

# **Do Religious and Superstitious Worldviews Influence the Tempting Fate Effect? An Online Priming Experiment on MTurk**

## **Abstract:**

The unscientific belief that if one tempts fate, one has a higher likelihood of subsequent negative outcomes is both widespread and incompletely understood in existing literature. In this study, we aim to further elucidate the factors underlying the “tempting fate effect”, by performing a replication of the original Risen and Gilovich study (2008) and a more recent failed replication by Mathur and Frank (2016). We extend the original experiment by testing whether two unscientific worldviews (religion and superstition) can influence whether participants who come “unprepared for class” (tempting fate) believe they are more likely to be “called on by the teacher” (negative outcome). Thus, filling the gap in literature between the irrational tempting fate effect and unscientific worldviews. We perform this between-subject, scenario-based online study via Amazon Mechanical Turk (MTurk). Participants (n=242) were randomized to one of four conditions: a non-tempting fate condition without primes, a tempting fate condition without primes, tempting fate with religious priming, or tempting fate with superstitious priming. Participants were primed using an unscramble-the-words task. While we did not find significant evidence for the tempting fate itself nor for the impact of superstitious worldviews on the effect, results indicated that religious priming significantly lowered participants’ perceived likelihood of experiencing a negative outcome. This finding, though statistically underpowered, has valuable implications for policy, insurance, and marketing interventions wherein individuals overestimate their potential risk of negative outcomes such as resumption of travel in a relatively safe, post-pandemic period.

**Word Count: 241**

**Keywords:** tempting fate, religion, superstition, priming, worldviews

Original OSF page: <https://osf.io/sg3su/>

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**Total Word Count: 6279**

## I. Introduction

Many people who do not believe in fate are still afraid to tempt it. Some believe it is more likely to rain if they do not carry their umbrella while others believe that an extremely slow checkout line will speed up the minute they leave it (Miller & Taylor, 1995). This belief that tempting fate leads to a higher likelihood of negative outcomes is called the *tempting fate effect*. What is puzzling is that this belief persists when the action that tempts fate is causally-unrelated to the outcome, making it an irrational or unscientific belief. As an illustrative example, doing wheelies on a motorcycle increases the likelihood of a crash and therefore, would not demonstrate the irrational tempting fate effect. However, not wearing a helmet and perceiving an increased likelihood of crashing would demonstrate the tempting fate effect since there is no causal relationship between the two. Motivated by this fascinating observation, we seek to further elucidate the underlying cognitive mechanisms behind this unscientific belief through an online experiment.

To do this, we aim to replicate and extend a paper by Risen and Gilovich (2008) titled, “why people are reluctant to tempt fate.” The authors, in a series of six studies, found that the tempting fate effect does indeed exist and is driven by System 1 processes (as reference to dual-process theory by Kahneman (2011)). Specifically, most people reason (System 2) that there is no such thing as bad luck but they have a strong intuition (System 1) that bad things happen to those who tempt fate. The researchers found that suppressing System 2 with a cognitive load task increased the tempting fate effect. However, in 2016, Mathur and Frank failed to replicate the findings in an online experiment. In order to reconcile these different findings, we aimed to replicate the experiment while extending it beyond dual-process theory. Therefore, our first research question - a direct replication of the original paper - is *do participants irrationally believe that negative outcomes are more likely following a behavior that tempts fate?*

Research on the roots of unscientific beliefs by Boudry et al. (2015) found that such beliefs develop and spread due to certain unscientific worldviews and belief systems. Two such worldviews are *superstitious worldviews* and *religious worldviews*. By priming each of these we aim to find causal relationships explaining the tempting fate effect. While religiosity and superstition are both unscientific worldviews, they differ in their level of personal agency and individuality (Jarvis,

1980). While religions usually defer to a higher power to prescribe divine protection or divine punishment, superstition generally gives more agency to the individual. For example, knocking on wood to dispel bad luck or making a wish at 11:11 (Jarvis, 1980). While existing literature suggests that religious and superstitious worldviews may impact the extent to which people believe that negative outcomes are more likely after having tempted fate (Zhang et al., 2014; Zhang et al., 2020), no prior study has empirically tested the tempting fate effect with regards to these two worldviews. Therefore, we decided to examine the impact of both worldviews on the effect. Our second and third research questions are, respectively: *Does religious priming impact participants' perceptions of the likelihood of a negative outcome for having tempted fate?* and *Does superstition priming impact participants' perceptions of the likelihood of a negative outcome for having tempted fate?*

In order to answer these questions, we conducted an online experiment using a between-subjects design with four conditions via Amazon Mechanical Turk (MTurk). Two conditions - having performed or not having performed the ‘tempting fate’ behavior - were duplicated from the original paper (Risen & Gilovich, 2008) while our other two conditions received religious priming and superstitious priming, respectively. The methodology we adopted to test perceived likelihood of negative outcomes for having tempting fate stayed the same, with the differentiating factor being an implicit priming manipulation involving an unscramble-the-words exercise. We did not find evidence confirming the existence of the tempting fate effect, nor did we find evidence for the effect of superstitious priming on the perceived likelihood of negative outcomes. However, we did find that religious priming significantly decreases the perceived likelihood of experiencing a negative outcome. We acknowledge that our study does not have high statistical power and hence, future research on this effect is required to further understand the driving factors.

Real-world implications of a better understanding of the tempting fate effect include lessening psychological distress by dispelling irrational beliefs, increasing prosocial behaviors with targeted priming, policy implications of tackling free-riding, and decreasing fate-tempting behaviors such as foregoing insurance. The sections that follow discuss the relevant literature leading to our hypotheses, methodological considerations, primary and secondary results, and a discussion of the implications and limitations of our current study.

## II. Literature Review and Hypotheses

Existing literature offers relevant context on core terms informing our research questions and experimental design, including the tempting fate effect, religious and superstitious worldviews, and priming. We base our hypotheses on the gaps of knowledge identified.

### Tempting Fate Effect

Prior research has focused on risk-taking behaviors, such as tempting fate, as well as the factors that might mediate these behaviors. This includes research on gamblers who have been found to have maladaptive personality traits (Nigro et al., 2018), certain risky marketing decisions that could impact follower counts (Wang et al., 2020), or the framing of obesity as a form of tempting fate for its detrimental effects on long-term health (Bray, 1992). Nearly all of this research, however, has been focused on risky actions that have a direct causal relationship to outcomes.

In contrast, the mechanisms driving irrational, non-causal tempting fate beliefs are not well understood. While many studies have found that this irrational belief exists, the causal drivers are only recently being examined. As an example, in the domain of insurance, a study by Dolansky et al. (2011) found that participants estimated a higher probability of a prized possession breaking (outcome) when it is not insured (tempting fate behavior), however they didn't evaluate what drives this belief. Our paper seeks to fill this knowledge gap by understanding first, whether the tempting fate effect exists and then, to evaluate whether differing worldviews contribute to the unscientific tempting fate belief. Thus, our first null hypothesis to be evaluated, consistent with Risen and Gilovich's (2008) original study, is:

**H1:** Participants believe that negative outcomes are more likely following a behavior that tempts fate.

### Religious Worldview and Priming

Depending on people's level of religious identification (Shariff et al., 2016; Zhang et al., 2020), and specific beliefs such as whether God is punishing or forgiving (Johnson 2005; Shariff &

Norenzayan, 2011), levels of cheating and subsequent expectation of punishment can differ significantly (McKay et al., 2011; Randolph-Seng & Nielsen, 2007). Furthermore, some previous research supports the notion that religious priming will induce a greater fear of subsequent punishment for unscrupulous or unfair actions (Fergus & Rowatt, 2015; McKay et al., 2011). Other investigators have shown that religious priming can increase subjective anticipation of divine protection within perilous situations or after risky behavior (Holbrook et al., 2016; Kupor et al., 2015). There is, therefore, uncertainty in hypothesizing whether religious identification would increase (divine protection) or decrease (divine punishment) behavior that tempts fate. Given these findings, we put forward the following non-directional hypothesis:

**H2:** Religious priming impacts participants' perceptions of the likelihood of a negative outcome for having tempted fate.

*Religious priming* is frequently relied upon to manipulate the salience of religious thinking, and thereby test its causal relationship with potentially related psychological outcomes (Shariff et al., 2016). Specifically, *implicit religious priming* - usually elicited by unscrambling scrambled, religiously-themed sentences - is a commonly employed priming technique (Billingsley et al., 2018). By limiting conscious awareness of the relevant concept, implicit religious priming minimizes any potential experimental demand effects that would be more significant with explicit priming techniques (Weber & Cook, 1972). Hence, we use implicit priming techniques to determine the directionality of the impact of religiosity on the tempting fate effect.

### **Superstitious Worldview and Priming**

Existing literature is divided on whether superstitious beliefs make people more risk-averse or risk-seeking. Superstitions have been demonstrated to have a protective effect for the believer. In other words, engaging in superstitious activities are believed to “push away” bad luck, potentially making people more risk-seeking (Zhang et al., 2014). However, Chinchanachokchai et al. (2017) distinguish between passive and proactive superstitious beliefs. Passive superstition is when the individual lacks control of their luck while proactive superstition is when participants take charge of their luck. Each of these could lead to different risk preferences and, consequently, different beliefs about tempting fate. Pontes and Williams (2021) add to the ambiguity regarding whether

superstitious beliefs enhance or diminish the tempting fate effect through a gambling study where red is good luck for some and bad luck for others. We attempt to fill this gap of knowledge by elucidating whether superstitious worldviews enhance or diminish the tempting fate effect using implicit superstition primes and put forward the following non-directional hypothesis:

**H3:** Superstitious priming impacts participants' perceptions of the likelihood of a negative outcome for having tempted fate.

Unscientific thought processes can underpin superstitious worldviews (Boudry et al., 2015). Therefore, increasing the salience of a superstitious worldview could impact the tempting fate effect by influencing risk preferences. A study by Nguyen (2012) found that superstitious primes including words and phrases such as “lucky”, “destiny” and “meant to be” can significantly impact risk preferences. We primed our participants with similar words to test whether superstitious worldviews impact the tempting fate effect.

### **III. Methods**

#### **Design**

We conducted a pre-registered<sup>1</sup>, between-subject experimental study on MTurk. The current study is an extension of Mathur & Frank's (2016) online replication of Risen and Gilovich (2008)'s study to investigate alternative explanations for the tempting fate effect. We compared participants in situations where they had or had not tempted fate—i.e. had or had not come prepared for a class (see Appendix A for the scenarios). In addition, we tested if religious or superstitious primes influence the tempting fate effect. We based our experimental survey on the replication files shared by Mathur and Frank (2016) using Qualtrics to design our questionnaire.

#### **Conditions**

To answer our research questions, we randomly assigned participants to one of four conditions (Figure 1). In the first condition, Replication Control,<sup>2</sup> participants received no priming and were

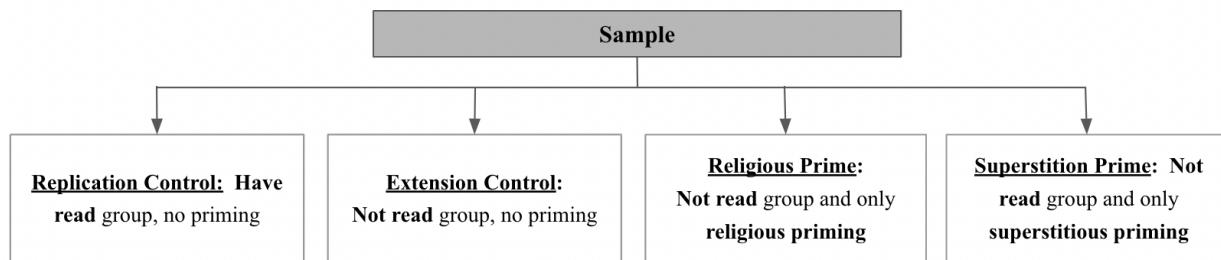
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<sup>1</sup> The pre-registration of our study can be found on <https://aspredicted.org/ib2u9.pdf>

<sup>2</sup> The Replication Control condition was previously referred to as the “Control” condition in the pre-registration. This was only a change made to the name of the condition for clarity for a lay audience. No changes to the experimental procedure were made for any of the conditions.

asked to imagine that they *had* completed the reading for class. The second condition was the Extension Control condition,<sup>3</sup> where participants received no priming and were asked to imagine that they *had not* done the reading for class (i.e., tempted fate). Both the Replication Control and Extension Control conditions were adapted from Risen and Gilovich (2008). Participants who did not receive a prime (Replication Control and Extension Control conditions) performed an unthemed, unscramble-the-words exercise (Appendix B).

The third condition was the Religious Prime,<sup>4</sup> where participants received implicit religious priming and were asked to imagine that they *had not* done the reading for class. The prime consisted of the validated, religiously-themed, unscramble-the-words exercise adapted from Billingsley et al. (2018) (Appendix C). The fourth condition was the Superstitious Prime,<sup>5</sup> where participants received superstitious priming and were asked to imagine that they had not done the reading for class. The prime consisted of an unscramble-the-words exercise containing superstition-related words adapted from Nguyen (2012) (Appendix D). Implicit priming techniques minimize experimenter demand effects that could be present with explicit priming alternatives (Weber & Cook, 1972).



**Figure 1.** Four experimental conditions

<sup>3</sup> The Extension Control condition was previously referred to as the “Treatment 1 (T1)” condition in the pre-registration.

<sup>4</sup> The Religious Prime condition is the “T2” condition in the pre-registration.

<sup>5</sup> The Superstitious Prime condition is the “T3” condition in the pre-registration.

## Sample

### *Sample Size Estimation*

We planned to collect data from at least 200 participants (i.e., 50 participants per condition) to detect an effect size of  $d=0.58^6$  with 80% power<sup>7</sup> at a significance level of 0.05. This estimated sample size was based on an a priori power analysis<sup>8</sup> for the Wilcoxon Rank Sum test conducted on G\*Power (Appendix E). However, we took into account our funding of \$100 for the entire study, and therefore aimed to collect as many responses as the funding permitted. We used an hourly rate of \$6 and an estimated completion time of 8 minutes<sup>9</sup> when recruiting participants in the actual data collection<sup>10</sup>.

### *Actual Sample: Sample Characteristics*

A total of 276 participants were recruited via MTurk and provided with monetary incentives for participation. Participants above the age of 18 years and United States residents were included in the study. Participants were provided experiment details and then asked to acknowledge their informed consent to participate (Appendix F). Additionally, all participants were required to complete an automated bot test; those who failed were excluded (Appendix G). As a benefit of the MTurk platform, enrolled participants could complete the survey only once, preventing multiple submissions.

Enrolled participants were randomly assigned to one of four conditions and required to respond to every question dictated by the ‘force response’ option on Qualtrics. Those who failed either of our two attention checks (Appendix H) were excluded from data analysis. Participants who took more than 60 minutes or less than 30 seconds to complete the survey were excluded. Upon enforcing

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<sup>6</sup> It is important to note that within the field of psychological research, this effect size seems quite large (Fritz et al., 2012) and thus might be an overestimate of the true effect.

<sup>7</sup> Cohen (1992) recommended 80% power as a convention for general use and suggested that a value smaller than 80% would incur a risk of a Type II error.

<sup>8</sup> We used the means and standard deviations from our pilot data collection (or pre-test) for the a priori power analysis. Pilot data collection was carried out from 5-16 November 2021, with the aim of testing our survey and obtaining feedback on our survey instructions. We only used the Extension Control and Superstitious Prime conditions in the pilot data collection. A total of 62 participants were recruited from our personal networks through online channels. They were randomly assigned into the Extension Control ( $n=33$ ,  $M=4.57575$ ,  $SD=2.33225$ , Median=5) or Superstitious Prime ( $n=29$ ,  $M=2.96551$ ,  $SD=2.39765$ , Median=2) conditions.

<sup>9</sup> This was based on the average completion time in our pilot data collection.

<sup>10</sup> Data collection was carried out on 19 November 2021 on MTurk.

these exclusion criteria, we had data from 242 participants (Replication Control: n=74, Extension Control: n=56, Religious Prime: n=53, Superstitious Prime: n=59; further details are presented in the Results section).

The mean age of the sample was 37.28 years old ( $SD=11.99$  years old). The distribution of age for each condition is presented in Table 1.

Age	Replication	Extension	Religious	Superstitious	All Groups
	Control	Control	Prime	Prime	
Mean	36.72	37.20	39.09	36.40	37.28
Median	34	35	36	33	34
SD	11.34	11.69	13.87	11.34	11.99

**Table 1.** Age composition by condition

A larger proportion of the sample was male (57.85%). The distribution of gender for each condition<sup>11</sup> is presented in Table 2.

Gender	Replication	Extension	Religious	Superstitious	All Groups
	Control	Control	Prime	Prime	
Female	32	27	19	23	101
Male	42	28	34	36	140
Other	0	1	0	0	1
Count	74	55	53	59	242

**Table 2.** Gender composition by condition

The majority of participants had a religious affiliation (73.55%). The distribution of religion for each condition is presented in Table 3.

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<sup>11</sup> None of our participants answered “Refuse to answer” for the question on gender.

Religion	Replication Control	Extension Control	Religious Prime	Superstitious Prime	All Groups
Theist <sup>12</sup>	61	40	38	39	178
Non-theist <sup>13</sup>	13	16	15	20	64
Count	74	55	53	59	242

**Table 3.** Religious affiliation by condition

## Measures

### ***Perceived Likelihood of a Negative Outcome for Having Tempted Fate***

The outcome variable in this experiment is the perceived likelihood of a negative outcome for having tempted fate. Participants were asked to rate “*How likely do you believe it is that the professor will call on you?*” on a scale from 0 (not at all likely) to 10 (extremely likely). This question was preceded by a scenario in which participants were asked to imagine themselves in a large lecture where the professor asked them a question about the assigned readings. Depending on the treatment condition, they either had done the readings (Replication Control) or not (Extension Control, Religious Prime, and Superstitious Prime). This measure was taken directly from Risen and Gilovich’s (2008) study.

### ***Religiosity***

Participants’ level of religiosity was a covariate measured in the post-intervention survey using the General Religiousness Scale (GRS) adapted from Rowatt et al. (2009). The scale consisted of four items, with multiple possible responses (4- to 9-point Likert scales depending on the item). The complete list of items can be found in Appendix I. Due to the multiple Likert scales used, we standardized the religiosity score using Z scores, a common method for standardizing variables

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<sup>12</sup> This consisted of participants who identified as Christian, Buddhist, Jewish, Muslim, or Hindu. None of the participants answered “Other”.

<sup>13</sup> This consists of people who were atheist, agnostic, answered “don’t know”, or refused to answer the question on religion.

measured on different scales (Diez et al., 2019). To do this for each participant, we calculated the Z scores for each of the four GRS items and subsequently calculated an average.

### ***Superstition/Superstitious Beliefs***

Participants' level of superstitiousness was a covariate measured in the post-intervention survey. It was operationalized using the Belief in Good Luck (BIGL) scale adapted from Darke and Freedman (1997). The scale consisted of 14 items, and participants had to indicate the extent of their agreement using a 6-point Likert scale (1 = strongly disagree, 6 = strongly agree). The complete list of items can be found in Appendix J. Items 5, 8, and 14 of the BIGL were reverse-scored; therefore, reverse-coding transformation was applied to these items. All items were summed into a total BIGL score that was used for analysis.

### ***Demographic Information***

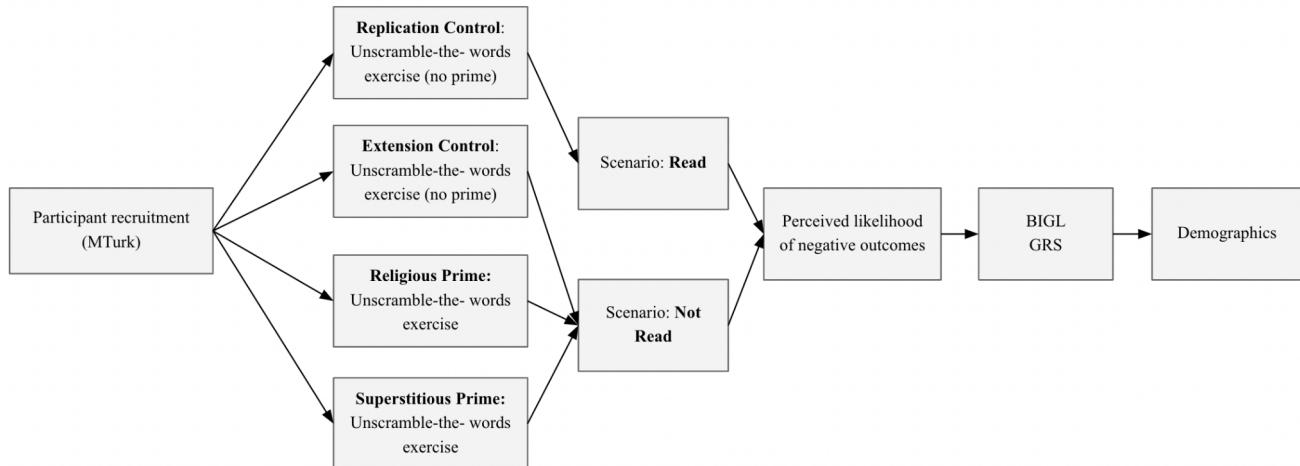
Age, gender, and religious affiliation were control variables. Age was measured in years. Participants had to indicate their gender by choosing Male, Female, Other, or Refuse to answer. Participants indicated their religion by choosing from 10 options: Christian, Jewish, Muslim, Hindu, Buddhist, Atheist, Agnostic, Other, Don't know, and Refuse to answer. In our analysis, we aggregated responses into two groups: theist (those who identified as having a religion) and non-theist (atheist, agnostic, refuse to answer, don't know). Demographic information was asked in the post-intervention survey (Appendix K).

### **Procedure**

Participants were recruited through MTurk and randomly assigned to one of four conditions (Figure 2) using the 'Randomizer' function on Qualtrics. As per our blinding strategy, participants were not informed of the other treatments being administered as it would have interfered with the prime and affect the results of the experiment. Participants first completed a priming manipulation (i.e., the unscramble-the-words exercise) according to their assigned condition. Each unscramble-the-words exercise contained 10 sentences to complete. Next, participants read a scenario which asked them to imagine themselves in a large lecture where the professor asks them a question about assigned readings (see Appendix A for the scenarios). Participants in the 'Replication Control' condition were told to imagine that they had completed the reading for the class, while participants in the other conditions were asked to imagine that they had not. After that, they were asked to

indicate how likely they believed that the professor would call on them on a scale from 0 (not at all likely) to 10 (extremely likely).

Participants were subsequently presented with a post-intervention survey with questions to measure their level of religiosity and superstition, as well as age, gender, and religion.



**Figure 2.** Experimental design

### Analytic Plan

The statistical analyses was conducted in R and G\*Power. Base, tidyverse, readxl, ggpibr, effsize, knitr, tinytex, ggplot2, sjPlot, sjmisc, sjlabelled, and rstatix packages were used in R. Descriptive statistics (i.e., mean, median, standard deviations) were conducted for all variables in R. Main analysis was conducted using Wilcoxon Rank Sum Test and regression analysis, while secondary analysis was conducted using regression analysis with interaction. Power calculations were performed on G\*Power.

### Main Analysis

Due to the inability to guarantee assumptions of normality, we conducted non-parametric tests: three Wilcoxon Rank Sum Tests, each comparing medians between two groups, to test H1, H2, and H3.

We also conducted a robustness check predicting the perceived likelihood of being called on with treatment dummies. These dummies indicated whether the participant had completed the readings for class and whether they received a religious or superstitious prime. We also controlled for

gender, age, religion/level of religiosity,<sup>14</sup> and superstition/superstitious beliefs. The goal of this analysis was to check whether the perceived likelihood of being called on was influenced by any other variables apart from the experimental manipulation. The regression model is shown below:

$$PL = \beta_0 + \beta_1(\text{Have read or not}) + \beta_2(\text{Religious prime}) + \beta_3(\text{Superstitious prime}) + \beta_4(\text{GRS score}) + \beta_5(\text{BIGL score}) + \beta_6(\text{Age}) + \beta_7(\text{Female}) + \beta_8(\text{Other gender}) + \beta_9(\text{Theist})$$

Where:

**PL** = Perceived likelihood of being called on

**$\beta_0$**  = Intercept coefficient

**$\beta_1, \beta_2, \beta_3, \beta_4, \beta_5, \beta_6, \beta_7, \beta_8, \beta_9$**  = Slope coefficient for each independent variable

**Have read or not** = Have read or not dummy variable (1 = have read, 0 = have not read)

**Religious prime** = Religious prime dummy variable (1 = received religious priming, 0 = did not receive religious priming)

**Superstitious prime** = Superstitious prime dummy variable (1 = received superstitious priming, 0 = did not receive superstitious priming)

**GRS score** = Standardized religiosity/GRS score

**BIGL score** = Total BIGL score

**Age** = Age (in years)

**Female** = Female dummy variable (1 = Female, 0 = Not female)

**Theist** = Theist dummy variable (1 = Theist, 0 = Non-theist)

### ***Secondary Analysis***

As outlined in our pre-registration, we suspected that participants' level of religiosity and superstition could alter the effects of our primes on the outcomes (i.e., religiosity/superstitious beliefs impact the effectiveness of the religious/superstition primes). We tested this via a regression analysis using an interaction term:

$$PL = \beta_0 + \beta_1(\text{Have read or not}) + \beta_2(\text{Religious prime}) + \beta_3(\text{Superstitious prime}) + \beta_4(\text{GRS score}) + \beta_5(\text{BIGL score}) + \beta_6(\text{Age}) + \beta_7(\text{Female}) + \beta_8(\text{Other gender}) + \beta_9(\text{Theist}) + \beta_{10}(\text{Religious prime} * \text{GRS score}) + \beta_{11}(\text{Superstitious prime} * \text{BIGL score})$$

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<sup>14</sup> Religion and level of religiosity was referred to as "religiosity" in the pre-registration.

Where:

**PL** = Perceived likelihood of being called on

**$\beta_0$**  = Intercept coefficient

**$\beta_1, \beta_2, \beta_3, \beta_4, \beta_5, \beta_6, \beta_7, \beta_8, \beta_9, \beta_{10}, \beta_{11}$**  = Slope coefficient for each independent variable

**Have read or not** = Have read or not dummy variable (1 = have read, 0 = have not read)

**Religious prime** = Religious prime dummy variable (1 = received religious priming, 0 = did not receive religious priming)

**Superstitious prime** = Superstitious prime dummy variable (1 = received superstitious priming, 0 = did not receive superstitious priming)

**GRS score** = Standardized religiosity/GRS score

**BIGL score** = Total BIGL score

**Age** = Age (in years)

**Female** = Female dummy variable (1 = Female, 0 = Not female)

**Theist** = Theist dummy variable (1 = Theist, 0 = Non-theist)

**Religious prime\*GRS score** = Interaction term between religious prime (1 = received religious priming, 0 = did not receive religious priming) and GRS score

**Superstitious prime\*BIGL score** = Interaction term between superstitious prime (1 = received superstitious priming, 0 = did not receive superstitious priming) and BIGL score

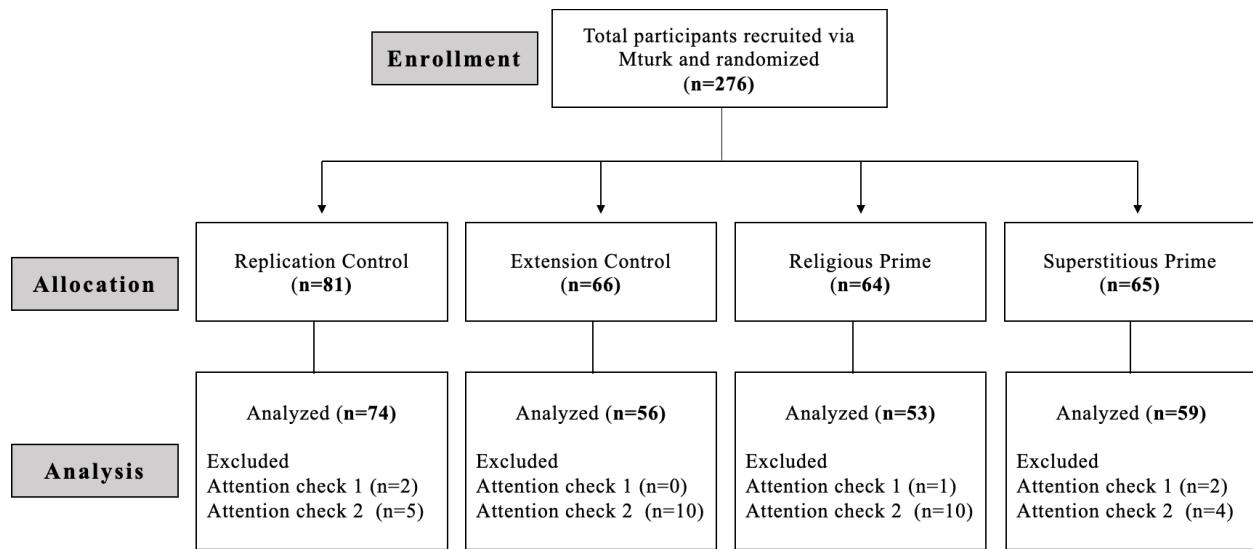
## IV. Results

### Descriptive Statistics

276 participants completed this study on MTurk. They were randomly assigned to one of four conditions. Forced-choice responses were used in the Qualtrics survey, so the study response rate was 100 percent. However, n=34 participants were excluded when constructing the analytic sample because they failed at least one of the two attention checks.<sup>15</sup> A breakdown of participants included in our analysis by condition is displayed in Figure 3.

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<sup>15</sup> Five participants failed the first attention check, and 32 failed the second attention check. Three people failed both, so there are 34 unique participants excluded.



**Figure 3.** Consort diagram detailing data collected, excluded, and analyzed by condition

The overall average perceived likelihood of being called on in class was 5.3 (on a scale of 10;  $SD=3.1$ ). Descriptive statistics of the perceived likelihood of a negative outcome for having tempted fate by condition are shown in Table 4<sup>16</sup>.

	Replication Control	Extension Control	Religious Prime	Superstitious Prime	All Groups
Perceived likelihood	Mean	5.7	5.7	4.4	5.2
	Median	6	7	3	7
	SD	3.0	2.9	3.3	3.1
	Count	56	74	53	59
					242

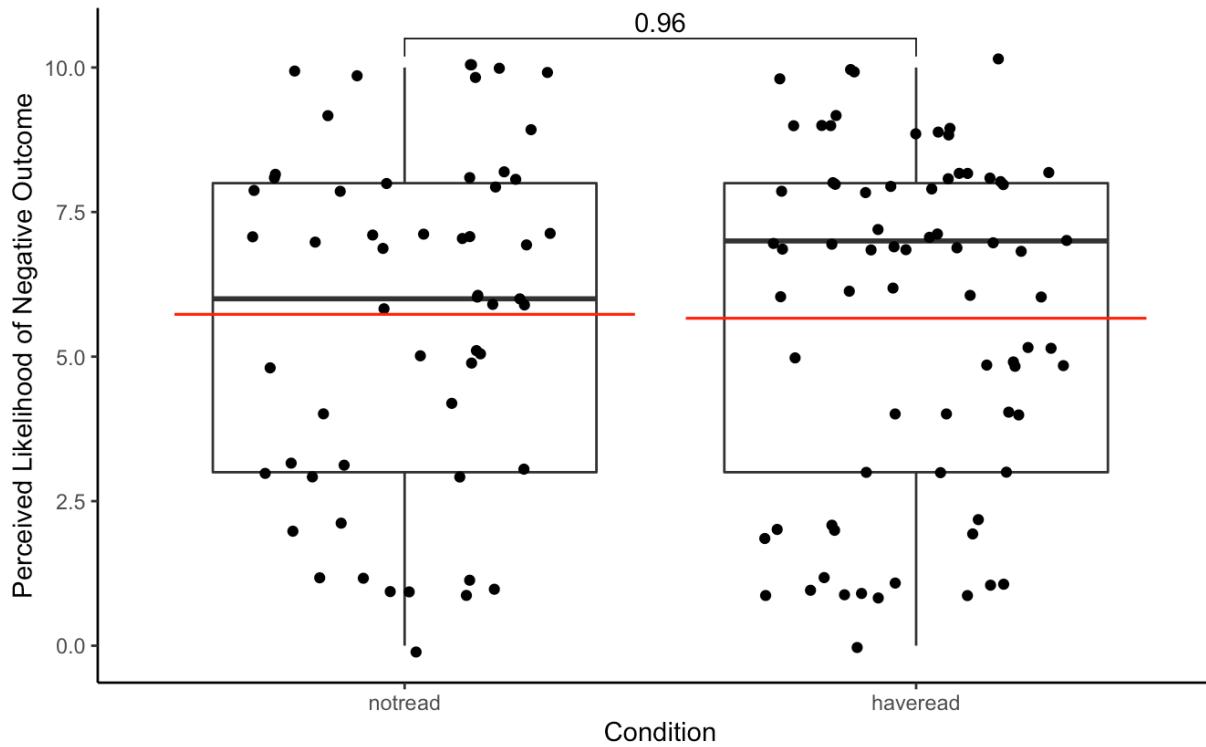
**Table 4:** Summary statistics of perceived likelihood of negative outcome by condition

## Primary Analyses

To investigate if the tempting fate effect exists, we tested our first research question—whether ‘tempts fate’ behavior increases participants’ perceived likelihood of negative outcomes. Our expectation was that when participants were told they had not done the reading for class (i.e.,

<sup>16</sup> Descriptive statistics of the participants’ level of religiosity and superstitious beliefs can be found in Appendix L.

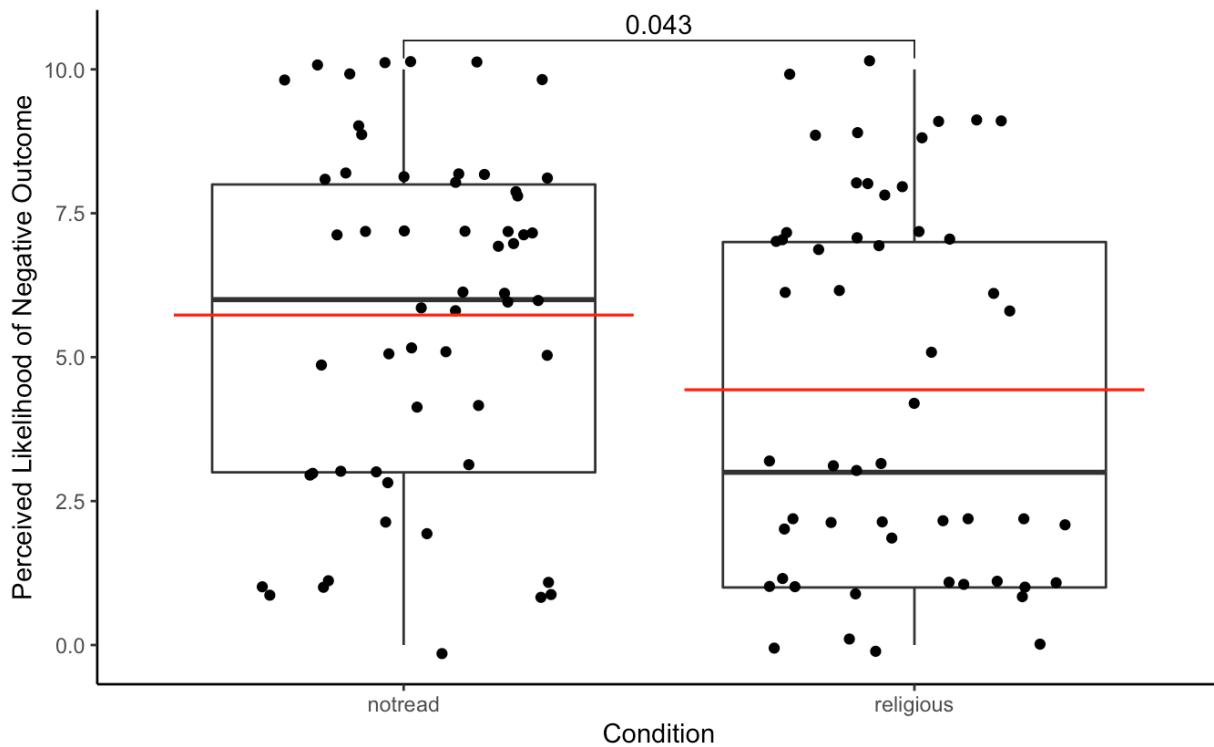
Extension Control condition) they would perceive a greater likelihood of being called on by the teacher than those who had done the reading (i.e., Replication Control condition). The median perceived likelihood score in the Extension Control condition was 6 ( $IQR=5$ ), whereas the median in the Replication Control condition was 7 ( $IQR=5$ ). A two-tailed Wilcoxon Rank Sum test revealed that the difference between these groups was not significant ( $p=0.9641$ ;  $CI = -1.0000739$  -  $0.9999315$ ) (Figure 4). By finding no evidence of the tempting fate effect, our results do not support H1 and consequently fail to replicate the original study's findings.



**Figure 4.** Results of the Wilcoxon Rank Sum test comparing the Extension Control (notread) and the Replication Control (haveread) conditions. Error bars represent 95% confidence intervals.

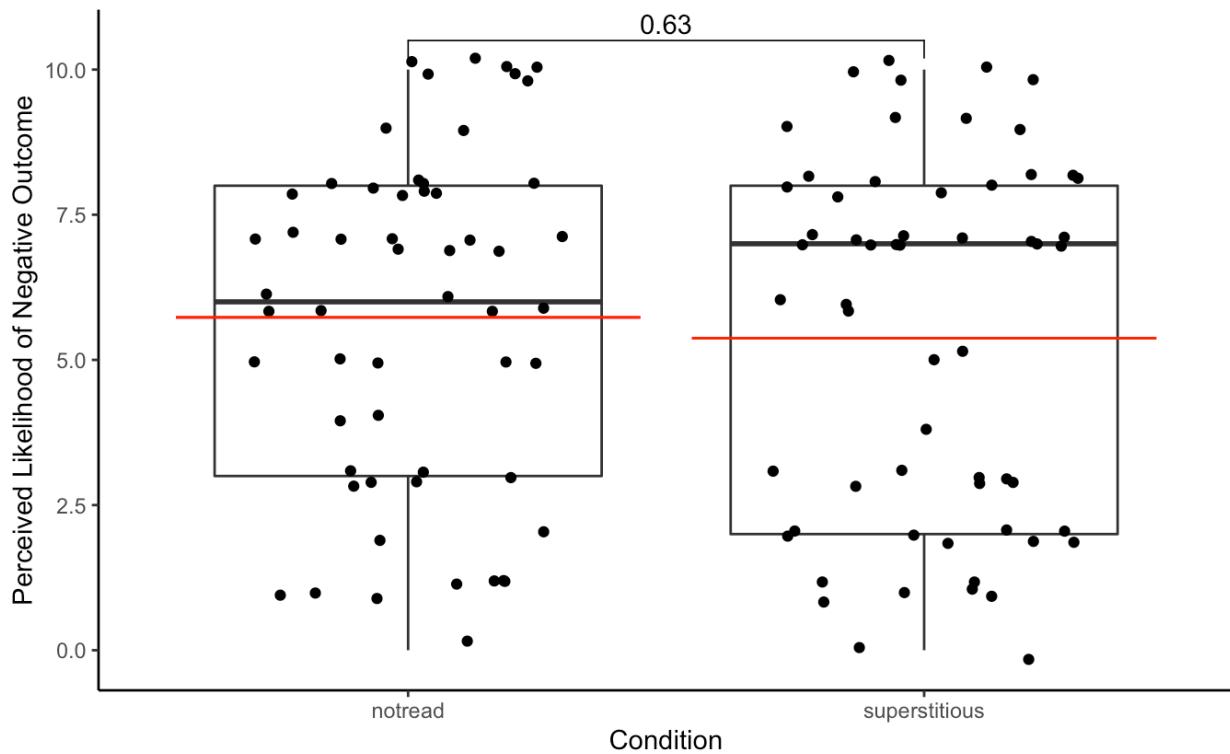
For our second and third hypotheses, we investigated the respective effects of either religious or superstitious priming on participants' perceived likelihood of negative consequences for tempting fate. As mentioned in our introduction, we predicted that the primes would increase the salience of unscientific beliefs and thereby impact the tempting fate effect. We expected that the respective perceived likelihood scores of the two priming conditions would significantly differ from the Extension Control condition.

To test our second hypothesis, we performed a two-tailed Wilcoxon Rank Sum test to investigate whether the median of the Religious Prime condition ( $median=3; IQR=6$ ) differed significantly from the Extension Control condition. The results (Figure 5) indicate a significant impact of religious priming on the perceived likelihood of a negative outcome ( $p=0.0433; CI = 0.000009 - 2.999999$ ). Participants who received a religious prime were significantly less likely to believe they would be called on than those who did not receive a prime.



**Figure 5.** Results of the Wilcoxon Rank Sum test comparing the Extension Control (notread) and the Religious Prime (religious) conditions. Error bars represent 95% confidence intervals.

To test our third hypothesis, we performed a two-tailed Wilcoxon Rank Sum test to investigate if the median of the Superstitious Prime condition ( $median=7, IQR=6$ ) differed from the Extension Control condition. These results (Figure 6) indicate no significant impact of priming superstitious worldviews on participants' perceived likelihood of a negative outcome compared to the Extension Control condition ( $p=0.6297; CI = -0.9999956 - 1.0000459$ ). Table 5 presents a summary of the three Wilcoxon Rank Sum tests.



**Figure 6.** Results of the Wilcoxon Rank Sum test comparing the Extension Control (notread) and the Superstitious Prime (superstitious) conditions. Error bars represent 95% confidence intervals.

Hypotheses	p-value
H1: Replication Control vs. Extension Control	0.9641
H2: Religious Prime vs. Extension Control	0.0433*
H3: Superstitious Prime vs. Extension Control	0.6297

\*p<0.05

**Table 5.** Summary of Results by Hypotheses based on two-tailed Wilcoxon Rank Sum Tests

Next, a multiple regression analysis was performed as a robustness check for our Wilcoxon Rank Sum tests. Our regression tested if having read or not having read for class and the presence of a prime significantly impacted participants' perception of the likelihood of a negative outcome, while controlling for a participants' religious and superstitious beliefs scores, age, gender, and

religious affiliation. The results of the regression indicated that 46.6% of the variance can be explained by the model and that the model was a significant predictor of perceived likelihood,  $F(9,232) = 22.4$ ,  $p < 0.001$ . The regression coefficients of Religious Prime ( $\beta = -1.1377150$ ,  $p = 0.011$ ), BIGL score ( $\beta = 0.1378967$ ,  $p < 0.001$ ), and religion ( $\beta = 1.1879616$ ,  $p = 0.017$ ) showed significance. The remaining variables were all insignificant (Table 6). The significance of the religious prime is consistent with the equivalent Wilcoxon Rank Sum test. Participants in the Religious Prime group reported they were less likely to be called on than those who did not receive the prime. Additionally, we found that higher levels of superstition—indicated by a high BIGL score—the more likely a participant believed a negative outcome would result from tempting fate. Finally, the perceived likelihood was higher for a participant who identified with one of the five listed organized religions compared to those who did not. The remaining results from the other controls can be found in the table below.

<i>Predictors</i>	<b>likelihood</b>		
	<i>Estimates</i>	<i>CI</i>	<i>p</i>
Intercept	-1.96 (0.81)	-3.54 – -0.37	<b>0.016</b>
Have read or not	-0.54 (0.41)	-1.35 – 0.27	0.191
Superstitious Prime	-0.29 (0.43)	-1.15 – 0.56	0.496
Religious Prime	-1.14 (0.44)	-2.01 – -0.26	<b>0.011</b>
GRS Score	-0.15 (0.26)	-0.67 – 0.37	0.573
BIGL Score	0.14 (0.01)	0.11 – 0.17	<b>&lt;0.001</b>
Age	0.00 (0.00)	-0.00 – 0.00	0.250
Gender(Female)	0.53 (0.30)	-0.06 – 1.13	0.078
Gender(Other)	-3.68 (2.33)	-8.28 – 0.92	0.116
Theist	1.19 (0.49)	0.22 – 2.16	<b>0.017</b>
Observations	242		
R <sup>2</sup> / R <sup>2</sup> adjusted	0.466 / 0.445		

**Table 6.** Results from the regression analysis for robustness check. Standard errors are in parentheses. Dependent variable was the perceived likelihood of a negative outcome. Have read or not, Superstitious Prime, and Religious Prime are dummy variables for the four conditions.

### Secondary Analysis

We ran a regression model testing for interaction effects to know whether a person's superstitious beliefs (indicated by the BIGL score) impacted the effectiveness of the superstitious prime - as well as whether a person's level of religiosity (indicated by the GRS score) impacted the effectiveness of the religious prime. However, our results found no significance for either interaction (Appendix M).

## **Reliability of Data**

Post-hoc power analysis for the two-tailed Wilcoxon Rank Sum Test was conducted on G\*Power using data from 242 participants (Appendix N). Given the sample sizes, effect sizes, and alpha level of 0.5, we achieved 5% power for the test comparing the Replication Control and Extension Control conditions (Hypothesis 1), 55% power for the comparison of the Extension Control and Religious Prime conditions (Hypothesis 2), and 9% power for the comparison of the Religious Prime and Superstitious Prime conditions (Hypothesis 3). These results suggest that our study was underpowered. Of the 276 participants recruited from Mturk, 34 participants were excluded due to failing the two attention checks; excluding nearly 10 percent of the enrolled participants left us with 242 observations for analysis. The decreased sample size was a potential contributing factor as to why this study was ultimately underpowered, thus making our results unreliable.

## **V. Discussion and Conclusion**

The original study by Risen and Gilovich (2008) found significant evidence for the tempting fate effect. Based on this insight, and in an attempt to replicate this original finding, we hypothesized that participants in the Extension Control condition would rate negative outcomes as being more likely to occur following a behavior that tempts fate compared with participants in the Replication Control condition. However, our experiment did not find evidence to support this hypothesis; that is, we did not observe the existence of an overall tempting fate effect. We do not find this result surprising given that replications of the original research failed to find evidence for the tempting fate effect. It is also important to consider these results in light of our study being underpowered with a small sample size due to resource constraints. Larger sample sizes are more likely to magnify the detection of differences in effect sizes; a statistically significant result alone might have little to no practical significance (Faber & Fonseca, 2014). Therefore, while we do call into question whether the tempting fate effect actually exists as a general psychological phenomenon, more research is needed to draw a firm conclusion.

In our two additional priming conditions—Religious Prime and Superstitious Prime—we wished to determine whether unscientific worldviews underpin the tempting fate effect by priming these

worldviews and examining the impact on the perceptions of negative outcomes. By comparing the respective perceived likelihoods of a negative outcome from these two treatment conditions with the Extension Control condition, we determined whether and how these worldviews could affect participants' risk-assessment of negative outcomes after having tempted fate. Our results demonstrate that participants who received religious primes perceived a significantly lower likelihood of a negative outcome after having tempted fate than in the non-primed condition. No effect was observed for the superstitious prime. The significant result for the religious prime is in agreement with findings by Holbrook et al. (2016) and Kupor et al. (2015), which suggest that religious priming can induce anticipation of divine protection after engaging in risk-taking behavior. However, our findings contrast those of Fergus & Rowatt (2015), that religious priming induces a greater fear for punishment after unscrupulous actions. Since the current experiment was not designed to tease out the individual nor combined effects for these two theories, future research may dig further to understand whether the two effects coexist and counteract with each other. The null result observed in the superstitious prime group contradicts previous research that superstitious words could impact individuals' risk assessments. However, though we based our superstitious primes on previously-validated religious priming methods, the former has not been specifically validated in the literature. Due to this fact, as well as the underpowered current study, we cannot make any definitive conclusions regarding the effect of superstitious primes. Rather, we believe future research is needed to validate the use of superstitious prime in studying decision making.

While our superstitious prime did not show a significant impact on participants' perceived likelihood of negative outcomes, we did find that an individual's personal superstitious beliefs - as measured by the *Belief in Good Luck* (BIGL) scale - significantly impacted perceived likelihood of a negative outcome for tempting fate. This is supported by existing literature, which suggests that superstitions are related to risk aversion and the perceived likelihood of negative events following tempting fate (Zhang et al., 2014). It therefore makes sense that holding superstitious beliefs could lead to higher expectations of a negative outcome. Future research could examine the respective impacts of negative vs positive superstitious beliefs - as well as engagement in superstitious rituals in pursuit of good luck - on individuals' perceived likelihood of negative outcomes.

In addition to this, another interesting finding that came out of our analysis was that participants who identified with one of the five listed religions, had a higher perceived likelihood of negative outcomes when compared to those who did not. This finding may signal an additional impact of religious beliefs on the tempting fate effect, though opposite of the religious prime impact. Beliefs such as whether God is authoritative/punishing or benevolent/forgiving can affect how people approach risky behavior (Shariff et al., 2016; Zhang et al., 2020). Future studies could further delineate the complex underlying mechanisms behind the effects of religious primes and personal religious beliefs on perceived likelihoods of negative outcomes.

In terms of policy applications, a better understanding of the factors, like religious priming and superstitious beliefs, contributing to the tempting fate effect could be directly applied to influencing behaviors that involve individual risk calculations. These may include people's judgment of how actions today affect negative outcomes in the future, such as insurance purchasing (Lyu & Barré, 2017) or health-related behaviors (Anderson & Mellor, 2008). These are especially relevant in the context of the COVID-19 pandemic, where, both the idea of tempting fate by venturing out in public spaces and yet, avoiding the virus, can explain how the tempting fate effect influences risk calculations in participants. If we define our study's tempting fate behavior - relying on others' preparedness for class - as free-riding, we can apply our findings to the greater policy implications of free-riding behavior especially in terms of free-riding COVID-19 vaccinations by relying on the preparedness of others. Given the societal costs of free-riding behavior (Albanese & Van Fleet, 1985; Armstrong, 2016), by better understanding the factors involved in the risk assessment of such behaviors, we might be able to modulate the behaviors themselves, thereby increasing prosocial behaviors (Shariff et al., 2016).

Furthermore, a better understanding of the amplitude and mechanisms underpinning this tempting fate effect - whether related to superstitious/religious beliefs or not - could be used to inform marketing and policy decisions within a post-pandemic world. For instance, using religious priming focused on protection from divinity or a higher power could be a useful way to encourage individuals to invest, trust, and engage in desired practices and behaviors as economies and businesses attempt to rebuild.

## VI. Limitations

Although our study has valuable implications, it is not without limitations. First, as per the analysis using our pilot data, we had projected a sample of about 200 participants to achieve 80% power in our final experiment with 50 participants per group. However, despite having more than 50 participants in each of the conditions, our post-hoc power analysis determined that our study was underpowered at a significance level of 0.5. As such, there may have been qualitative differences between our pilot sample and our experimental Mturk population of professional survey-takers that could have led to both the effect size discrepancy and post-hoc underpowering. Additionally, we excluded 34 participants from our data analysis due to their failure to pass the mid-survey attention checks, which brought our overall sample size down from 276 to 242 participants.

Second, while the unscramble-the-words exercise used for the religious prime condition and the control conditions was directly borrowed from research by Billingsley et al. (2018), the superstitious prime was based on research by Nguyen (2012) and adapted to be similar to the unscramble-the-words exercise for the other three conditions. Given that we were unable to test the validity of this prime, it is a possible shortcoming of our research as our study lacked a manipulation check to evaluate the effectiveness of the primes.

As this study looks at the impact of the primes, it is possible that the sequencing of the primes prior to the GRS and BIGL scales could have led to order effects biasing responses on these scales. Thus, our recommendation to future researchers would be to evaluate whether counterbalancing the primes and scales impacts the results.

Additional shortcomings of the research include the lack of action-compatible incentives in the research design. Neither the original study, nor the replications adhere to Induced Value Theory (IVT). In our study, due to financial constraints, we gave a flat-rate show-up fee to all participants, which was not incentive compatible. As discussed in class (Lecture 3, Slide 14), monetary incentives lie at the heart of experimental economics and hence, only providing participants with a participation fee while not giving them task-related payments may not accurately reflect their

beliefs.

Lastly, our replication and extension of the study was conducted within a more general population of MTurk users, and thus, the scenario of being ‘called on in class’ may not have been as relatable or salient to our sample as the undergraduate students in the original study by Risen and Gilovich (2008). However, this scenario was standardized across all our treatment conditions, controlling for any differences across groups owing to the scenario. An additional limitation of conducting an online experiment is that participants have to imagine themselves in certain scenarios, rather than actually experiencing them. This leads to a relative lack of experimental control as the primes and the hypothetical scenarios would not be as salient as lived experiences.

## VII. Team Reflection

In this section, we share our reflections on the team research process. This includes what went well and what played out differently than we expected.

First, we learned the value of pre-registration. Pre-deciding the hypotheses, methods, and analysis, as well as finalizing the design and implementation following pilot data collection (pre-test) made our research process more efficient and transparent. It made us carefully evaluate the experimental design and analysis methods *before* we carried out data collection.

Second, our awareness of the challenges of running an experiment online increased. Participants recruited via MTurk did not uniformly put adequate effort into the unscramble-the-words exercise. We wanted to add criteria and manipulation checks that ensured participants did the task properly yet were not able to due to limitations of the online survey software. So, some of the participants wrote unrelated answers. If we had excluded those responses from the dataset post-experiment, it would have substantially decreased our sample size. As such, with a larger budget, we would have preferred to collect enough data so we could have excluded people who did not complete the exercise properly by determining a certain cut-off threshold of correct answers.

Third, navigating team and project changes was also a key learning experience. When our group expanded from 4 to 6 members, we were excited to incorporate new research questions into the

project. The new members' ideas were insightful, and we believed ensuring that everyone's ideas were welcomed was vital to our team development. However, adding new ideas on such short notice was challenging. We spent a fair amount of time and effort trying to both justify and detail these potential new directions. However, we ultimately decided not to go through with the additions due to their complexity. Had we decided this prior to working on the pre-analysis plan, we could have spent more time on the aspects we decided to ultimately keep.

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## Appendices

### Appendix A

#### **Scenarios of Having Tempted Fate or Not Having Tempted Fate**

##### **Scenario for the Replication Control Condition**

Please read the following scenario:

Imagine that you are in a large lecture with a few hundred students and you are sitting in the middle section, a little more than half-way back in the room. The professor asks a question about the readings, but no one raises his or her hand to answer. You have done the reading and feel confident that the professor would like your answer, but prefer not to volunteer answers in large classes. The class sits in silence for two minutes before the professor explains that if no one volunteers, he will choose someone.

How likely do you believe it is that the professor will call on you? Mark your answer by choosing the appropriate number below.



##### **Scenario for the Replication Control, Religious Prime, and Superstitious Prime Conditions**

Please read the following scenario carefully.

Imagine that you are in a large lecture with a few hundred students and you are sitting in the middle section, a little more than half-way back in the room. The professor asks a question about the readings, but no one raises his or her hand to answer. You have not done the reading and feel confident that you would not be able to answer the question. The class sits in silence for two minutes before the professor explains that if no one volunteers, he will choose someone.

How likely do you believe it is that the professor will call on you? Mark your answer by choosing the appropriate number below.



## Appendix B

### Unscramble-the-Words Exercise for the Replication Control and Extension Control Conditions

Create 4-word sentences from the groups of 5 scrambled words below. **One word doesn't belong.**

For example: "high winds the flies plane" would become "the plane flies high".

fall was worried she always

shoes give replace old the

retrace good have holiday a

more paper it once do

send I over it mailed

saw hammer he the train

yesterday it finished track he

sky the seamless blue is

predictable he shoes his tied

prepared somewhat I was retired

## Appendix C

### Unscramble-the-Words Exercise for the Religious Prime Condition

Create 4-word sentences from the groups of 5 scrambled words below. **One word doesn't belong.**

For example: "high winds the flies plane" would become "the plane flies high".

felt she eradicate spirit the

dessert divine was fork the

appreciated presence was imagine  
her

more paper it once do

send I over it mailed

evil thanks give god to

yesterday it finished track he

sacred was book refer the

reveal the future simple prophets

prepared somewhat I was retired

**Appendix D****Unscramble-The-Words Exercise for the Superstitious Prime Condition**

Create 4-word sentences from the groups of 5 scrambled words below. **One word doesn't belong.**

For example: "high winds the flies plane" would become "the plane flies high".

star upon sun a wish

fortune brave the land favours

it beginner's love luck is

more paper it once do

send I over it mailed

third fun charm a time's

yesterday it finished track he

for to meant be it's

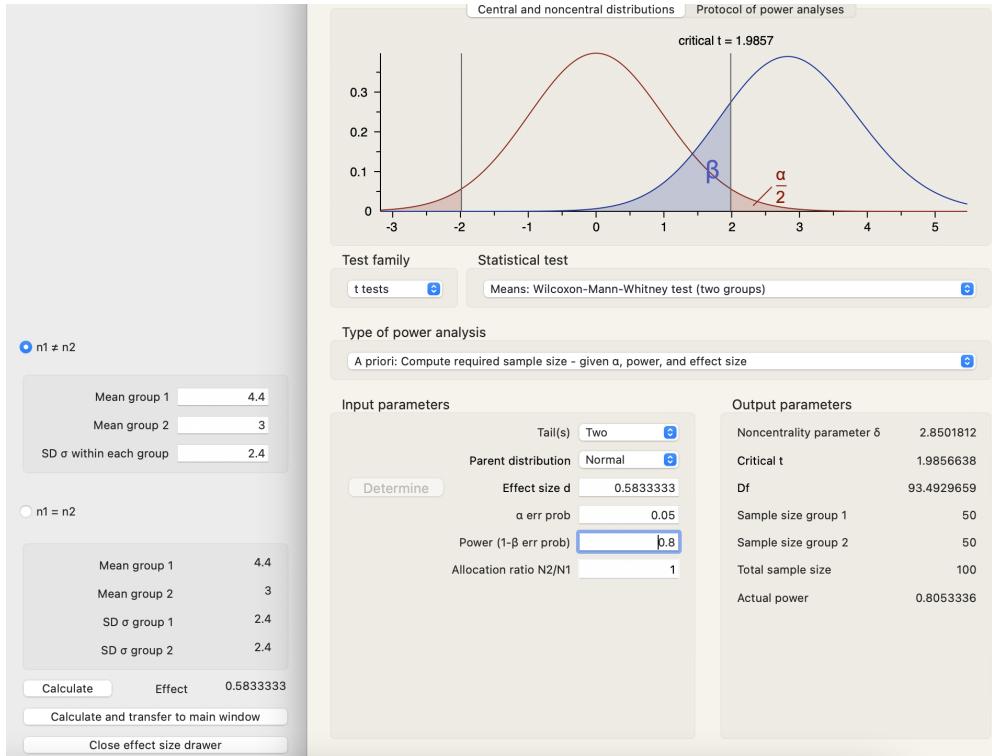
take I'll chase gamble the

prepared somewhat I was retired

**Appendix E**

## A Priori G\*Power Calculations using Data from Pilot Data Collection

### 80% Power (Superstition vs Extension Control (have not read) means from pretest)



## Appendix F

### Informed Consent Form

Please read the instructions below prior to beginning this short survey. You are invited to take part in this study which aims to understand human beliefs.

You will read a series of statements and then answer a questionnaire. We will also ask you to provide demographic information. Overall, this study will take approximately 5-8 minutes.

The risks to participating are no greater than those encountered in everyday life. Your participation in this study is completely voluntary, and you may refuse to participate. If you withdraw from the study before completing it, you will not be paid and your data will be discarded. Compensation will be awarded upon completion of the entire study.

However, if a sizeable number of people quit the survey halfway, the data quality of the survey will be compromised. Our research depends on good quality data. So, please make sure you have 8 minutes to take this survey before starting it.

All responses within this survey will be analyzed anonymously.

You will be paid within 10 days of completing the study via MTurk. Your email address will only be used to pay you correctly and will be deleted permanently from the experimenter's data after payment is complete.

If you have any questions about this study, you may contact us at [ssawhney@sas.upenn.edu](mailto:ssawhney@sas.upenn.edu)

Please feel free to print or save a copy of this consent form.

**By continuing from this page, you are indicating that you have read and understood this consent form, and wish to continue your participation in this study.**

## Appendix G

**Automated Bot Check**

Please check the box.

I'm not a robot

  
reCAPTCHA  
Privacy - Terms

## Appendix H

### Attention Checks

#### Attention Check 1

We ask you to select "Disagree" to show that you have read this sentence

- Agree
- Somewhat agree
- Somewhat disagree
- Disagree

#### Attention Check 2

Strongly  
disagree      Somewhat  
disagree      Slightly  
disagree      Slightly  
agree      Somewhat  
agree      Strongly  
Agree

We ask you to  
select "Somewhat  
disagree" to show  
that you have read  
this sentence

- 
- - 
  - 
  - 
  - 
  -

**Appendix I**  
**General Religiousness Scale (GRS; Rowatt et al., 2009)**

**The following questions ask about your religious beliefs.**

How religious do you consider yourself to be?

- Not at all religious
- Not too religious
- Somewhat religious
- Very religious

How often do you attend religious services?

- Never
- Less than once a year
- Once or twice a year
- Several times a year
- Once a month
- 2-3 times a month
- About Weekly
- Weekly
- Several times a week

How often do you read the Bible, Koran, Torah or other sacred book?

- Never
- Less than once a year
- Once or twice a year
- Several times a year
- Once a month
- 2-3 times a month
- About weekly
- Weekly
- Several times a week

How often do you pray or meditate outside of religious services?

- Never
- Only on certain occasions
- Once a week or less
- A few times a week
- Once a day
- Several times a day

## Appendix J

Indicate the extent of your agreement with the following statements.

## Appendix K

### Demographic Questions

We will now ask you a few brief questions.

Please enter your age, in years:

What is your gender?

- Male
- Female
- Other
- Refuse to answer

What is your religious affiliation, if any?

- Christian
- Jewish
- Muslim
- Hindu
- Buddhist
- Atheist
- Agnostic
- Other (Please specify)
- Don't know
- Refuse to answer

## Appendix L

### Descriptive Statistics for Level of Religiosity and Superstition

		Replication Control	Extension Control	Religious Prime	Superstitious Prime	All Groups
Standardized GRS Score	Mean <sup>17</sup>	0.13	-0.02	-0.16	0.00	0.00
	Median	0.12	-0.04	-0.14	0.22	0.04
	SD	0.85	0.84	0.86	0.95	0.87

*Table 7: Descriptive statistics for participants' standardized religiosity scores (level of religiosity)*

BIGL Score (Level of superstition)	Replication Control	Extension Control	Religious Prime	Superstitious Prime	All Groups
Mean	50.00	47.96	46.62	47.68	48.22314
Median	53.50	49.50	51.00	48.00	51
SD	13.52	10.41	14.75	12.51	12.896

*Table 8: Descriptive statistics for participants' level of superstition*

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<sup>17</sup> The standardized score is the mean of the Z scores for each item. A standardized score of 0 means that the participants' score is in the mean.

## Appendix M

### Secondary Analysis Results

Based on previous literature, we expected that the religious prime's impact on how likely a person thought they would be called on in class could be influenced by their religiosity (indicated by the GRS score). We also expected that the superstitious prime's impact on the outcome would be dependent on how superstitious a participant was (indicated by the BIGL score). To do this, we ran a regression model testing for interaction effects. However our results found no significance for either interaction. That is, a participant's religiosity in the Religious Prime group did not make them perceive a higher likelihood of being called on in class compared to the Extension Control group; additionally, a participant's superstitious beliefs in the Superstitious Prime group did not impact their perception of a negative outcome (Table 9).

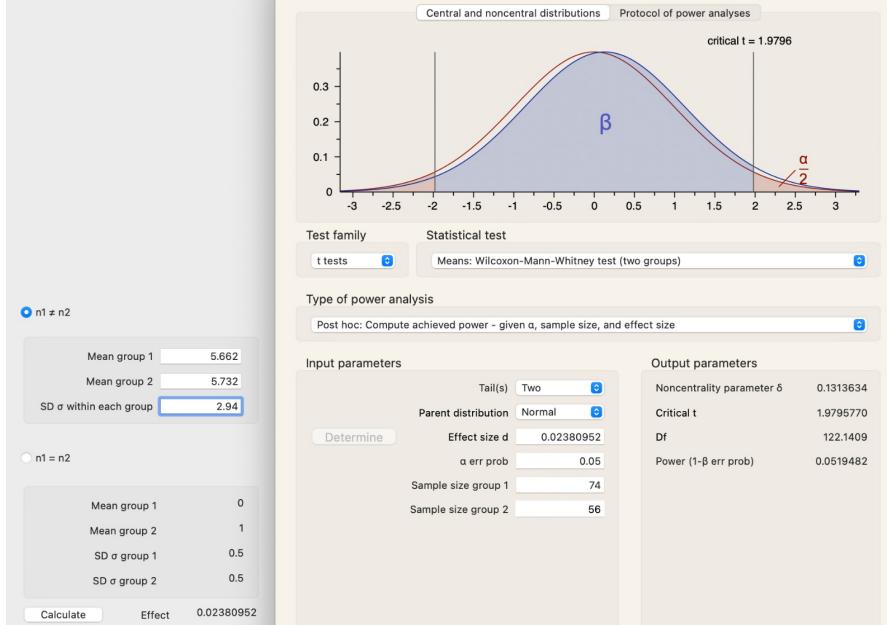
<i>Predictors</i>	<b>likelihood</b>		
	<i>Estimates</i>	<i>CI</i>	<i>p</i>
Intercept	-1.87 (0.88)	-3.61 – -0.12	<b>0.036</b>
Have read or not	-0.54 (0.41)	-1.35 – 0.27	0.193
Superstitious Prime	-1.17 (0.45)	-2.05 – -0.28	<b>0.010</b>
Religious Prime	-0.12 (0.28)	-0.66 – 0.43	0.671
GRS Score	-0.95 (1.42)	-3.76 – 1.85	0.504
BIGL Score	0.14 (0.02)	0.10 – 0.17	<b>&lt;0.001</b>
Age	0.00 (0.00)	-0.00 – 0.00	0.267
Gender(Female)	0.54 (0.30)	-0.06 – 1.14	0.077
Gender(Other)	-3.65 (2.34)	-8.26 – 0.97	0.121
Theist	1.19 (0.50)	0.22 – 2.17	<b>0.017</b>
Religious Prime & GRS Score Interaction	-0.22 (0.44)	-1.08 – 0.65	0.624
Superstitious Prime & BIGL Score Interaction	0.01 (0.03)	-0.04 – 0.07	0.626
Observations	242		
R <sup>2</sup> / R <sup>2</sup> adjusted	0.468 / 0.442		

**Table 9:** Results from the regression analysis for secondary analysis. Standard errors are in parentheses. Dependent variable was the perceived likelihood of a negative outcome. Have read or not, Superstitious Prime, and Religious Prime are dummy variables for the four conditions.

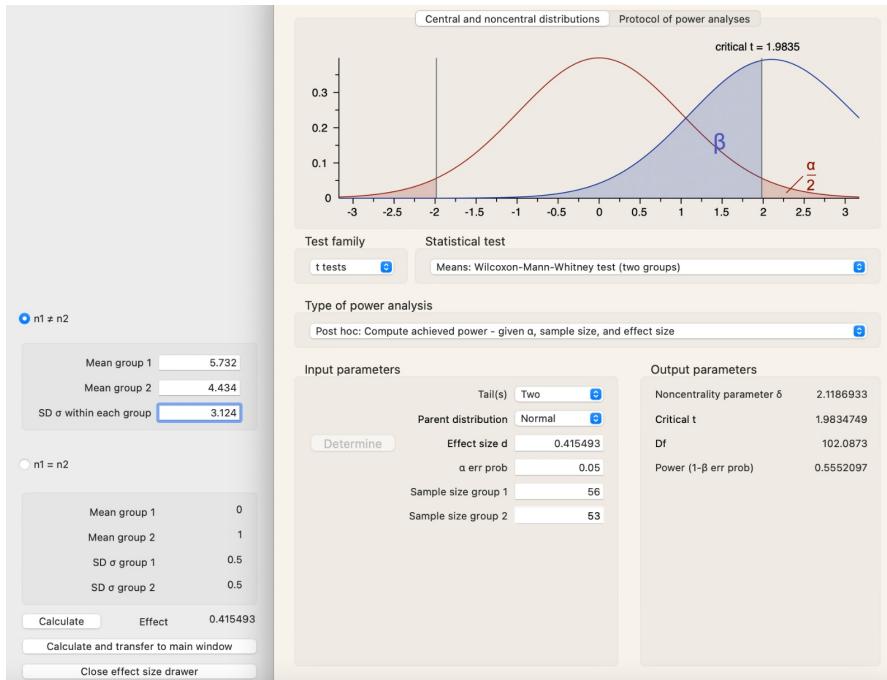
## Appendix N

### Post Hoc G\*Power Calculations

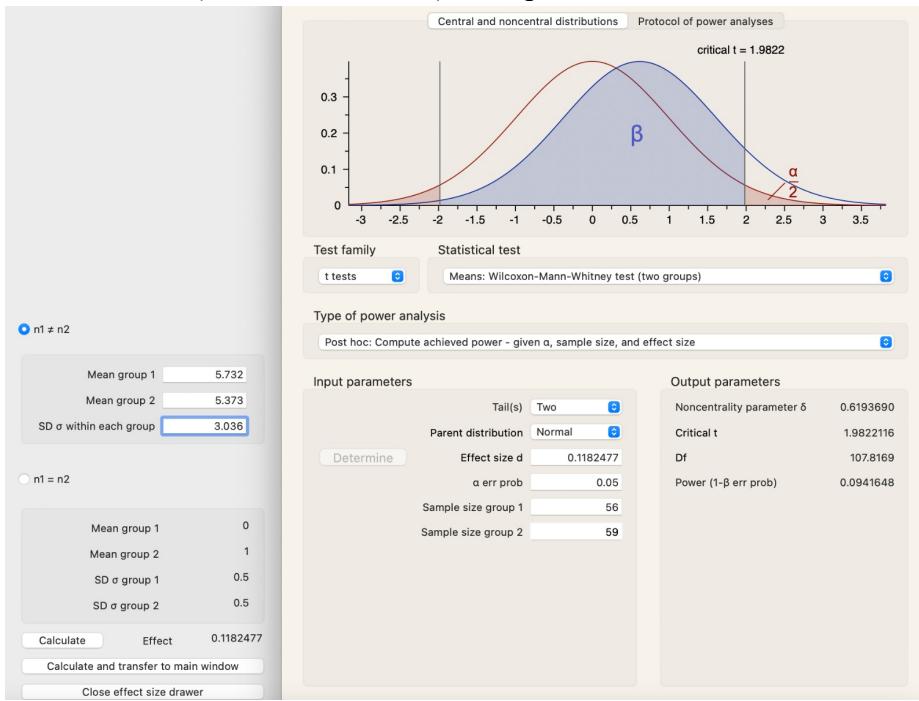
Have Read (Replication Control) vs have not read (Extension Control):



Have Not Read (Extension Control) vs Religious Prime:



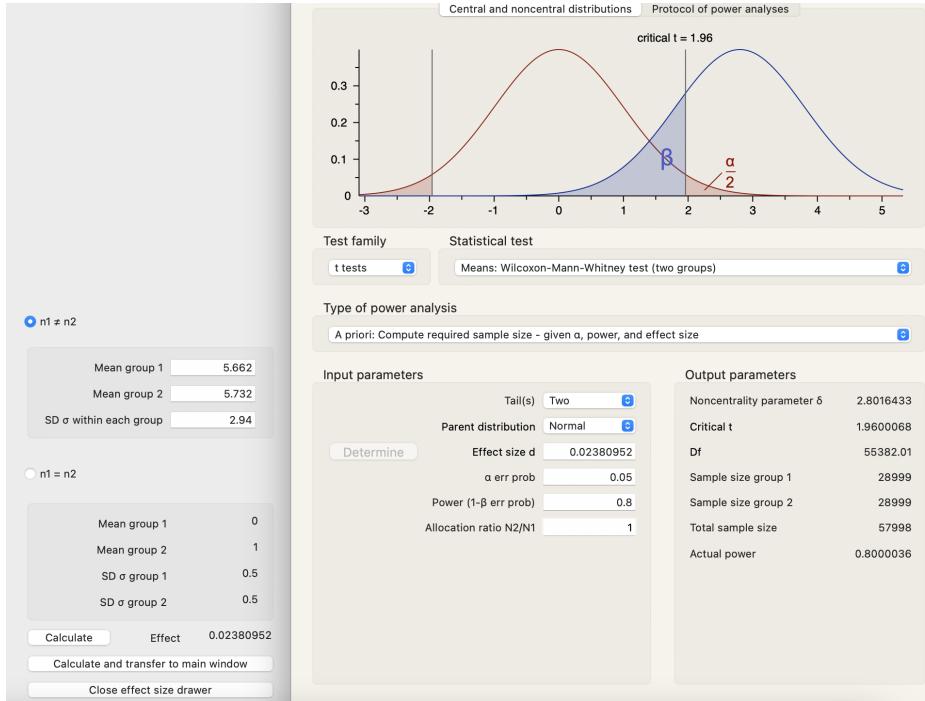
Have not read (Extension Control) vs Superstitious Prime:



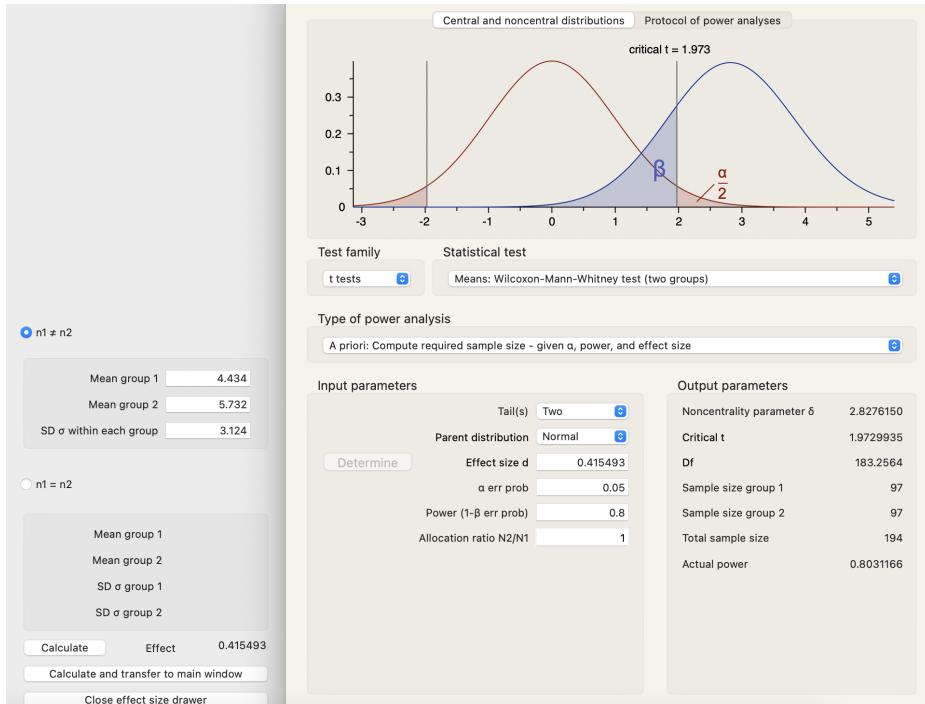
## Appendix O

### Necessary Sample Size to Achieve 80% Power

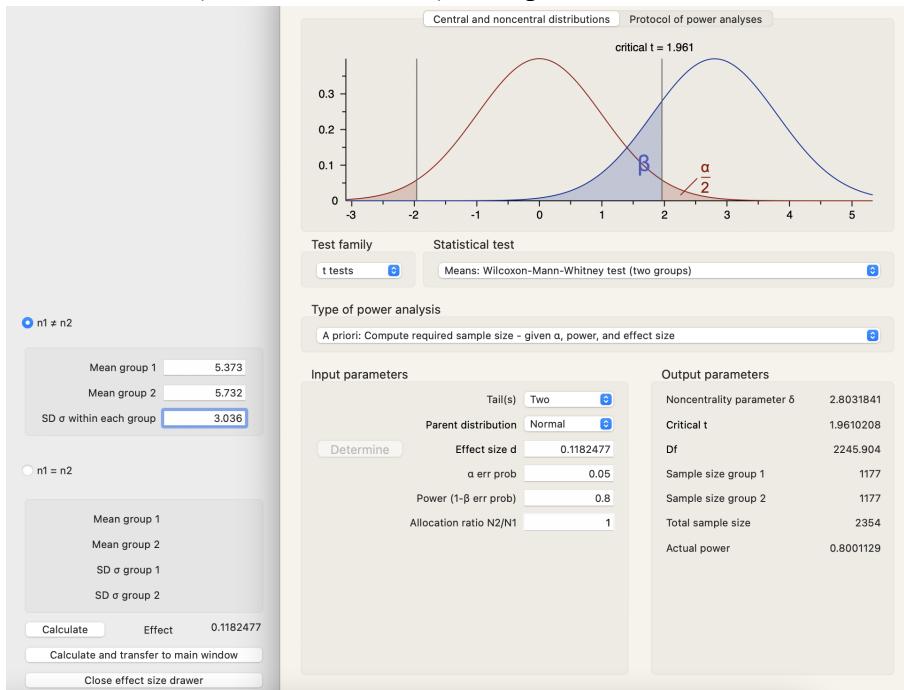
Have Read (Replication Control) vs have not read (Extension Control):



Have Not Read (Extension Control) vs Religious Prime:



## Have not read (Extension Control) vs Superstitious Prime:



**CRediT Authorship Contribution Statement**

Anna Lamb: Conceptualization, Methodology, Formal analysis, Investigation, Data curation, Writing – original draft, Writing – review and editing, Visualization, Project administration

Xiteng Lin: Conceptualization, Methodology, Investigation, Writing – original draft, Writing – review and editing, Project administration

Sita Sanjivini Sawhney: Conceptualization, Methodology, Formal analysis, Investigation, Writing – original draft, Writing – review and editing, Project administration

Riya Sirdeshmukh: Conceptualization, Methodology, Formal analysis, Investigation, Writing – original draft, Writing – review and editing, Project administration

Jessie Janny Thenarianto: Conceptualization, Methodology, Formal analysis, Investigation, Writing – original draft, Writing – review and editing, Project administration

Joshua Weiss: Conceptualization, Methodology, Investigation, Writing – original draft, Writing – review and editing, Project administration

## Group Evaluation Screenshots

### Riya Sirdeshmukh:

Thank you for completing the evaluation Inbox x 🖨️ 📎 ⋮

 **Eugen Dimant** via qemailserver.com  
to me 1:41 AM (0 minutes ago) ⭐ ↤ ⋮

Sirdeshmukh, Riya,

Thank you for completing the group evaluation for Behavioral Science: Theory and Application of Experimental Methods (Spring 2021). Your response was recorded on Saturday, December 11th at 11:41 PM.

Please be sure to save this email and include it with your group's final project submission.

### Jessie Janny Thenarianto:

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 **Eugen Dimant** via qemailserver.com  
to me 9:48 AM (0 minutes ago) ⭐ ↤ ⋮

Thenarianto, Jessie Janny,

Thank you for completing the group evaluation for Behavioral Science: Theory and Application of Experimental Methods (Spring 2021). Your response was recorded on Sunday, December 12th at 7:48 AM.

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### Xiteng (Steven) Lin:

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 **Eugen Dimant** via qemailserver.com  
to me Thu, Dec 9, 10:29 AM (3 days ago) ⭐ ↤ ⋮

Lin, Xiteng,

Thank you for completing the group evaluation for Behavioral Science: Theory and Application of Experimental Methods (Spring 2021). Your response was recorded on Thursday, December 9th at 8:29 AM.

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### Joshua Weiss:

Thank you for completing the evaluation Inbox x 🖨️ 📎 ⋮

 **Eugen Dimant** edimant@sas.upenn.edu via qemailserver.com  
to jhsweiss 3:16 PM (10 minutes ago) ⭐ ↤ ⋮

Weiss, Joshua Hillel Samuel,

Thank you for completing the group evaluation for Behavioral Science: Theory and Application of Experimental Methods (Spring 2021). Your response was recorded on Sunday, December 12th at 1:15 PM.

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### Sita Sanjivini Sawhney:

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5:43 PM (1 minute ago)



Sawhney, Sita Sanjivini,

Thank you for completing the group evaluation for Behavioral Science: Theory and Application of Experimental Methods (Spring 2021). Your response was recorded on Sunday, December 12th at 3:43 PM.

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Anna Lamb:



Eugen Dimant via qemailserver.com  
to me ▾

10:10 PM (0 minutes ago)



Lamb, Anna Katelin,

Thank you for completing the group evaluation for Behavioral Science: Theory and Application of Experimental Methods (Spring 2021). Your response was recorded on Sunday, December 12th at 8:10 PM.

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