreoccupying) secrets increase judgments of hill slant. The plotted regression lines are simple effects of the recall manipulation on judged hill slant assessed at one standard deviation above and below the mean of manipulation correspondence.

## Studies Combined

To test for the robustness of these results, we conducted analyses on a combined data set, including study as a predictor variable.<sup>4</sup> In Step 1, no variables predicted judgments of hill slant: secret recall ( $b = 1.16$ ,  $SE = 0.76$ ; 95% CI =  $[-0.33, 2.66]$ ;  $t(1912) = 1.53$ ,  $p = .13$ ); manipulation correspondence ( $b = -.27$ ,  $SE = 0.29$ ; 95% CI =  $[-0.30, 0.83]$ ;  $t(1912) = 0.93$ ,  $p = .35$ ); study ( $b = 0.56$ ,  $SE = 0.75$ ; 95% CI =  $[-0.9, 2.02]$ ;  $t(1912) = 0.75$ ,  $p = .45$ ).

As with the above analyses, in Step 2, we also entered the secret recall by manipulation correspondence interaction term (with the inclusion of study as a predictor). This revealed a Secret Recall  $\times$  Manipulation Correspondence interaction ( $b = 2.94$ ,  $SE = 0.57$ ; 95% CI =  $[1.82, 4.06]$ ;  $t(1911) = 5.13$ ,  $p < .0001$ ). Thus, across both studies, the effect of the secret recall manipulation on judgments of hill slant was moderated by manipulation correspondence (there was no three-way interaction with study ( $b = 0.16$ ,  $SE = 1.15$ ; 95% CI =  $[-2.09, 2.41]$ ;  $t(1908) = 0.14$ ,  $p = .89$ ).

These results did not extend to control judgments: Step 1, secret recall ( $b = 0.03$ ,  $SE = 0.03$ ; 95% CI =  $[-0.03, 0.08]$ ;  $t(1912) = 0.88$ ,  $p = .38$ ); manipulation correspondence ( $b = 0.02$ ,  $SE = 0.01$ ; 95% CI =  $[-0.01, 0.04]$ ;  $t(1912) = 1.38$ ,  $p = .17$ ); study ( $b = 0.01$ ,  $SE = 0.03$ ; 95% CI =  $[-0.05, 0.06]$ ;  $t(1912) = 0.21$ ,  $p = .83$ ). In Step 2, there was no Secret Recall  $\times$  Manipulation Correspondence interaction on control judgments ( $b = 0.02$ ,  $SE = 0.02$ ; 95% CI =  $[-0.02, 0.07]$ ;  $t(1911) = 0.99$ ,  $p = .32$ ), and there was no three-way interaction with study ( $b = 0.05$ ,  $SE = 0.04$ ; 95% CI =  $[-0.04, 0.13]$ ;  $t(1908) = 1.05$ ,  $p = .29$ ).

To illustrate how the current moderation by manipulation correspondence leads to a contrast effect for participants who do not recall secrets that correspond with the secret recall prompt, we can examine the interaction the “other way around,” testing the simple slopes of manipulation correspondence per each secret recall condition. When the secret recall condition was dummy coded such that 0 = preoccupying condition and 1 = nonpreoccupying condition (thus assessing the effect of manipulation correspondence when recalling preoccupying secrets), there was a positive relationship between manipulation correspondence and judgments of hill slant ( $b = 1.62$ ,  $SE = 0.39$ ; 95% CI =  $[0.86, 2.38]$ ;  $t(1911) = 4.17$ ,  $p = .00003$ ). Thus, when participants were asked to recall preoccupying secrets, the more they recalled secrets that corresponded with these instructions, the steeper they judged the hill.

When the secret recall condition was dummy coded such that 1 = preoccupying condition and 0 = nonpreoccupying condition (thus assessing the effect of manipulation correspondence when recalling nonpreoccupying secrets), there was a negative relationship between manipulation correspondence and judgments of hill slant ( $b = -1.32$ ,  $SE = 0.42$ ; 95% CI =  $[-2.15, -0.50]$ ,  $t(1911) = -3.14$ ,  $p = .002$ ).

We present a graph of these results in Figure 5, along with some additional descriptive information. The means per each secret recall condition for both manipulation correspondence and judgments of hill slant are plotted as the circular points (falling along the regression lines). Plotted with these means are error bars (extending from the circular points), which represent the 95% confidence interval around the means. This graph makes clear that manipulation correspondence is slightly but significantly higher when asked to recall nonpreoccupying secrets than when asked to recall preoccupying secrets—comparing the two horizontal error bars, which do not overlap.

Although the means for the direct effect of secret recall on judgments of hill slant do not differ (comparing the two vertical error bars), it is clear that the conditions do differ when taking manipulation correspondence into account. At high levels of manipulation correspondence (the right side of the graph), the burdening effects of preoccupying secrets can be seen. Participants in the preoccupying secret condition are judging the hill to be steeper than are participants in the nonpreoccupying secret condition. At low levels of manipulation correspondence (the left side of the graph), however, that is no longer the case. In fact, it appears in this case that the reverse effect has occurred: at low manipulation correspondence, participants in the preoccupying secret condition are judging the hill to be less steep than are participants in the nonpreoccupying secret condition. This may be due in part to a contrast effect, as described in the next section.

## Contrast Effect

Hidden in Figure 5 is the predicted contrast effect. The different slopes (i.e., signs of the regressions) across the two conditions reveal the continuous outcome of manipulation correspondence. When asked to recall preoccupying secrets, the more participants recalled secrets that corresponded with these instructions, the more steep they judged the hill. When asked to recall nonpreoccupying

<sup>4</sup> In all analyses, removing study as a predictor does not alter the patterns of results or significance.

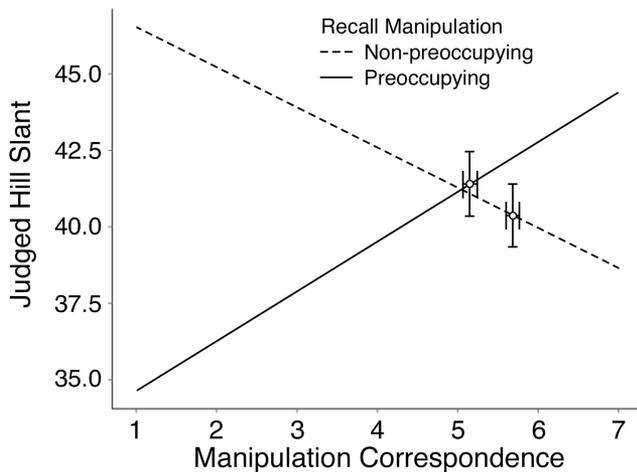


Figure 5. Studies 1 and 2 combined (including study as a predictor): Recalling secrets that corresponded with the preoccupation prompt increased judgments of hill slant, whereas recalling secrets that corresponded with the nonpreoccupation prompt decreased judgments of hill slant.

secrets, the more participants recalled secrets that corresponded with these instructions, the less steep they judged the hill, consistent with the proposal that reduced preoccupation is associated with reduced judgments of hill slant.

If we “flip” the dashed nonpreoccupied line around (i.e., horizontally flipping it along the  $x$ -axis), the only change in interpretation of the graph is that we are now examining the relationship between preoccupation with recalled secrets (rather than manipulation correspondence) and judged hill slant across the two conditions. We present the same exact graph, but now without reverse-scoring preoccupation values for the nonpreoccupied condition (i.e., this “flips” the nonpreoccupation line around, thereby presenting preoccupation along the  $x$ -axis rather than manipulation correspondence).

Figure 6 shows all of the reported results plus the predicted contrast effect. The means per each secret recall condition for both preoccupation and judgments of hill slant are plotted as the circular points (falling along the regression lines). Plotted with these means are error bars, which represent the 95% confidence interval around the means (extending from the circular points). This graph makes clear that being asked to recall preoccupying secrets leads to more preoccupation than does being asked to recall nonpreoccupying secrets (compare the two horizontal error bars, which are far apart). Next, as both regression lines are positive and significant and slope, it is clear that the more participants are preoccupied with their secrets, the steeper they judge the hill. As before, comparison of the plotted means—but here along judgments of hill slant—reveals that the secret recall manipulation does not have a direct effect on judgments of hill slant (comparing the two vertical error bars, which overlap).

Thus, in this graph we can see that (a) random assignment to recall preoccupying (vs. nonpreoccupying) secrets leads to the recall of more preoccupying secrets (see horizontal error bars), and (b) recalling more preoccupying secrets is associated with increased judgments of hill slant (across conditions, see the slopes of both lines), but (c) random assignment to recall preoccupying (vs.

nonpreoccupying) secrets did not increase hill slant judgments (see vertical error bars). Figure 6 reveals the explanation for this perplexing pattern; it reveals the hidden contrast effect.

As previewed already, comparing the two regression lines reveals the predicted contrast effect. While the slopes are similar, the nonpreoccupying line is higher than the preoccupying line (i.e., the dashed intercept is above the solid intercept), revealing that when controlling for how preoccupied participants are with their recalled secret, being randomly assigned to recall preoccupying secrets is associated with reduced judgments of hill slant.

We tested the contrast effect statistically. We conducted the same regression analysis as reported above in the combined moderation analysis, but swapping the manipulation correspondence index for the preoccupation index. In Step 1 (entering both recall manipulation and measured preoccupation), preoccupation with secrets positively predicted judgments of hill slant ( $b = 1.48$ ,  $SE = 0.29$ ; 95% CI = [0.92, 2.04];  $t(1912) = 5.19$ ,  $p < .00001$ ), and—consistent with a contrast effect emerging when accounting for preoccupation—recalling preoccupying (vs. nonpreoccupying) secrets decreased judgments of hill slant ( $b = -3.19$ ,  $SE = 1.10$ ; 95% CI = [-5.34, -1.03];  $t(1912) = -2.9$ ,  $p = .004$ ); there was no effect of study ( $b = 0.56$ ,  $SE = 0.74$ ; 95% CI = [-0.9, 2.01];  $t(1912) = 0.75$ ,  $p = .45$ ). In Step 2, there was no Preoccupation  $\times$  Recall Manipulation interaction ( $b = 0.39$ ,  $SE = 0.57$ , 95% CI = [-0.83, 1.42];  $t(1911) = 0.52$ ,  $p = .60$ ). There was no three-way interaction with study ( $b = 0.44$ ,  $SE = 1.15$ ; 95% CI = [-1.81, 2.68];  $t(1908) = 0.38$ ,  $p = .70$ ).

### Summary of Manipulation Correspondence Effects

We observed two effects due to manipulation correspondence, which are visualized in Figures 5 and 6. These two figures are identical, except for “flipping” the nonpreoccupation line (flipping it horizontally, along the  $x$ -axis). Figure 5 demonstrates moderation by manipulation correspondence. On the right side of Figure

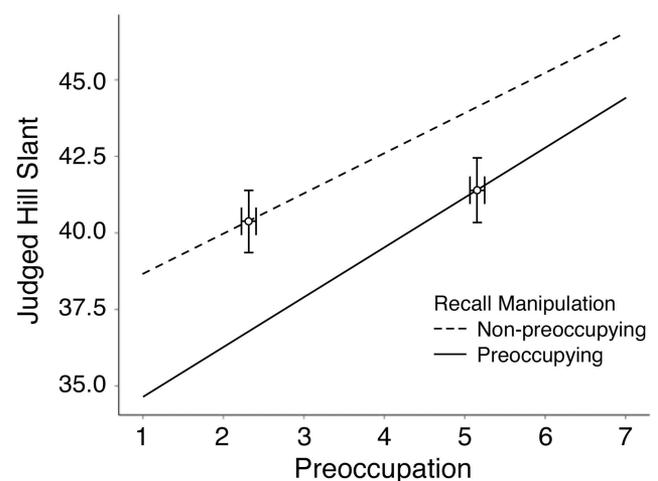


Figure 6. Studies 1 and 2 combined (including study as a predictor): Preoccupation with secrets, independent of the secret recall manipulation, is associated with increased judgments of hill slant, but being asked to recall preoccupying (vs. nonpreoccupying) secrets, independent of preoccupation, is associated with reduced judgments of hill slant.

5 is the burdening effect of preoccupying secrets, as hypothesized in prior work (Slepian et al., 2015). When manipulation correspondence is low (on the left side of Figure 5), however, that effect is no longer seen. In fact, the reverse pattern appears to be true: under low manipulation correspondence, participants in the nonpreoccupying secret condition are estimating steeper hills than participants in the preoccupying secret condition. This may be due in part to a contrast effect, which is revealed in Figure 6.

In Figure 6, we see what initially seem to be contradictory findings. We see that (a) participants prompted to recall preoccupying secrets do recall more preoccupying secrets than participants prompted to recall nonpreoccupying secrets (i.e., compare the two horizontal error bars, which do not overlap), and (b) the more preoccupied participants are with their secrets, the steeper they judge the hill (i.e., examine the two regression slopes, which are both significantly positive), but (c) participants asked to recall preoccupying secrets did not judge the hill as steeper than participants asked to recall nonpreoccupying secrets (compare the two vertical error bars, which overlap). This seemingly perplexing pattern of results is explained by the contrast effect: When accounting for preoccupation with recalled secrets, participants asked to recall nonpreoccupying secrets judge the hill as steeper than do participants asked to recall preoccupying secrets (compare the two intercepts; the nonpreoccupying line is higher than the preoccupying line). In other words, the reason we find that (a) the secret recall manipulation predicts preoccupation and (b) preoccupation predicts judgments of hill slant, but (c) the secret recall manipulation does not predict judgments of hill slant, is that (d) the contrast effect is masking the moderation by manipulation correspondence.

Low manipulation correspondence promotes contrast. The instructions for preoccupying secrets ask participants to recall a secret they think about often, really affects them, and really bothers them. Recalling anything short of a highly preoccupying secret in response to these instructions could make such a secret seem trivial, relative to the prompt, and thereby less burdensome. In contrast, the prompt that asks for nonpreoccupying secrets asks participants to recall a secret that they almost never think about, does not really affect them, and that they feel okay about. Recalling anything short of the least preoccupying secret in response to these instructions may make such a secret seem significant, relative to the prompt, and thereby burdensome. This would lead to precisely the results found in Studies 1 and 2 (see Figure 6).

## Discussion

Preoccupation with secrets was related to increased judgments of hill slant in Studies 1 and 2, consistent with the notion that by devoting resources toward a secret, other pursuits seem more forbidding. With high-powered designs (total  $N = 2,000$ ), we find the manipulation of recalling preoccupying versus nonpreoccupying secrets, however, can fail to produce consistent manipulation correspondence. Without taking into account manipulation correspondence, the current work would have failed to detect two effects that mask the relationship between preoccupation with secrets and judgments of hill slant: (a) moderation by manipulation correspondence and (b) a contrast effect induced by low manipulation correspondence.

Studies 1 and 2 measured manipulation correspondence, an often-unappreciated feature of statistical power. That is, measuring manipulation correspondence can vastly increase statistical power, whereas simply increasing sample size may not be enough (see Hansen & Collins, 1994). With two high-powered studies, we found that when accounting for correspondence with the secrecy manipulation, a clear relationship emerged between preoccupation with secrets and judgments of hill slant. Other replication work should consider not just sample size, but also the measurement of manipulation correspondence, thereby increasing statistical power.

## Study 3

Decades of research on contrast effects demonstrate that when people compare their mental contents to extreme exemplars, they contrast away from those exemplars in their judgments (Bless & Schwarz, 2010; Herr et al., 1983; Strack & Mussweiler, 1997). Our studies reveal exactly this contrast effect pattern by accounting for manipulation correspondence. Recalling a preoccupying secret in the nonpreoccupying condition felt particularly preoccupying, and recalling a nonpreoccupying secret in the preoccupying condition felt particularly nonpreoccupying.

In Study 3, we manipulated this context effect directly. To do this, we used the same secret recall manipulation as in Studies 1 and 2, but we added a subsequent manipulation that exposed participants to either a highly preoccupying or nonpreoccupying exemplar secret. This procedure provides an experimental test of the contrast effect and also offers a more potent means of manipulating preoccupation with secrets. In this experiment, we leveraged the reliable finding that people contrast away from extreme exemplars. In one condition, the exemplar secret was an extreme example of the original manipulation prompt (e.g., after recalling a preoccupying secret, participants are exposed to an extremely preoccupying secret). This exposure to an extreme exemplar should promote contrast away from the extreme exemplar. Hence, by providing an extreme example of the prompt, people should be likely to contrast away from the intent of the original prompt because people contrast away from extreme stimuli. In this condition, we thus present prompt-extreme exemplars.

In the other condition, the exemplar secret was an extreme version of the opposite prompt (e.g., after recalling a preoccupying secret, participants were exposed to an extremely nonpreoccupying secret). By contrasting from an exemplar that highly fits the opposing prompt, participants are thereby assimilating toward the intent of the original prompt (i.e., akin to how a double negative yields a positive). In this condition, we thus present extreme-opposing exemplars.

We predict that when participants recall a preoccupying secret, and are exposed to an extremely preoccupying exemplar, they will then judge their secret as less significant compared to the exemplar, and thereby they will feel less preoccupied by their own secret. Conversely, we predict that when participants recall a preoccupying secret and are exposed to an extremely nonpreoccupying exemplar, they will then judge their secret as more significant relative to the exemplar, and thereby feel more preoccupied by their own secret.

Finally, we predict that increased preoccupation with secrets will predict increased judgments of hill slant (but not control judgments). Thus, after recalling a secret, and being exposed to a

secret, we ask participants how significant their secret now seems (to measure how well they feel their secret corresponds to the prompt) and then measure consequent preoccupation and judgments of hill slant.

A main goal of Study 3 was to examine not only the effect of exposing participants to extreme exemplars, but critically the process by which these exposures might influence downstream outcomes. From the preceding studies, we propose that (a) the exemplar provides an anchor that influences participants' judgments of their own manipulation correspondence, and (b) this anchoring effect then changes how preoccupied participants feel by their secret. Asking participants how much their secret differed from the subsequent exemplar thus captured the extent to which participants adjusted their judgment of the secret from the anchor set by the exemplar (i.e., how much the exemplar influenced their own perceived manipulation correspondence; this correspondence with the recall prompt should then go on to predict preoccupation with the secret).

Measuring these variables thus allowed us to model the effect of exposure to extreme exemplars on how much participants feel preoccupied by their own secret (i.e., a measure of how burdensome they feel that secret is) through their judged significance of their secret, which would depend on the nature of the exemplar exposure (i.e., moderated mediation). The a priori order of the variables in our model is perfectly and purposefully aligned with the order in which the variables were measured (see Figure 7).

### Participants and Design

As with the prior studies, 1,000 participants (432 male, 568 female;  $M_{\text{age}} = 33.10$  years,  $SD = 10.37$ ) were recruited on Mechanical Turk. The design was a 2 (recall prompt: preoccupying, nonpreoccupying)  $\times$  2 (exemplar secret: prompt-extreme, prompt-opposing) between-subjects design. The recall prompt and exemplars concerned secrets being kept from one's partner; this was done to maximize the likelihood that participants would compare their secrets to the secrets that they were subsequently shown (i.e., we did not want secrets recalled and subsequent exemplars to be too distinct to compare to each other). Recruitment materials advertised the study as for only participants who were currently in committed relationship.

As before, exclusions were decided ahead of time. Sixty-one participants (6.1%) stated that they did not have a secret to recall ( $n = 36$  preoccupying;  $n = 25$  nonpreoccupying); 21 participants (2.1%) claimed to have recalled a secret but then later indicated

that it was not a true secret during the honesty check (described subsequently); an additional 8 (0.8%) participants provided a hill slant judgment other than a number between 1 and 89, and thus these participants were excluded from analysis.

### Procedure

Upon entering the study advertised for those in committed relationships, participants first reported how long they have been with their partner. Next, participants completed the same recall prompt as in Studies 1 and 2 and were asked to recall a secret that they were keeping from their partner.

Participants were then exposed to one of 20 secrets (10 extremely preoccupying, 10 extremely nonpreoccupying), which were adapted from prior secrets collected by the authors (for the exact exemplars see the Appendix). It was explained that as part of the study, the researchers are asking participants to evaluate a secret someone shared on a social media website for sharing secrets. The presented secrets were adapted from secrets collected for another research project on secrets kept from partners, and were about 10 different topics (money, sex, illegal drugs, issues of trust, breaking a law, having children, gambling, substance abuse, employment, and a childhood story). These 10 topics capture some of the most common kinds of secrets people keep from their partner, and for each type of secret (e.g., gambling), we created a nonpreoccupying and a preoccupying version (e.g., secretly paying \$5 to enter an office betting pool and not telling one's partner vs. losing substantial sums of money gambling on the Internet and not telling one's partner). Secrets were thus matched on content and length of text but differed in terms of how preoccupying the secrets would be. By creating 20 different exemplars (10 preoccupying, 10 nonpreoccupying), rather than 1 preoccupying exemplar versus 1 nonpreoccupying exemplar, results from secret exemplar exposure would not be a consequence of a single stimulus. Moreover, this method allows us to account for random variance of exemplar exposure on judged comparative secret significance.

To capture the validity of the context effects, participants reported how their secret compared with the one to which they were exposed. Participants were asked, "How does this person's secret compare to the secret you described earlier?" from 1 (my secret is very trivial and insignificant, compared to this person's) to 7 (my secret is very much more serious and significant, compared to this person's), with the midpoint, 4 (my secret is as significant as this person's). In the preoccupation condition, for example, the more participants judge their secret

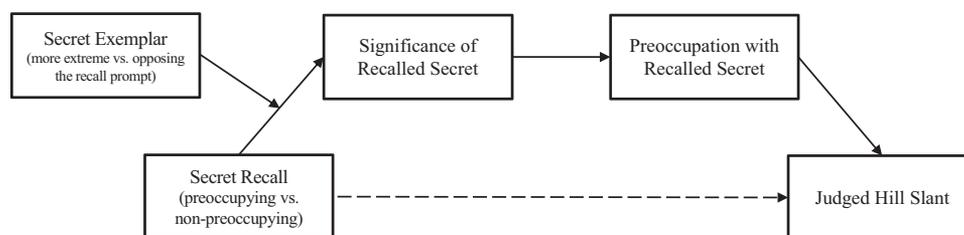


Figure 7. Predicted moderated mediation model in Study 3. When the secret exemplar leads participants to judge their own secret as significant, it will increase preoccupation and judgments of hill slant. Conversely, when the secret exemplar leads participants to judge their own secret as not very significant, it will decrease preoccupation and judgments of hill slant.

as more significant than the subsequent exemplar, the more they should feel that their secret corresponds with their original recall prompt. Next, participants completed the measure of preoccupation ( $\alpha = .96$ ), and the control estimations along with judged hill slant (from Studies 1 and 2). Last, participants were informed that the researchers understand that sometimes people might not be fully accurate in completing a study, and why a participant might not give accurate information. Participants were reminded that they would be compensated no matter how they answered, but that it would help the researchers out to know whether the participant actually did not recall a true secret in the study. Participants who indicated that they did not recall a true secret in the study thus failed the honesty check.

## Results

Given that participants were exposed to one of 20 different secret exemplars this allows us to test for our predicted interaction while also accounting for random variance from exemplars. Thus, we can treat stimulus exposure as a random factor to ensure that any effect on judged secret significance (i.e., the reanchoring effect) is not due to specific stimulus selection. This is important because it allows us to suggest resulting reanchoring effects generalize across both participants and stimuli (see Judd, Westfall, & Kenny, 2012).

To account for random variance from stimuli (i.e., secret exposure exemplars), we used the R package lme4 to implement mixed-effects models (Bates, Maechler, Bolker, & Walker, 2015). In calculating  $p$  values, we used the R package lmerTest to run lme4 models through Satterthwaite approximation tests to estimate the degrees of freedom (these estimated degrees of freedom scale the model estimates to best approximate the  $F$  distribution, and thus can be fractional and differ slightly across tests; Kuznetsova, Brockhoff, & Christensen, 2013).<sup>5</sup>

**Exemplar exposure reanchoring participants' judgments of their secret's significance.** We first entered both secret recall prompt and secret exemplar conditions as fixed factors and stimulus (i.e., secret exposure exemplar) as a random factor, predicting judgments of secret significance, which revealed a main effect of secret recall. Recalling preoccupied secrets led participants to judge their secrets as more significant ( $M = 4.14$ ,  $SD = 2.09$ ) than did participants recalling nonpreoccupied secrets ( $M = 2.92$ ,  $SD = 1.92$ ;  $b = 1.24$ ,  $SE = 0.13$ ; 95% CI = [0.98, 1.50];  $t(904.91) = 9.34$ ,  $p < .0001$ ). There was no main effect of the exemplar condition ( $M_{\text{prompt-extreme}} = 3.46$ ,  $SD = 1.90$ ;  $M_{\text{prompt-oppsing}} = 3.58$ ,  $SD = 2.26$ ;  $b = -0.14$ ,  $SE = 0.13$ ; 95% CI = [-0.40, 0.11];  $t(904.91) = -1.09$ ,  $p = .27$ ).

Critically, these effects were qualified by an interaction between these two factors ( $b = -4.80$ , 95% CI = [-5.21, -4.39],  $SE = 0.21$ ;  $t(900.6) = 22.78$ ,  $p < .0001$ ). When exposed to prompt extreme (contrast producing) exemplars, recalling preoccupied versus nonpreoccupied secrets led to decreased judgments of secret significance ( $b = -1.31$ , 95% CI = [-1.61, -1.01],  $SE = 0.15$ ;  $t(902.6) = 8.52$ ,  $p < .0001$ ;  $M_{\text{preoccupying}} = 2.79$ ,  $SD = 1.71$ ;  $M_{\text{nonpreoccupying}} = 4.11$ ,  $SD = 1.85$ ).

Conversely, when exposed to extremely prompt-opposing (assimilation producing) exemplars, recalling preoccupied versus nonpreoccupied secrets led to increased judgments of secret significance ( $b = 3.49$ ,  $SE = 0.14$ ; 95% CI = [3.21, 3.78];

$t(900.6) = 24.14$ ,  $p < .0001$ ;  $M_{\text{preoccupying}} = 5.34$ ,  $SD = 1.62$ ;  $M_{\text{nonpreoccupying}} = 1.85$ ,  $SD = 1.23$ ).

These two simple effects are consistent with our reanchoring predictions. That is, when recalling a preoccupied (vs. nonpreoccupied) secret, exposure to another targets' extremely preoccupied secret leads to a contrast effect, whereby participants feel their own secret to be comparatively less significant (i.e., corresponding less with the original recall prompt). This was precisely the mechanism proposed for the contrast effects found in Studies 1 and 2. Conversely, when recalling a preoccupied (vs. nonpreoccupied) secret, exposure to another targets' extremely nonpreoccupied secret leads to a reanchoring effect, in which by seeing their secret as different from the exemplar; participants now feel their own secret to be more significant (i.e., corresponding more with the original recall prompt).

**A moderated indirect effect on preoccupation.** We predicted that the effect of secret recall on judged secret significance would predict preoccupation. That is, increases in judged secret significance as a function of secret recall—moderated by the secret exposure manipulation—will predict preoccupation with those recalled secrets. Stated in more simple terms, the reanchoring effect on judged secret significance will go on to influence participants' felt preoccupation with their secret.

As predicted, judged secret significance predicted preoccupation with the recalled secret when also entering secret recall, type of anchor, and their interaction term as fixed factors, and stimulus exemplar as a random factor ( $b = 0.31$ , 95% CI = [0.26, 0.37],  $SE = 0.03$ ,  $t(905) = 11.40$ ,  $p < .0001$ ).

This led us to test a formal multilevel bootstrapped moderated mediational path (with 5,000 iterations) testing for the indirect effect of Secret Recall (preoccupied vs. nonpreoccupied)  $\times$  Exposure (extreme-prompt exemplar vs. prompt-opposing exemplar) on preoccupation—through judgments of secret significance (including stimulus exemplar as a random factor). This analysis revealed that when exposed to extreme-prompt (i.e., contrast producing) exemplars, recalling preoccupied secrets decreased preoccupation with secrets through judging the recalled secrets as less significant ( $M_{\text{indirect effect}} = -0.4112$ ,  $SE = 0.0708$ ; 95% CI = [-0.5667, -0.2859]). Again, this is parallel to the contrast effects found in the earlier studies.

A different pattern of results emerged, however, when exposed to secrets that opposed the prompt. Here, recalling preoccupied (vs. nonpreoccupied) secrets increased preoccupation with secrets through judging the recalled secrets as more significant (i.e., through the reanchoring effect;  $M_{\text{indirect effect}} = 1.0970$ ,  $SE = 0.1210$ , 95% CI = [0.8586, 1.3313]). This comparison was assimilative, causing participants to find their preoccupied secrets as correspondingly more preoccupied.<sup>6</sup>

<sup>5</sup> All Study 3 analyses (i.e., direct, indirect, and moderated effects) that are significant when using multilevel modeling are also significant when using traditional regression analyses that do not include stimuli as a (random) factor.

<sup>6</sup> For the interested reader, we report the moderated indirect effect using traditional regression procedures (rather than multilevel models) in this study. When exposed to extreme prompt exemplars, recalling preoccupied secrets decreased preoccupation with secrets through judging the recalled secrets as less significant ( $M_{\text{indirect effect}} = -0.4127$ ,  $SE = 0.0735$ ; 95% CI = [-0.5759, -0.2859]). When exposed to secrets that opposed the prompt, recalling preoccupied (vs. nonpreoccupied) secrets increased preoccupation with secrets through judging the recalled secrets as more significant ( $M_{\text{indirect effect}} = 1.0954$ ,  $SE = 0.1227$ ; 95% CI = [0.8600, 1.3395]).

One way to visualize these results on preoccupation is to present the density graphs of preoccupation with recalled secrets for the four conditions (per the earlier preoccupation density graphs). We present these density graphs in Figure 8. Each condition is presented in a different color (and shading) and where the overlaps exist so too do the colors and shadings. One feature that can be seen in this graph is that by reanchoring participants, we reduce the overlap between preoccupation of recalled secrets [where the prompt-opposing (assimilation producing) conditions overlap (the diamond-patterned brown) is significantly reduced, relative to where the prompt-extreme (contrast producing) conditions overlap (the hatched purple plus the diamond-patterned brown)].

**A moderated indirect effect on judged hill slant.** Last, we predicted that the moderated indirect effect of secret recall on preoccupation (through secret significance) would go on to predict judgments of hill slant, but not control judgments (see Figure 7). Preoccupation with recalled secrets (a fixed factor) predicted judged hill slant (when also entering secret recall, secret exposure, their interaction term, and judged secret significance as fixed factors and stimulus exemplar as a random factor),  $b = 1.78$ ,  $SE = 0.41$ ; 95% CI = [0.98, 2.58];  $t(900.12) = 4.37$ ,  $p < .0001$ . There was no such effect when predicting control judgments,  $b = 0.02$ ,  $SE = 0.02$ ; 95% CI = [-0.01, 0.05];  $t(904) = 1.11$ ,  $p = .27$ .

This led us to test a formal bootstrapped multilevel moderated meditational path (5,000 interactions) testing for the indirect effect of secret recall on judged hill slant through judgments of secret significance and preoccupation with recalled secrets, when exposed to prompt-extreme versus prompt-opposing anchors.

This analysis revealed that when exposed to prompt-extreme exemplars, recalling preoccupied (vs. nonpreoccupied) secrets decreased judged hill slant through the recalled secrets being seen as less significant and less preoccupied ( $M_{\text{indirect effect}} = -0.7330$ ,  $SE =$

0.2099; 95% CI = [-1.2009, -0.3795]). There was no such effect for control judgments ( $M_{\text{indirect effect}} = -0.0071$ ,  $SE = 0.0068$ ; 95% CI = [-0.0213, 0.0055]). Once again, this is parallel to the contrast effects found in the earlier studies.

As expected, a different pattern was found for exposure to prompt-opposing exemplars. When exposed to a secret that highly opposed the recall prompt, recalling preoccupied (vs. nonpreoccupied) secrets now increased judged hill slant through the recalled secrets being seen as more significant and more preoccupied ( $M_{\text{indirect effect}} = 1.9557$ ,  $SE = 0.5059$ ; 95% CI = [1.0481, 3.0777]). There was no such effect for control judgments ( $M_{\text{indirect effect}} = 0.0191$ ,  $SE = 0.0177$ ; 95% CI = [-0.017, 0.0532]).<sup>7</sup>

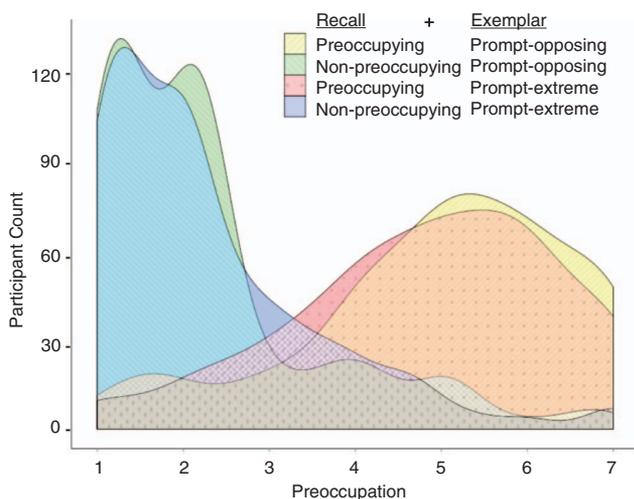
## Discussion

Study 3 provides experimental evidence in support for the contrast effects found in Studies 1 and 2. The two prior studies demonstrated strong and reliable contrast effects, but these were found when taking into account an individual difference variable. Study 3 tested this hypothesized contrast effect directly by exposing participants to secrets that were either highly extreme, consistent with the extreme prompt, and thereby created a contrast effect (i.e., by contrasting from this extreme exemplar, participants are also contrasting from the prompt) or were opposed to the prompt and thereby created an assimilation effect (i.e., by contrasting from this extreme opposing exemplar, participants are assimilating toward the prompt). The moderation pattern found in Study 3 resulted from a manipulated variable; Study 3 thus provides experimental support for the contrast effect.

A contribution of Study 3 is presenting an experimental paradigm that overcomes the limitations of the paradigm of Studies 1 and 2. By exposing participants to secrets that opposed their secret recall prompt, participants felt that their secret fit the prompt well; participants who were asked to recall a preoccupied secret and compared it to a nonpreoccupied secret felt that their recalled secret was indeed significant (corresponding with the prompt) and thereby preoccupied. This moderated indirect effect predicted judgments of hill slant (but not control judgments), thereby presenting an experimental paradigm that can be used to find a causal influence of secrecy recall on judgments of hill slant.

## General Discussion

Three high power studies (total  $N = 3,000$ ) found that preoccupation with secrets increased judgments of an external environ-



**Figure 8.** Density plot preoccupation across conditions in Study 3. The contrast effect (here shown with prompt-extreme exemplars) produces greater overlap in preoccupation (exactly as in Studies 1 and 2), whereas providing participants with prompt-opposing exemplars reanchors participants to be more preoccupied by secrets recalled in the preoccupation condition (and less preoccupied by secrets recall in the nonpreoccupation condition), thereby reducing overlap between conditions. See the online article for the color version of this figure.

<sup>7</sup> Again, for the interested reader, we report the moderated indirect effect using traditional regression procedures (rather than multilevel models) in this study. When exposed to prompt-extreme exemplars, recalling preoccupied (vs. nonpreoccupied) secrets decreased judged hill slant through the recalled secrets being seen as less significant and less preoccupied ( $M_{\text{indirect effect}} = -0.7339$ ,  $SE = 0.2098$ ; 95% CI = [-1.2242, -0.3848]). There was no such effect for control judgments ( $M = -0.0072$ ,  $SE = 0.0067$ ; 95% CI = [-0.0208, 0.0054]). When exposed to a secret that highly opposed the recall prompt, recalling preoccupied (vs. nonpreoccupied) secrets increased judged hill slant through the recalled secrets being seen as more significant and more preoccupied ( $M_{\text{indirect effect}} = 1.9479$ ,  $SE = 0.5103$ ; 95% CI = [1.0195, 3.0414]). There was no such effect for control judgments ( $M_{\text{indirect effect}} = 0.0190$ ,  $SE = 0.0177$ , 95% CI = [-0.0161, 0.0538]).

ment as more forbidding, consistent with the notion that secrecy is burdensome. These effects only occurred when participants recalled preoccupying secrets or compared their secrets with an extremely nonpreoccupying exemplar. These findings reveal important insights into the nature of secrecy, replications, and contrast effects.

## Implications for Secrecy

**Manipulating secrecy.** Secrecy research can focus on two processes that can be empirically separated: (a) inhibition (independent of secrecy cognition) and (b) secrecy cognition (independent of inhibition). For example, prior work has examined the effects of inhibition stemming from concealment, divorced from secrecy cognition. Critcher and Ferguson (2014) asked heterosexual individuals to not reveal their sexual orientation in a mock interview. What made this difficult is that the interviewer asked questions about their ideal dating partner, requiring participants to respond while not uttering pronouns like “he,” “she,” “him,” or “her.” Avoiding highly common words like “he” in speech is difficult and depleting. By using heterosexuals, for whom sexuality is not a secret, this prior work was able to focus on the effects of inhibition separate from keeping secrets. Inhibition during concealment is depleting, consistent with an extensive body of work demonstrating that spending cognitive resources can be fatiguing and leads to conservation of cognitive, motivational, and energetic resources (Baumeister & Vohs, 2007).

Yet, independent of inhibition processes, thinking about the content of a secret leads to feelings of burden, as indicated by perceptual judgments (Slepian, Masicampo, & Ambady, 2014; Slepian et al., 2015). Exaggerated perceptual judgments reflect a judgment of that perceptual space as challenging to navigate. When a physical distance seems challenging, people judge it as forbidding (i.e., far). When a hill slant seems challenging, people judge it as forbidding (i.e., steep). Many factors that make physical space seem more challenging to navigate (advanced age, fatigue, low fitness, pain) increase judgments of hill slant and distance (Cole & Balci, 2013; Eves, 2014; Proffitt, 2006; Sugovic & Witt, 2013; Witt et al., 2004, 2009). Simply thinking about secrets, even without concealing them in the moment, also leads to increased judgments of hill slant.

Secrecy may influence perceptual judgments by highlighting a lack of social resources. There is a large and growing body of work demonstrating that perceived social resources influence judgments of the external world as forbidding. Secrecy is a multifaceted process and not a simple act of inhibition (see Kelly, 2002). That is, by not revealing the secret to another person, one denies oneself the chance to be forgiven for misdeeds. Without another to discuss the secret with, people may feel at a loss for how to handle the sensitive information, misunderstood, guilty, lost, and without purpose.

Feeling guilty, unsupported, unforgiven, lacking purpose, and misunderstood, all linked to secrecy, have also each been linked to burden-based outcomes in prior work. For example, feeling the experience of carrying weight has been linked to feelings of guilt (Kouchaki, Gino, & Jami, 2014). Conversely, feeling guilty can lead to enhanced feelings of burden as demonstrated by increased subjective judgments of the self’s body weight (Day & Bobocel, 2013) and judged weight of objects (Min & Choi, 2015). Being

forgiven, however, reduces this feeling of burden, thereby decreasing judgments of hill slant (Zheng, Fehr, Tai, Narayanan, & Gelfand, 2015). Feeling supported also reduces one’s feeling of burden, decreasing judgments of hill slant (Schnall et al., 2008) as do feelings of being understood by others (Oishi, Schiller, & Gross, 2013). Likewise, feeling affirmed decreases a sense of burden, as demonstrated by reduced judgments of distance (Shea & Masicampo, 2014). Finally, when people feel they lack purpose in life (i.e., feeling that they are missing an important psychological resource), whether measured or manipulated, they judge both pictured and real hills as more effortful to climb, and thereby steeper (Burrow et al., 2015). This growing body of literature demonstrates that reductions in perceived social resources are related to experiences of burden, influencing perceptual judgments of the external world, and that increases in perceived social resources diminish this experience of burden. Secrecy similarly influences perceptual judgments of the external world, but this can be hidden in experimental designs that do not precisely manipulate or measure secrecy processes.

We have demonstrated that when simply thinking about a secret, without having to actively hide it, participants feel a sense of burden from recalling preoccupying secrets. As a consequence, it is not necessary to artificially give participants secrets in the laboratory to test for the effects of secrecy. In other words, to the extent that researchers are interested in studying the effects of thinking about secrets that people actually keep, and those that actually matter in their personal lives, the recall manipulation is an excellent paradigm to explore the effects of secrets. The current research demonstrates, however, that certain manipulations of secret recall can have unintended consequences, which could render the secret recall manipulation as potentially problematic to implement. In other words, the secret recall manipulation is the only manipulation in extant literature that can experimentally test how the secrets people actually keep are burdensome (independent of inhibition), and yet certain instantiations of this manipulation can actively work against the researcher if they introduce unwanted contrast effects.

**“Big” versus “small” secrets.** Originally, Slepian and colleagues (2012; Studies 1–2) asked participants to recall “big” versus “small” secrets as a manipulation of secret recall. It was reasoned that recalling “big” versus “small” secrets should lead to more burden-consistent because the former should be more preoccupying and thus burdensome. Yet, Slepian and colleagues (2015) demonstrated that asking participants to recall “big” secrets does not reliably lead participants to recall secrets that are more preoccupying than “small” secrets. What seems to be the case is that “big” secrets can be thought of as both secrets that are normatively and conventionally treated as big, and as secrets that individuals are more preoccupied by, relative to “small” secrets. For example, conventionally, infidelity is treated as a “big” secret. Yet, individual differences exist in how preoccupied individuals are by having such a secret. Only to the extent that individuals are preoccupied by having committed infidelity, do they exhibit burden-consistent outcomes (Slepian et al., 2012, Study 3). Thus, a participant could recall a secret normatively treated as “big” when asked to do so, but actually recall a secret that they are not highly preoccupied by. In this example, when Slepian and colleagues (2012, Studies 1–2) asked participants to recall “big” versus “small” secrets, they assumed this conceptual dichotomy

would reliably place participants into distinct conditions in which they would be recalling secrets that they were highly versus lowly preoccupied by. This assumption turns out to have been unwarranted, and thus it makes sense that this manipulation produces inconsistent results.

**Preoccupying versus nonpreoccupying secrets.** To overcome the limitations of asking participants to recall “big” versus “small” secrets, Slepian and colleagues (2015) asked participants to recall secrets that they were preoccupied versus not preoccupied by, thereby asking for secrets with qualities closer to the experience of interest (i.e., secrets that individuals devote more vs. less resources toward). Indeed, this manipulation effectively does manipulate the variable of interest (i.e., how preoccupied participants are with their secret, which in turn, predicts judging the external world as more challenging and forbidding; Slepian et al., 2015). That said, in the real world, there are not categorically different kinds of secrets; secrets exist on many psychological continua. Hence, despite asking for two kinds of secrets, the current work demonstrates that participants produce a wide range of secrets in both preoccupying and nonpreoccupying conditions, and it is this range of secrets recalled that invites the contrast effect we demonstrate throughout the current paper.

In Studies 1 and 2, while the secrets recalled in the preoccupying (vs. nonpreoccupying) condition were more preoccupying (and increased preoccupation predicts increased judgments of hill slant), there was a contrast effect hiding a main effect of the recall prompt on judgments of hill slant. Asking a participant to recall an extremely preoccupying secret—if it is anything short of extremely preoccupying—can lead the recalled secret to pale in comparison to the prompt, making the participant’s own secret seem less preoccupying than it otherwise would feel (thereby contrasting from the intent of prompt). Studies 1–2 demonstrated this by accounting for an individual difference variable. Study 3, however, demonstrated this contrast effect with a manipulated variable, exposing participants to other people’s secrets. When this manipulation was in the same direction but more extreme than the prompt, this created a contrast effect (i.e., recalling preoccupying secrets ultimately reducing judgments of hill slant through participants contrasting away from the extreme exemplar, and thus in this case also the prompt). Yet, when participants were exposed to a prompt-opposing exemplar, this led participants who were asked to recall a preoccupying secret to feel that their secret was more preoccupying (i.e., by contrasting away from an extreme prompt-opposing exemplar, they assimilated toward the intent of the original prompt). Thus, while the recall manipulation can lead to a contrast effect, a second manipulation can re-anchor participants to feel that their secrets do fit the extremity of the prompt.

## Implications for Replications

**Implications for replications of the burdens of secrecy.** The effect of recalling “big” versus “small” secrets on perceptual judgments has been inconsistent in the literature. Slepian and colleagues (2012) and Goncalo and colleagues (2015) found an influence of recalling “big” versus “small” secrets on burden-consistent outcomes, but LeBel and Wilbur (2014); Pecher and colleagues (2015), and Slepian and colleagues (2015) found no influence of this manipulation. There are four issues to consider in making sense of these inconsistent results (the content of the

chosen independent variable, the experience of the independent variable within the context of the study, the use of manipulation checks, and sample size), and we argue that none of these issues cast doubt on the hypothesis that secrecy is burdensome, or has a relationship with perceptual judgments.

**Content of the independent variable.** One issue to consider with regard to inconsistent results of secrecy on perceptual judgments is the content of the independent variable. For instance, to interpret inconsistent results of the manipulation of recalling “big” versus “small” secrets, one must consider what content this manipulation produced. As already discussed, recalling “big” (vs. “small”) secrets does not consistently lead to the recall of secrets that are preoccupying and psychologically burdensome (Slepian et al., 2015, Studies 1–2). Before claiming secrecy does not influence perceptual judgments, researchers should be confident that specific secrecy content was reliably introduced in the study in the first place. Indeed, studies that have failed to find an effect of secrecy on perceptual judgments have used the “big” versus “small” manipulation, and have not assessed whether participants actually recalled the secrets they were prompted to (LeBel & Wilbur, 2014; Pecher et al., 2015).

**Experience of the independent variable.** In addition to the content of the independent variable being used, it is important to consider the experience the participant has with the independent variable within the context of the study. For instance, is the secret participants are asked to recall experienced as preoccupying? There has not been a single study that we are aware of that has failed to find a relationship between self-reported preoccupation with a secret and judgments of hill slant. In our work, this correlation is highly consistent and reliable (e.g., Slepian et al., 2012, Study 3; Slepian et al., 2015, Studies 1–4, and the current Studies 1–3). Unlike manipulations of secrecy, which the current work reveals has some inconsistency in predicting burden-consistent outcomes, individual difference measures of preoccupation have successfully predicted burden-consistent outcomes in every study that we are aware of.

First, consider preoccupation with secrets as an individual difference measure. The experience people show across this individual difference is that people who think frequently about their secret and feel particularly bothered and affected by it, find the world more challenging and forbidding. Rather than use a manipulation of secrecy, one could instead measure this continuous individual difference variable of preoccupation with secrets, which is both psychologically meaningful and consistently associated with burden-consistent outcomes. The advantage of this correlational approach is that it does not force a conceptual dichotomy of secrets that does not exist in the real world (i.e., there are not two categorical kinds of secrets). Of course, no causal claim can be made about a correlational relationship between preoccupation with secrets and burden-based outcomes without experiments, and third-variable criticisms could be made. For example, it could be that people who dispositionally judge the world as more challenging both see their secrets as more preoccupying, and physical space as more challenging to navigate. This is still consistent, however, with the theory that secrecy is burdensome: People who experience their secrets as preoccupying find the external environment as more challenging. A third variable explanation for the relationship between secrecy and perceptual judgments might invoke an effort-based mechanism linking the two. People whose experience is that

more effort is required to accomplish daily tasks might experience secrets as more challenging to keep (and thus preoccupying), and the environment as more challenging to navigate (and thus forbidding). Indeed, prior work suggests perceived effort expenditure as a variable that links preoccupation with secrets to judgments of the environment as forbidding (Slepian et al., 2015).

Second, consider the experience of being prompted to recall either an extremely preoccupying secret or an extremely nonpreoccupying secret. The present manipulation of preoccupation with secrets utilizes extreme wording to help separate the recalled secrets into a conceptual dichotomy that attempts to carve secrecy as its joints, but in reality there are no such joints. When searching for a secret that fits either prompt, the participant must compare the recalled secret to the extremely worded prompt (to see how well it fits), which can cause a contrast effect as people contrast from extremes. Prior researchers (the present ones included) did not consider the experience of trying to recall a secret that fits an extremely worded prompt. Being asked to recall a secret that fits a prompt necessitates comparing one's secret with the prompt, and thus comparing the secret with an extremely worded prompt creates the conditions known to invoke contrast effects (comparing to extreme exemplars or categories promotes contrast effects).

Experiments are important for establishing causality, and the current work offers a new experimental method to counteract unintentional contrast away from extremely worded recall prompts. We randomly assigned participants to recall preoccupying versus nonpreoccupying secrets, and then exposed participants to secrets representing extreme opposites to the manipulation prompt; this led participants to see their secret as consistent with the original manipulation prompt. In sum, one way forward is being aware of the limitation of the secret recall manipulation and how comparison processes can promote an unintentional contrast effect. Simply following the recall manipulation with prompt-opposing exemplars can eliminate the problem caused by extreme prompts.

**The importance of manipulation checks as a source of statistical power.** Why have prior studies on this topic found effects, whereas others have not? With a highly consistent effect of measured preoccupation with secrets predicting judgments of hill slant, we should expect studies that manipulate preoccupation with secrets to—at least sometimes—alter perceptual judgments. Increasing sample size increases statistical power, and thus  $p$  values  $< .05$  are more likely to be obtained for real effects with larger sample sizes, but still we should expect substantial variation in  $p$  values across repeated experiments (Halsey, Curran-Everett, Vowler, & Drummond, 2015). Sample size is not the only determinant of statistical power, however. Accounting for additional variance also increases statistical power. One simple way to account for variance in the current context is to measure whether participants recall secrets that fit the recall prompts. Yet, prior studies that purport to find no evidence that secrecy is burdensome have failed to assess whether preoccupying (vs. nonpreoccupying) secrets were recalled (i.e., they have not utilized manipulation checks; LeBel & Wilbur, 2014; Pecher et al., 2015). Claims to have found support for the null hypothesis that the postulated independent variable has no influence on the outcome of interest would be strengthened by actually demonstrating the postulated independent variable was manipulated in the first place. Replication attempts for any method would be more diagnostic if including a manipulation check.

**Sample size.** We argue that it is difficult to make meaningful nonreplication claims from a failure to find an effect on downstream judgments—when not confirming the postulated independent variable was manipulated. In the current domain, we argue that no meaningful claims about the lack of an effect of recalling secrets can be made without ensuring such a recall manipulation influenced secrecy processes. By simply asking participants to what extent they recalled a secret that fit the prompt, we revealed that many participants do not recall the kind of secret they were prompted to. Not only might participants fail to recall secrets that fit the prompt, the current studies demonstrated an even subtler aspect of this manipulation. By asking participants to recall a secret that fits an extremely worded prompt, the participant has to compare their recalled secret to the extremely worded prompt, and such a comparison invites a contrast effect. Revealing this contrast effect, however, requires measuring correspondence to the manipulation (which prior studies have not done), and also sufficient sample size.

Here, with a sample size of 2,000 (Studies 1 and 2), when not accounting for manipulation correspondence, we too “fail” to replicate the effect. Yet we also demonstrate that simply accounting for manipulation correspondence reveals a reliable relationship between secret recall and judgments of hill slant, overcoming the hidden contrast effect. It should be noted, however, that the current effects are small. For example, power analyses ( $\alpha = .05$ , power = 80%) reveal that 243 participants would be needed to find the effect of increased hill slant judgments from increasing correspondence with the preoccupying secret prompt ( $r = .179$ ), and 709 participants would be needed to find decreased hill slant judgments from increasing correspondence with the nonpreoccupying secret prompt ( $r = .105$ ).

Increasingly, researchers are becoming aware that large sample sizes are needed for even the most mundane and “obvious” research questions. For instance, a sample size of 94 participants is needed to detect that people who like eggs (vs. not) eat more egg salad, and 100 participants, recruited online, are needed to detect that men weigh more than women (Simmons, 2014). Thus, clearly a sample size of 100 participants is inadequate to make claims about the lack of an effect. Prior replication studies have used fairly small sample sizes. Larger sample sizes are needed, particularly when utilizing a manipulation that relies on participants recalling experiences that match a set of defined criteria as a manipulation.

A small effect size does not indicate that the current results are not meaningful. The observed effect size is limited by the ability to manipulate the postulated independent variable, which the current work demonstrates is limited without taking into account manipulation correspondence, or experimentally re-anchoring participants. The observed effect size is also limited by the ability to measure the dependent outcome. We do not think judgment of hill slant is the only interesting measure of experienced burden, but examine it here in the current work due to the recent interest in the relationship between secrecy and judgments of hill slant. Other work should examine other operationalizations of feeling burdened. Improved operationalizations that better capture the variables of interest should yield larger effect sizes to the extent that the current measures are noisy. In sum, when participants recall a secret that corresponds with the manipulation prompt, or if participants are experimentally reanchored to judge their secret as fitting

the prompt, recalling preoccupying (vs. nonpreoccupying) secrets increases a measure of finding the external environment as forbidding.

#### Implications for replications of other recall manipulations.

The current results demonstrate that prompts asking participants to recall personal experiences as a manipulation can have relatively complex results that do not fit a straightforward relationship wherein X leads to Y. The recall manipulation used in the current work is used widely in other domains. For example, research on the experience of power has manipulated power by asking participants to recall a time they experienced power or a lack of power (e.g., Galinsky, Gruenfeld, & Magee, 2003). This manipulation has been used extensively (see Galinsky et al., 2015 for a review). Several studies asked participants to recall a time that they were socially excluded versus included as a manipulation of social exclusion versus inclusion (Bernstein, Young, Brown, Sacco, & Claypool, 2008; Maner, DeWall, Baumeister, & Schaller, 2007; Mead, Baumeister, Stillman, Rawn, & Vohs, 2011; Pickett, Gardner, & Knowles, 2004). Attachment style has been manipulated by recalling experiences of secure, anxious, or ambivalent attachment (Chugh, Kern, Zhu, & Lee, 2014; Lee & Thompson, 2011). Creativity has been manipulated by recalling experiences of creativity (Sassenberg & Moskowitz, 2005). Promotion and prevention focus have been manipulated by recalling experiences of making progress toward, or being vigilant in avoiding, something (Hamstra, Van Yperen, Wisse, & Sassenberg, 2013; Higgins et al., 2001).

A host of many other goals and motivations have been manipulated by recalling experiences, including goals of romance versus intelligence (Park, Young, Eastwick, Troisi, & Streamer, 2015); being rejected versus ignored (Molden, Lucas, Gardner, Dean, & Knowles, 2009); succeeding versus failing (Leith et al., 2014); being moral versus immoral (Conway & Peetz, 2012; Jordan, Mullen, & Murnighan, 2011); complimented versus criticized (Stinson et al., 2010); in love versus happy (Maner, Rouby, & Gonzaga, 2008); tempted versus not tempted (Sheldon & Fishbach, 2015); helped versus were helped (Bohns & Flynn, 2010); affirmed for intrinsic versus extrinsic reasons (Gordon & Chen, 2010); experienced prejudice versus negative life experiences (Inzlicht & Kang, 2010); felt supported versus positive (Clark et al., 2011; Gillath, Sesko, Shaver, & Chun, 2010); felt authentic versus hubristic pride (Ashton-James & Tracy, 2012); felt elation, gladness, or joy (de Rivera, Possell, Verette, & Weiner, 1989); and so on. The preceding is just a short list of the many ways in which recall tasks have been used as manipulations.

These recall manipulations might have more complex outcomes than previously realized. That is, we might find that recall manipulations in other domains change in strength with increasing and decreasing manipulation correspondence. These manipulations might actually yield contrast effects under low manipulation correspondence. In examining effects of manipulation correspondence, it is important to consider when it should be measured. It may make sense to measure in between the independent and dependent measure, but only to the extent that doing so does not call attention the manipulated variable in a problematic way. In the current experiments, it did not seem to matter when manipulation correspondence was measured. In other domains, it may problematically call attention to what was intended to be a subtle manipulation. For example, exposure to risk-seeking exemplars has been found to induce a more risk-seeking response bias, but only when

attention is not called to the manipulation (Erb, Bioy, & Hilton, 2002). Thus, when the intention of a manipulation is that it subtly prompts a psychological process, it may be problematic to measure manipulation correspondence before the dependent measure. When the manipulation itself is not so secret (e.g., there is nothing subtle about asking participants to recall an experience of secrecy, of power, of ostracism and so on), then the measure of manipulation correspondence could go right after the independent measure. The most prudent approach may to manipulate when manipulation correspondence is measured to empirically examine whether it moderates any effects as we did in the current work.

Measuring and accounting for manipulation correspondence could allow for more precise tests of the effect of recall manipulations on downstream outcomes. Moreover, perhaps the “exemplar exposure” manipulation used in the current work could effectively strengthen other recall manipulations. When asking participants to recall an experience of power, creativity, morality, or intelligence, after having done so, the manipulation might be strengthened by presenting participants with another targets’ story of low power, creativity, morality, or intelligence. Asking participants to recall experiences as a manipulation is used extensively and widely in the psychological literature, but little work has examined how correspondence to these recall tasks might influence their results, or whether certain forms of these recall prompts might promote unintended contrast effects.

#### Implications for Contrast Effects

The current findings reveal that simply asking participants to recall a secret that fits a set of criteria can change how the participant feels about that secret. In other words, the current studies present an interesting case of the *observer effect*. The observer effect, identified in physics, is that the effect of measuring a variable has an influence on that actual variable. A classic example is measurement of pressure in a tire, where by checking the pressure, some air is released, thereby changing the pressure in the tire (i.e., you need to let out some of the air to insert the gauge).

By asking participants to recall a highly preoccupying secret (i.e., one that matches a set of extremely worded criteria), participants must search their memory for a similar secret, and necessarily compare it to the prompt to see how well it fits (where it likely fails to fully meet the extreme requirements of the prompt). This latter process of comparison to the prompt is unavoidable (i.e., it is what lets participants decide whether they have recalled the appropriate secret), but it can also lead to a contrast effect (i.e., comparison to extremes leads to contrast).

We show that because secrets exist on the continuum of preoccupation, and do not fall into these dichotomous extremes, distance from the prompts invites the contrast effect. And therefore, the manipulation (i.e., the experimentally introduced observation)—although it leads to the recall of preoccupying versus nonpreoccupying secrets—also leads to a change in the perceived burden of those secrets in a contrastive manner. Asking participants to recall a kind of secret can actually change how participants view that secret.

The current work presents a new kind of contrast effect, which diverges from the kinds of contrast effects we discussed at the outset of this paper. For example, one classic study (Herr et al., 1983) asked participants to judge the size animals. Preceding the text-based presentation of some of these animals were primes of animals that were

extremely small (flea, ant), or extremely big (elephant, whale). Participants contrasted away from extreme exemplars. For example, a pig was judged as big when previously exposed to the word *flea* and small when previously exposed to the word *whale*.

The contrast effect found in Studies 1 and 2 is similar in direction to these results, but with a notable difference—the lack of an actual exemplar against which to compare. When holding preoccupation with the recalled secret constant, being asked to recall a highly preoccupying secret led participants to feel less burdened by that secret, whereas being asked to recall a highly nonpreoccupying secret led participants to feel more burdened by that secret. Participants thus contrasted away from the extremely worded prompts. This is similar to judging a pig as big when exposed to the word *flea* and small when exposed to the word *whale*. Yet, in our Studies 1 and 2, there was no exemplar secret provided that could serve as a comparison anchor. Instead, the prompt itself, an introduced category, served as the anchor.

There are two possible mechanisms for this new kind of contrast effect. One possibility is that when participants are exposed to the introduced category, this then brings to mind some specific exemplar that meets the extreme criteria, and participants use this imagined exemplar as a comparison point. Alternatively, participants may actually contrast away from the category itself. Either of these explanations is possible, and they are not mutually exclusive (e.g., some participants could be calling to mind a concrete exemplar to fit the category, whereas others may only consider the category). Whether the proximal comparison point for our participants is some kind of inferred exemplar from the initial category activation, or actually the introduced category itself, this would not change the direction of the results (i.e., as long as the extreme comparison point is relevant it promotes contrast, independent of whether it is from an exemplar or a category; LeBoeuf & Estes, 2004). Likewise, whether participants see their own secret as ambiguous or clear, in terms of how preoccupying it is, would not change the direction of the results (i.e., extreme exemplars promote contrast in both cases; Herr et al., 1983).

In sum, by using extremely worded prompts (to separate participants upon the dimension of interest), participants have to compare their recalled secret to the prompt to see how well it fits. In many cases, it will fall short of the prompt (given that they are extremely worded). Correspondingly, comparison to the prompt invites a contrast effect. Study 3 experimentally verified this casual chain. The current contrast effect suggests more complex models of assimilation and contrast than do extant models, and new insights into the effects of asking participants to recall an experience, which can actually change how that experience is interpreted.

## Conclusion

The current research establishes the complexities and the systematic patterns that can govern the recall of secrets. Asking participants to recall their secrets as a manipulation allows researchers to study the effects of people's actual secrets. But unlike presenting participants with the same exact stimulus (vs. another) as a manipulation, the manipulation of thinking of one's secret through a recall paradigm relies on participants recalling their own experimental stimulus, which may or may not conform to the guidelines asked for by the researcher (e.g., a preoccupying vs. nonpreoccupying secret). As a result, this manipulation runs the risk that participants may not recall a secret that corresponds to the

recall prompt; that is, they may recall the wrong kind of secret or compare their secret with an extremely worded prompt and correspondingly contrast away from the prompt. Accounting for manipulation correspondence, or experimentally producing perceived manipulation correspondence, reveals that preoccupation with secrets increases a judgment of the environment as more forbidding and extreme.

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## Appendix

### Exemplar Secrets from Study 3

Secrets used for exposing participants to exemplars that re-assert extremity of recall prompts (i.e., showing extremely preoccupying secrets to participants recalling preoccupying secrets and showing extremely non-preoccupying secrets to participants recalling non-preoccupying secrets) and those used to re-anchor participants to judge their secret as fitting the prompt (i.e., showing nonpreoccupying secrets to participants recalling preoccupying secrets, and showing preoccupying secrets to participants recalling non-preoccupying secrets).

Secrets (preoccupying and non-preoccupying) are matched across ten content domains (money, sex, illegal drugs, issues of trust, breaking a law, having children, gambling, substance abuse, employment, and a childhood story), and matched for length; (that is, number of words [ $M_{\text{preoccupying}} = 53.7$  words,  $SD = 14.94$ ;  $M_{\text{non-preoccupying}} = 57.7$  words,  $SD = 15.56$ ;  $t(18) = 0.59$ ,  $p = .56$ ].

#### Preoccupying

**Money.** My secret is that I'm carrying a lot of credit card debt that my partner doesn't know about. It's into the 5 digits now and it's becoming really difficult to keep secret. Sometimes I am

struggling to just to make the minimum payments. It is very stressful to keep this secret from my partner.

**Sex.** I have been having an affair for 3 years now. I feel very guilty about this, and don't know what to do about it. It hangs over me all the time. I don't know if I should break it off, or if I should leave my partner for this other person.

**Illegal drugs.** My secret is that I'm addicted to cocaine. I've been doing it behind my partner's back for the last 2 years and she has no idea whatsoever. I feel extremely guilty about it, but I can't seem to give it up.

**Trust issue.** I have been secretly reading my partner's e-mails now for the past 6 months to see if my partner is cheating on me or is on any dating websites. I feel terrible every time I snoop like this, but I am concerned he might be cheating on me.

**Law breaking.** Before I met my partner, I was in jail for 4 months after leaving the scene of a crime. It was a hit and run, and I fled the scene. I am always worried that somehow my partner will find out it's on my record and that if she finds out the details of the hit and run, she will leave me.

**Children.** She does not know this yet, but I am sterile, and I have yet to tell her. She absolutely wants to have children, though. And I know she does not like the idea of adoption. I am extremely worried about what this will do to our relationship.

(Appendix continues)

**Gambling.** I have lost \$20,000 gambling on the Internet in the past 6 months. I have hidden this loss. My partner did not want me gambling anymore, and we cannot afford to lose this much money. We are pretty much living paycheck to paycheck, and I have been making it worse every month and have been hiding my gambling losses. It has gotten so bad that I was forced to go out and get a second job. I feel really bad because this is serious money I am wasting.

**Substance use.** I have an eating disorder and abuse laxatives after I go out to eat with my partner. This has been happening a lot now, and I am having trouble stopping. I worry if I stop using laxatives, I will gain weight and my partner will become less attracted to me.

**Employment.** I worked for the same employer for 5 years. I was laid off, but I have not told my partner about this and feel extremely guilty. When my partner thinks I'm at work, I'm actually at the library looking for new jobs. I don't know why I haven't told her yet, but everyday it gets even worse that I haven't told her that I was laid off.

**Childhood story.** I was sexually abused as a child. I have never told my partner about this. I worry it would change things somehow if I revealed this because we regularly see this family member who sexually abused me.

### Nonpreoccupying

**Money.** I sometimes buy little gifts for my niece when I think she deserves it, like for getting good grades. This is not something that bothers me, but I don't check in with my partner about this. I like that the gifts come from me.

**Sex.** I have only had sex with one other person before I was with my partner. It's not something I think much about, and I don't care about it either. I just don't think my partner knows how few people I've been with before, but I know it wouldn't make any difference if they found out.

**Illegal drugs.** I tried marijuana once in college. I think my partner thinks I've never tried it, but only because they have never asked. I know he has had it before back in college, so wouldn't think anything of it, or care at all that I had tried it once.

**Trust issue.** The only thing I can think of is that once I was using my partner's laptop and noticed his e-mail was open. I happened to glance at the e-mail and saw that my partner wrote to

a friend about meeting up for coffee. It was just an accident, and not a big deal, but I shouldn't have read it.

**Law breaking.** I almost got a ticket for having a taillight that was out. The cop told me that if I got it fixed, I wouldn't be ticketed. My partner doesn't know I got the taillight fixed to avoid getting a ticket, but she just thinks I was being responsible. It's a small little thing I didn't mention. She would probably laugh if I told her and just be happy that we didn't end up getting a ticket and that everything is fine.

**Children.** She does not know that in my previous marriage, I learned that I was unable to have children. We are too old now to have children anyway. Neither of us had kids in our prior marriages. We met when we were both in our 50s, and now we are in our 60s. We never wanted children together, since we are too old, so this actually doesn't really matter. I haven't thought to mention it yet because it doesn't really make any difference.

**Gambling.** I paid \$5 to enter the March Madness basketball office pool. I don't ever place bets of any kind, but I find the college basketball tournament fun, so I like to participate in the office pool. I didn't think to mention this to my partner just because it's such a small amount of money.

**Substance use.** The only thing I can think of that I haven't told my partner is that at one point, I used to have trouble sleeping and got prescribed insomnia medication. It was only during 1 year when I was in college. I don't have trouble sleeping any more though.

**Employment.** I have been looking for new jobs. I haven't told my partner that there have been a few jobs that might be good options, but I haven't applied to them because I am looking for a higher salary than my previous job. I don't know if she knows this, but I'm actually in a very good position because many people don't have my skill set, which is why I have been holding out for something better.

**Childhood story.** When I was a child, I stayed with my grandparents one month while my mom was working abroad. I never told my partner that story. It's the only thing I can think of that I have yet to share.

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