

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

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Abstract:

The unscientific belief that if one tempts fate, one has a higher likelihood of subsequent punishment is both widespread and incompletely understood. In this study, we aim to further elucidate the mediating factors underlying the ‘tempting fate effect,’ by performing a replication of the original Risen and Gilovich study (2008) and a more recent replication by Mathur (2016). We specifically test whether religious or superstitious beliefs influence the tempting fate effect using targeted primes. Further, we examine possible associations between the tempting fate effect and individuals’ beliefs regarding both free-will vs. determinism as well as regret aversion. We perform this between-subject study design using Amazon’s MTurk survey platform. Subjects are randomized to one of four conditions: a non-tempting fate condition, a tempting fate condition without primes, tempting fate with religious priming or tempting fate with superstitious priming. Beliefs regarding religion, superstition, free-will vs determinism and regret aversion are subsequently assessed in a post-intervention survey. Our findings regarding the mediating factors of the tempting fate effect could be applied to sectors in which individuals’ risk calculations of future events impact present decisions, such as insurance purchasing, marketing, and health-related behaviors.

Word Count: 189 words

Introduction**Background**

Many people who don’t believe in fate are still afraid to tempt it. Some believe it is more likely to rain if they don’t bring their umbrella while others believe that an extremely slow checkout line will speed up the minute they leave it (Miller & Taylor, 1995). This irrational belief that tempting fate leads to a higher likelihood of bad outcomes is called the *Tempting Fate Effect*.

The Risen and Gilovich (2008) study that we will be replicating focused on a ‘two system approach’ as a mediator of the tempting fate effect. People’s rational faculties tell them that there is no association between two events. However, a set of associations stored by the intuitive system can give people a very strong “gut feeling” that such acts do in-fact bring bad luck (Evans, 2007; Kahneman & Frederick, 2002). In other words, 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 conducted six studies to test whether System 1 mediates the tempting fate effect and whether the irrational belief can be suppressed by System 2. Together, the results of Studies 1 and 2 indicate that people believe that negative outcomes are more likely following behaviors that tempt fate. Study 3 found that participants were faster to recognize that a negative outcome was a sensible conclusion to each story if they had read that the protagonist had engaged in an action that tempted fate. Study 4 showed that actions that

tempt fate are more likely to call to mind the prospect of a negative outcome than actions that do not tempt fate. The results of Study 5 lend further support to the role of accessibility (priming through stories of tempting fate) as a determinant of people's belief that negative outcomes are especially likely following behaviors that tempt fate. Finally, Study 6, the subject of our replication, found that cognitive load (occupying system 2 resources) increased the irrational belief that tempting fate leads to negative outcomes (Risen & Gilovich, 2008).

Maya Mathur, a Stanford University researcher, attempted to replicate study 6 in 2012. They used the original study questionnaire consisting of a scenario, a cognitive load task, and a manipulation check but conducted the study online via MTurk (Mathur, 2016). Participants read a self-scenario which asked them to imagine themselves in a large lecture. One treatment group read that they had done the readings for the class while the other read a scenario in which they had not done the readings for the class. Half of the participants who read each scenario were under cognitive load (i.e. asked to count backwards by 3s starting from 564). Hence, the experiments had 4 conditions in total, forming a complete 2x2 design. All the participants were asked to indicate the likelihood that they would be called on in the lecture. The results showed that there was no interaction between perceived likelihood of being called on, tempting fate behavior, and cognitive load. Hence, the original findings failed to be replicated.

In our replication of Study 6, we will focus on enhancing other mediators of unscientific beliefs to better elucidate what mediates the tempting fate effect. Research on the roots of unscientific beliefs by Boudry et al. (2015) found that such beliefs develop and spread due to certain worldviews and pseudoscientific belief systems. Two such worldviews are *superstitious worldviews* and *religiosity worldviews*. By enhancing, or priming, each of these mediators, we might be able to find causal relationships explaining the tempting fate effect. Worldviews can be sub-mediated by beliefs about free will versus determinism as well as internal psychological mediators such as regret aversion (Delfrabbro & Winefield, 2000; Viney et al., 1982). We will use post-intervention survey questions to examine whether the mediators and sub-mediators of the tempting fate effect are correlated.

If the tempting fate effect is mediated by a religious worldview, we plan to increase the salience of this worldview to enhance the tempting fate effect. Religious *priming* is frequently relied upon in the psychology literature 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* - often elicited by unscrambling scrambled, religiously-themed sentences - is a commonly employed religious 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).

There remains important disagreement in the literature about both the underlying psychological mechanisms and effectiveness of religious priming and implicit religious priming (Gomes & McCullough, 2015). Though p-curve analyses and Bayesian bias correction methods – techniques used to distinguish real effects from statistical flukes - have suggested that RP does indeed affect behavior (Shariff et al., 2016; Val Elk et al., 2015), a PET-PEESE meta-analysis concluded that religious priming effects are driven by publication bias alone (Val Elk et al., 2015). As this disagreement remains unresolved, and due to the implicit religious prime's continued dominance in the experimental study of religious influence on behavior (Billingsley et al., 2018) we have elected to rely on implicit religious prime to manipulate the salience of religion in the current study.

Furthermore, religious priming can activate psychological mechanisms that can differentially impact behavior. Depending on subjects' level of religious identification (Shariff et al., 2016; Zhang et al., 2019) as well as specific beliefs such as whether God is authoritative/punishing vs benevolent/forgiving (Johnson 2005; Shariff & Norenzayan, 2011), levels of cheating and subsequent expectation of punishment can differ significantly (Randolph-Seng & Nielsen, 2007; McKay et al., 2011). As such, we remain uncertain of the directionality of the implicit religious prime's impact on the tempting fate effect. 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 prime can induce increased subjective anticipation of Divine protection within perilous situations or after having engaged in risk-taking behavior (Holbrook et al., 2016; Kupor et al., 2015).

Most academics view superstitious thinking as irrational on an individual level, and even detrimental on a global level (George & Sreedhar, 2006). Increasing the salience of a superstitious worldview could mediate the tempting fate effect by mediating risk preferences. A study by Nguyen (2012) found that superstitious primes including words such as “lucky”, “destiny” and “meant to be” significantly impacted risk preferences. However, the literature is divided on whether superstitious beliefs would make people risk averse or risk seeking.

Superstitions have been demonstrated to have a protective effect. In other words, engaging in superstitious activities are believed to “push away” bad luck, making people more risk seeking (Zhang et al., 2014). However, Chinchachokchai et al. (2016) 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 leads to different risk preferences and hence, different beliefs about tempting fate.

An interesting study by Pontes and Williams (2021) adds to the ambiguity about whether superstitious beliefs lead to risk seeking or risk averse behavior, and hence, whether such beliefs

enhance the tempting fate effect. In the context of gambling, they find a significant effect of a superstition that red color signals a bad outcome, and hence, red leads to risk aversion. However, when participants report “feeling lucky” or are from Asian Chinese backgrounds, the effect is reversed and they take more risks.

In addition to the factors relating to the differential effects of religious priming noted above, subjects’ beliefs regarding free-will versus determinism may also impact the tempting fate effect. This relationship between free-will vs determinism and any anticipated punishment by tempting fate is likely to be complex, if present (Viney et al., 1982). Though free-will vs. determinism beliefs can be informed by religious-philosophical attitudes, expectations regarding free-will vs determinism-driven punishment can also be mediated through the lens of psychosocial determinism, the belief that environmental factors determine our behavior (Stroessner & Green, 1990). Given its potential as a mediating factor for the tempting fate effect separate but related to religious attitudes and beliefs, we believe free-will vs determinism beliefs can also be interrogated within our current study. However, similarly to the directionality of religious prime’s effects on the tempting fate effect, we are uncertain of the directionality of effect between subjects’ free-will vs determinism beliefs and the tempting fate effect. For example, subjects with strong beliefs in fate have been shown to believe that its action and direction are unpredictable (Paulhus & Carey, 2011). Some research has demonstrated that a strong belief in free-will increases subjects’ beliefs in a just world as well as punishment and accountability for unjust action (Carey & Paulhus, 2013). Conversely, whether subjects with strong beliefs in free-will consider cheating or free-riding behavior – such as coming unprepared for class – unjust and worthy of punishment can vary greatly based on non-psychological factors such as subjects’ country of residence (Martin et al., 2017).

A final factor that may play an influential role in the tempting fate effect involves subjects’ levels of regret aversion. Regret-averse individuals are motivated to select choices that minimize future regret (Zeelenberg et al., 1996). In a relevant example for our current research, higher levels of regret aversion (Tochkov, 2009) has been shown to counteract the irrational beliefs held by gamblers regarding lower likelihoods of future negative outcomes (Delfrabbro & Winefield, 2000). In other words, high levels of regret aversion can be linked to a stronger belief in anticipated future negative outcomes. Similar to the irrational thinking of problem gamblers (Clotfelter & Cooke, 1993), the tempting fate effect is an unscientific belief regarding the likelihood of future negative outcomes based on past actions/events. As such, we anticipate that a higher level of regret aversion could result in increasing our subjects’ perceived likelihood of being called upon in class while unprepared.

Research Questions

Our primary, overarching research goal is to better understand the tempting fate effect, whereby individuals irrationally believe that an increased likelihood of experiencing a negative outcome is

caused by a non-causal, ‘tempting fate’ action. Specifically, we wish to understand *which factors are responsible for influencing the tempting fate effect*. Given the fact that religious and superstitious beliefs often underpin irrational or unscientific thinking (Boudry et al., 2015), we will ask the following:

RQ1: Does religious priming influence the tempting fate effect, either making subjects perceive an increased or decreased likelihood of a negative outcome for having tempted fate?

RQ2: Does superstitious priming influence the tempting fate effect, either making subjects perceive an increased or decreased likelihood of a negative outcome for having tempted fate?

Furthermore, beliefs regarding free-will vs determinism can influence how individuals understand and predict future events (Paulhus & Carey, 2011). As free-will vs determinism beliefs may or may not be related to the religious factors (Stroessner & Green, 1990) we interrogate in RQ1, we wish to additionally ask the following:

RQ3: Are beliefs regarding free-will vs determinism associated with an increased or decreased perceived likelihood of a negative outcome for having tempted fate?

Lastly, some research has demonstrated that regret aversion can modulate some individuals’ irrational beliefs regarding future negative outcomes (Tochkov, 2009), making these negative outcomes more readily anticipated. As such, we will assess whether this holds true for the tempting fate effect as well. Specifically, we will ask:

RQ4: Are higher levels of regret aversion associated with individuals perceiving an increased likelihood of a negative outcome for having tempted fate?

Broadly speaking, a better understanding of the tempting fate effect could be directly applied to influencing behaviors that involve individual risk calculations about how actions today affect negative outcomes in the future, such as insurance purchasing (Lyu & Barre, 2017) or health-related behaviors (Anderson & Mellor, 2008). If we better understood the amplitude and mechanisms underpinning the tempting fate effect, this knowledge could inform marketing and policy decisions within these domains.

Additionally, the behavior which ‘tempts fate’ within our replication of Risen and Gilovich’s sixth experiment involves subjects imagining that they have come unprepared to class (Risen & Gilovich, 2008). This behavior could be alternatively interpreted as subjects engaging in free-riding behavior, hoping not to be ‘called on’ while relying on the preparedness of others. Within this interpretation, if we could better understand the factors involved in subjects’ risk assessments of such free-riding behavior, perhaps we might be able to modulate the behaviors

themselves. The religious priming we include in this study, for example, has been repeatedly linked to increased prosocial behaviors (Shariff et al., 2016). Given the societal costs of free-riding behavior (Albanese & Van Fleet, 1985; Armstrong, 2016), we believe it is crucial to better understand the factors involved in the risk assessment of such behaviors, which we plan to elucidate in our current study.

Research Design

Basic Methodological Framework

To understand the impact of unscientific beliefs on one's perceived likelihood of negative outcomes, we propose an experimental study to be conducted on the online platform, Amazon's Mechanical Turk (MTurk). The study will include 71 participants who will be recruited via the MTurk platform and randomly assigned to one of the four groups.

Our four groups include two control groups from the original study measuring the conditions of 'having completed the readings for class' and 'not-having read for the class'. The new treatments would include one of a religious prime and another of a superstitious prime.

At the start of the experiment, participants will be required to provide informed consent (Appendix A). Post consent, there will be a bot check using the question provided in Appendix B, to identify the real participants from the bots. Qualifying participants would then proceed to receive instructions for the experiment (Appendix C). After instructions, each participant will be provided with a sentence unscrambling exercise which acts as a prime for our experiment, based on the treatment group they are in. The first two groups will be provided with a generic sentence unscrambling exercise, the second group will receive religious beliefs motivated sentences to unscramble and the last group will receive superstitious beliefs motivated sentences to unscramble. The religious prime has been adapted from the research done by Billingsley et al. (2018), we would also be using the implicit control prime used in the same study for our groups that do not have the priming conditions. The superstitious prime has been adopted from research conducted by Nguyen (2012). Each exercise will have 10 sentences to unscramble and we would also readapt the superstitious prime to be similar to the religious and control primes.

Upon completion of the unscrambling sentence exercise, participants will be provided with a scenario of themselves as students in a class, and would be informed of whether they have completed the reading for the class prior to it (condition 1) or not (conditions 2 to 4). After reading the text, the participants will then be asked about their perceived likelihood of being picked on by the teacher to answer questions based on the assigned reading for that class. This measure will be the same as tested by the replication and original study. Participants will also be additionally required to answer demographic questions, their perceived levels of religiosity and

superstitious beliefs, regret aversion, and beliefs on free-will. The participant responses will be recorded using Likert scales and a numerical slider (see Types of Data for details).

Hypotheses

H1: Religious priming, together with unpreparedness, impacts the perceived likelihood of negative outcomes

H2: Superstition priming, together with unpreparedness, impacts the perceived likelihood of negative outcomes

H3: A strong self-reported belief in free-will impacts the perceived likelihood of a negative outcome

H4: A higher self-reported level of regret aversion increases the perceived likelihood of negative outcomes.

Outcome Variables

The primary outcome variable for this study is a self-report measure of the perceived likelihood of occurrence of the negative outcomes by the participants. The participants will be required to answer how likely it is that they would be picked on by the teacher if they have not done the reading for the class (conditions 2 to 4) and if they have done the reading for the class (condition 1). This variable will directly be taken from the original paper. We would be using the General Religiousness Scale (GRS) adapted from the research done by Rowatt et al. (2009) (Appendix D) to measure the levels of religiosity of our participants, and the Belief in Good Luck (BIGL) scale adapted from the research conducted by Darke and Freedman (1997) (see Appendix E) to measure superstition. Data will be collected via Likert scales powered by Qualtrics owing to their ease of applicability of these scales to our experimental methodology.

Additionally, our mediating variables will also be evaluated through self-reported measures of free-will and regret aversion. For the free-will measure, participants will be asked to indicate their agreement with the statement: “I have free will” on a 100-point sliding scale with values ranging from 0 (Strongly disagree) to 100 (Strongly agree) (Appendix F). The scale is adapted from Schooler et al. (2014) and Nadelhoffer et al. (2020), and has been shown to have good predictive validity (Feldman et al., 2016). To measure regret aversion, we plan to use a 5-item Regret Scale that assesses the tendency to anticipate and feel regret (Schwartz et al. 2008). Participants will respond to a 7-point Likert scale from 1 (completely disagree) to 7 (completely agree). The item composition is shown in Appendix G.

Lastly, the comprehension of the priming material will be evaluated using an item that asks participants to identify the prime they received at the start of the study. This will be administered as an attention check and presented in a multiple-choice question format.

Intervention

The experiment will be conducted on Amazon's Mechanical Turk (MTurk) in mid-November 2021. Participants will be primed according to their treatment groups. Post the priming, they will be provided with the task of identifying whether they completed the reading for class (condition 1) or not (condition 2 to 4). In the last stage of the experiment, participants will be provided with a post-intervention survey which will include their measure on the perceived likelihood of the negative outcome, the GRS (Rowatt et al., 2009), the BIGL (Darke & Freedman, 1997), the free-will slider (Nadelhoffer et al., 2020; Schooler et al., 2014), regret aversion survey (Schwartz et al. 2008), and demographic questions including age, gender, and religion followed.

We aim to recruit 71 participants using the online platform Amazon's MTurk, who will then be randomly assigned to one of our four treatment groups: Completed reading for class and no prime, Not completed reading for class and no prime, Not completed reading for class and religious prime, and Not completed reading for class and superstitious prime. Randomization will be done using the 'Randomizer' function offered on Qualtrics and participants will not be informed of the other treatments being administered as it may interfere with the prime and affect the results of the experiment. We would also be using the understanding and identification of the prime as a manipulation check in the post-study intervention. Further details of the prime can be found in Appendix H.

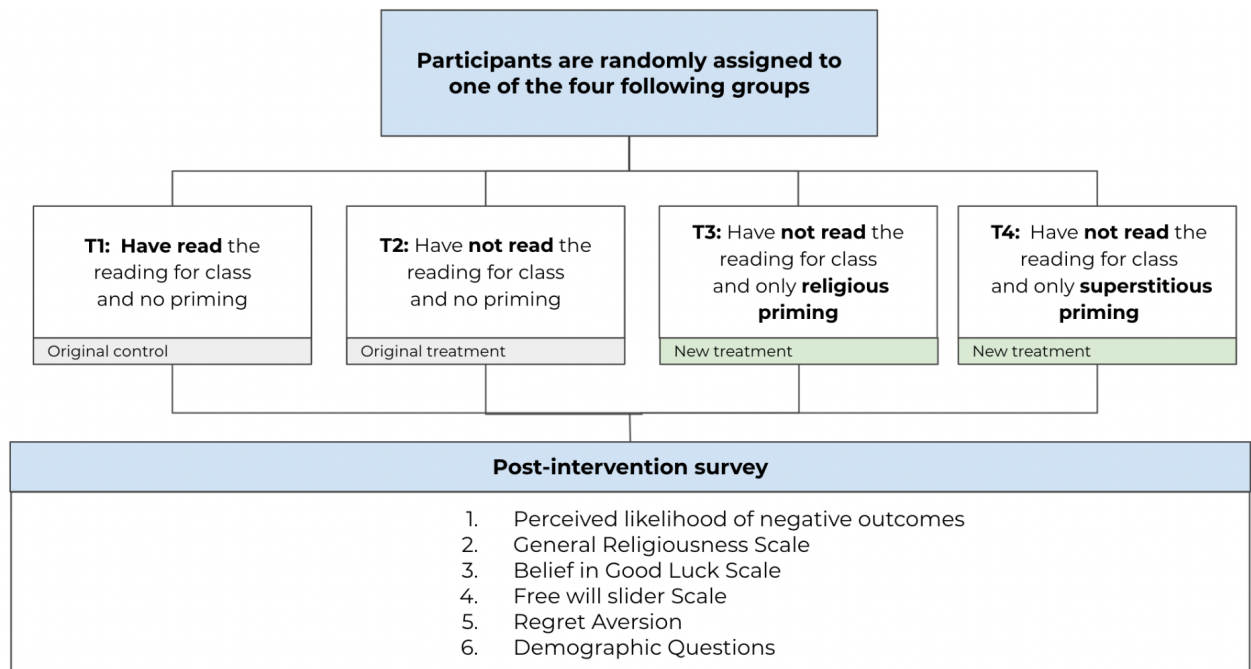


Figure 1. Experimental Framework

Theory of Change

The original study hypothesized that subjects who are ‘tempting fate’ – by not coming prepared to class, and thus free-riding on the preparedness of others – will perceive an increased risk of ironic punishment – such as being called on by the teacher. Though the original authors found effects of the tempting fate belief, the results of the study did not replicate in a follow-up attempt. In an attempt to further understand the sources of manipulation of the tempting fate belief, we study the impact of internal mediating factors.

Two primary mediating factors underlying irrational or unscientific beliefs include superstition and religion. By both manipulating and successfully elucidating the sources of the tempting fate belief, we believe we could better understand the tempting fate belief. Additionally, we would also compare these to the participant’s perceived free will and regret aversion beliefs to understand the role of internal agency in moderating the tempting fate belief.

Religious Beliefs

Prior research suggests that religious priming makes people believe that unfair acts should be punished, and can also make them fearful of being caught when doing immoral acts (McKay et al., 2011; Randolph-Seng & Nielsen, 2007). We believe that, for this reason, participants when provided with an implicit religious prime, will show a higher perceived likelihood of tempting fate and being picked on by the teacher. However, prior studies have not tested the effect of religious priming on the perceived likelihood of punishment as a negative consequence of tempting fate.

Superstitious Beliefs

Existing literature also suggests that superstitions may be related to risk aversion and the perceived likelihood of negative events following tempting fate (Zhang et al., 2014). Rituals to gain good luck, for example, may lead to a lower perceived likelihood of negative events. We believe that, for this reason, participants when provided with an implicit negative superstitious prime, will show a higher perceived likelihood of tempting fate and being picked on by the teacher. However, prior studies have not tested the effect of priming specific negative superstitions on the perceived likelihood of negative consequences of tempting fate.

Free Will and Determinism Beliefs

Free will beliefs (FWBs) are associated with the idea that individuals are morally responsible for their actions and that their fate is in their own hands (Schooler et al, 2014). In contrast, prior research has found that people who believe in fatalistic determinism believe fate is unpredictable (Paulhus, 2011). Therefore, it may be the case that people who hold FWBs and tempt fate believe their unfair act should be punished. This might make bad outcomes more accessible in their minds and increase the perceived likelihood of being called on in class. Notably, we are uncertain about the directionality of the relationship between deterministic views and the

likelihood of bad outcomes as a result of tempting fate.

Regret Aversion Beliefs

The original study claims that the mind's tendency to seize on negative prospects influences people's perception of the likelihood of negative outcomes. The anticipation of the bad outcome affects how people behave at the present moment. One closely related concept is the idea of regret aversion (Zeelenberg et al., 1996) which suggests that people may exhibit different risk preferences and behavior depending how aversive they are to the anticipation of regret. It may be the case that people who anticipate greater regret after tempting fate might enhance their mental accessibility of the bad outcome thus placing a higher likelihood value on the occurrence of bad outcomes.

Past research has documented the impact of anticipated regret on people's decision making. It can promote risk-seeking as well as risk-averse behavior (Zeelenberg, 1999). It may also increase consistency between one's intentions and the eventual outcome (Abraham, 2010). Anticipated regret may also help to reduce excessive gambling (Tochlov, 2009). Thus, whether the anticipation of regret plays a role in people's judgment when it comes to tempting fate might help us better understand the psychological mechanism behind their irrational judgments. More specifically, we are curious to know the degree to which people are regret averse mediates the relationship between "tempt fate" and "perceived likelihood of negative outcome".

Sample

Specification of unit of analysis

Participants for the study will be recruited through MTurk. MTurkers who live in the United States, have completed a minimum of 500 tasks, and have a 95% completion rate (HIT approval rate) or higher will be eligible and included in the study. This minimum number of completed tasks and completion rate follows the minimum figures to ensure data quality that have been used by previous researchers (Bergman et al., 2020; Peer et al., 2014).

The original study used undergraduate students as their sample (Risen & Gilovich, 2008), whilst the replication of the original study used registered MTurkers of all ages (Mathur et al., 2020). Our proposed study will recruit MTurkers of all ages (over 18 years old). Mathur et al. (2020) assessed the possibility that the experimental scenario (answering questions in class) may be more personally salient to actual students than MTurkers. They found that the use of MTurkers of all ages was not problematic since MTurk subjects in their study attached importance – to answering the professor's questions correctly – that is at least comparable to on-site undergraduate subjects. As such, we argue that our proposed sample of MTurkers of all ages is appropriate.

Projected sample size and statistical power calculations

We used data from Risen and Gilovich (2009)'s Study 6 to calculate the required sample size using G*Power. We used the a priori test with the Wilcoxon-Mann-Whitney test. Using data from the original study (Group 1 [$M = 2.93$, $SD = 2.16$], Group 2 [$M = 1.90$, $SD = 1.42$]), we found an effect size of $d = 0.5635066$. 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.

For this pre-analysis plan, we assume that the effect size found in the study is the true effect size. At a significance level of $\alpha = .05$, an a priori sample size calculation using G*Power found that approximately 168 participants, that is 42 subjects per condition, are needed in order to detect an effect size of $d = 0.5635066$ with 80% power. The calculations in G*Power is presented in Appendix J.

Variations from the intended sample

High attrition rates are recognized as one of the challenges in conducting research on MTurk (Aguinis et al., 2021). Prior studies suggest a five-and-20 rule of thumb with regard to attrition; that is, up to 5% attrition introduces little bias while more than 20% attrition introduces significant threats to validity (Schulz & Grimes, 2002). We aim for a limit of acceptable attrition of 5%, or approximately 9 participants, which means we need to collect 177 participants.

To keep attrition rates low, we plan to carry out several strategies when conducting data collection. First, we will appeal to the participants' conscience (e.g., explaining at the start of the survey that dropping out could affect the data quality), which is one of the ways that Zhou and Fisbach (2016) have found to be able to attenuate dropout rates. Second, we introduce a standard fair pay for MTurkers, which should incentivize individuals to participate and complete the task. Participants will receive payment after they have completed the study.

Besides attrition, treatment non-compliance (i.e., the participant's received treatment does not match the assignment treatment) also threatens the validity of causal inference and the extent to which the treatment effects can be interpreted (Sagarin et al., 2014). Differences in non-compliance across conditions would make the treatment and the control groups not comparable, since "the control group cannot act as a proxy for what would have occurred to the treatment group participants, if they have not received the treatment" (Sagarin et al., 2014, p. 319). To prevent treatment non-compliance and unmeasured outcomes, all tasks and questions will be mandatory for all participants to carry out and answer. We will apply a force response rule that is available in Qualtrics to achieve this. Participants who fail to complete the task will be excluded from the analysis.

Data Collection and Processing

Types of Data

Data Category	Prompt	Variable (Data Type)
Consent	“Do you agree to participate?”	I understand and agree to participate. No, I do not agree and would not like to participate. (categorical)
Perceived likelihood of occurrence of the negative outcomes	“How likely do you believe it is that the professor will call on you? Mark your answer by choosing the appropriate number below.”	1 item with 10 possible responses 1-Not at all likely 10-Extremely likely (numerical, discrete)
GRS Score (Religiosity scale)	Sample item: “How religious do you consider yourself to be?”	4 items, with multiple possible responses (4 to 9-point Likert scale depending on the item). Sample: 1-Not at all religious 2-Not too religious 3-Somewhat religious, 4-Very religious (numerical, discrete)
BIGL Score (Superstition scale)	“Indicate the extent of your agreement with the following statements.”	14 items, with 6 possible responses (6-point Likert scale) 1-Strongly disagree 2-Somewhat disagree 3-Slightly disagree 4-Slightly agree 5-Somewhat agree 6-Strongly agree (numerical, discrete)
Free Will Score	“Indicate the extent to which you agree with the following statement”: “I have free will”	1 item, to select a value ranging from 0 (strongly disagree) to 100 (strongly agree). (numerical, discrete)

Regret Scale	“Indicate the extent to which you agree with the following statements”	5-items, with 7 possible responses (7-point Likert scale) 1-Strongly disagree 2-Disagree 3-Somewhat disagree 4-Neither agree nor disagree 5-Somewhat agree 6-Agree 7-Strongly agree (numerical, discrete)
Manipulation check	“At the beginning of this experiment, you were asked to unscramble words into a proper sentence. Which of the following categories do you think best describes the words that you had to unscramble?”	Religious Superstition None of above (categorical)
Self-reported demographic information	“Now we would like to ask you a few questions about yourself.”	Age, in years (ratio) Gender (categorical) Religion (categorical)
Attention check 1 (during BIGL Scale)	“Please select ‘Somewhat agree’”	1-Strongly disagree 2-Somewhat disagree 3-Slightly disagree 4-Slightly agree 5-Somewhat agree 6-Strongly agree (numerical, discrete)
Attention check 2 (during Regret Scale)	“Please select ‘Somewhat disagree’”	1-Strongly disagree 2-Disagree 3-Somewhat disagree 4-Neither agree nor disagree 5-Somewhat agree 6-Agree 7-Strongly agree (numerical, discrete)

Table 1. Types of Data

Data collection methods, sources, and timeline

All data will be collected using a Qualtrics-based survey and through the MTurk platform. We will first run a pre-test (pilot study) before carrying out the actual experiment (final data collection). Data collection for the pilot study will be carried out around early November 2021, followed by the final data collection around mid-November 2021. We expect that all data in the final data collection will be collected by the end of the day of the launch.

Termination considerations and stopping rules

Data collection will end when at least 71 participants have completed the experiment (the number of 75 participants due to financial constraints which will be elaborated further in Cost Structure), which is equal to 17 to 18 participants in every condition, after applying exclusion criteria. The exclusion criteria are the following:

- Participants who fail to finish the task
- Participants who ask their data to be removed
- Participants who fail to pass the manipulation check for the primes, the attention check, and the bot test. The manipulation check in the post-intervention survey asks the participants what kind of prime they thought they received. If their answer does not match the prime that was given (e.g., they perceived the superstition prime as a religious prime), they will be excluded from the sample. Participants who fail any one of the attention checks and the bot test will also be excluded from the data analysis.

Data Management Plan

Tools. The survey will be created using Qualtrics. Pilot data collection will be carried out through our personal networks via chat messaging, social media, and email. Final data collection will be conducted on MTurk. The main statistical analyses will be conducted in R and G*Power. Supporting modifications or analyses in Microsoft Excel and SPSS will be used if needed.

Confidentiality. All data collected from participants will be kept confidential. No individual identifying information will be collected. Each participant will be assigned a code number (i.e., subject ID). Participants will be told that they are free to withdraw from the experiment at any time. The data from participants that withdraw will be removed and excluded from the final dataset and analysis.

Storage. Data will be stored on a third-party cloud service that can only be accessed by authorized users (i.e., research team and the BDS 501 teaching team).

Pilot Data Collection

Pilot data collection will be carried out around early November 2021. Participants will be recruited from the researchers' personal networks through chat messaging, social media, and email. We plan to recruit at least a total of 60 participants. Data from the pilot data collection will be used to inform any necessary modifications to the task and survey in the final data collection. Modifications may include the length of the task (e.g., number of questions) and the wording of the instructions or questions. The means and standard deviations from the pilot study dataset will be used to calculate the sample size and power for the final data collection. We predict that the survey will take approximately 8-10 minutes to complete from start to finish. The pilot data collection will provide a clearer picture of the amount of time needed to complete the task.

Final Data Collection

Final data collection will be carried out on MTurk around mid-November 2021. Based on the current sample size calculation using data from the original study and taking attrition into account, we intend to collect 177 participants, divided into four treatment arms. Using this sample size, we should be able to reach an effect size of $d = 0.5635066$ with a statistical power of 80%. Anticipating an 5% attrition rate, we would have 168 participants to run two-sided t-tests. After the pilot data collection has been carried out, we will use the pilot study data to calculate the sample size needed to achieve 80% statistical power for the final data collection.

Cost Structure

The basic cost consists of \$5 per hour show-up fee and a 40% MTurk fee added to the total cost for every participant. For our study, we develop several scenarios for cost structure are outlined below:

Scenario 1 (Ideal). The design of the original study was not incentive compatible. If we are able to collect as much data as would be needed to reach 80% statistical power and have incentive compatibility, the total cost would be \$470.40. This is based on a total show up fee of \$168, a total maximum incentive of \$168 (\$0.10/each correct answer per participant in the unscrambling task), and a total MTurk fee of \$134.40 (40% of total cost/participant). The breakdown of the cost is presented in the following table:

Scenario 1	
Median time for experiment	10 minutes
Show up fee per participant	\$1.00

Incentive for correct answers in the unscrambling task (assuming that all participants get all of the 10 sentences correct) per participant	\$1.00
MTurk Fee of 40% per participant	\$0.80
Total cost per participant	\$2.80
Total cost for the experiment at 80% power	\$470.40
Total number of participants to achieve 80% power	168
Total participants per treatment to achieve 80% power	42

Table 2. Cost Structure Scenario 1

Scenario 2. Based on deception methods from social psychology (e.g., treating the participants as-if they are paid for all of their decisions), without adhering to incentive compatibility, and taking into account the budget constraints of \$100 for the entire study, we would only be able to collect 17 to 18 participants per treatment group. The breakdown of the cost is presented in the following table:

Scenario 2	
MBDS Funds	\$100.00
Median time for experiment	10 minutes
Show up fee per participant	\$1.00
MTurk Fee of 40% per participant	\$0.40
Total cost per participant	\$1.40
Total number of participants given the MBDS funds	71
Total participants per treatment given the MBDS funds	17 to 18

Table 3. Cost Structure Scenario 2

The cost structure may be modified when we have re-calculated the required sample size using data from the pilot data collection.

Empirical Analysis

Statistical Methods

The experiment will employ a between-subject design, whereby we make comparisons between the four conditions (control [read], original treatment [did not read], religious prime, and superstition prime). We will determine if any observed differences are statistically significant using the commonly used criterion of a p-value that is less than 0.05. This allows us to gauge if our findings are significantly different from expected outcomes if the null hypothesis were true. Determining significance at this level follows the same criteria as both the original Risen & Gilovich (2008) study and the Mathur (2016) replication study.

- **Main evaluation and underlying assumptions**

Financial constraints will limit our sample size compared to the Mathur (2016) replication study. As such, we cannot guarantee assumptions of normality will hold. We will employ non-parametric tests in light of this limitation. This will differ from the ANOVA and pairwise t-test employed in the original Risen & Gilovich (2008) study and the Mathur (2016) replication study. Using non-parametric tests ensures robust findings in the event that the data are not normally distributed.

- **Rules for missing values**

The original study did not mention specific data cleaning or exclusion criteria, though the authors of both the original study and replication study excluded people who appeared to make no effort in the counting task. However, our replication will not include the cognitive load task, so this will not be included as an exclusion criterion. We will follow the replication study's additional criterion of excluding any subjects who submit the survey multiple times. Additionally, subjects who fail to complete the entire experiment, either by failing to answer all questions or by withdrawing, will be excluded from our analyses. Since we are adding two primes, subjects who fail our manipulation check will have their data excluded. Lastly, subjects who fail the attention test in the post-intervention survey will not be included in our analyses.

- **Definition and rules for outliers**

We do not expect to exclude any "outlier" in this experiment since our main outcome variable, perceived likelihood, is judged using a scale from zero to ten, with both being reasonable end-values that participants may select. In addition, for our post-experiment survey, we also do not intend to exclude any outliers due to the nature of our measures that the regret aversion scale, free-will vs. determinism scale, and religiosity are both constructed to reflect the full spectrum of individual beliefs. Thus, consistent with the

analysis procedure in the original study, we are not concerned with the potential influence of extreme values.

Statistical Model

- Main statistical tests

Effect of religious and superstition primes on the tempting fate effect

In the original Risen & Gilovich (2008) study and the Mathur (2016) replication study, the authors conducted two parametric tests, namely a 2x2 ANOVA and pairwise t-tests, that looked at the behavior (had read vs. had not read) and if the cognitive load task was present to compare across conditions to predict the main effect of behavior. Our analysis will differ from the original and replication studies based on our intention to conduct non-parametric tests due to the inability to guarantee assumptions of normality. Therefore, we will conduct a Wilcoxon Rank Sum Test that allows us to compare means in a between-subject design.

- *Effect of Religiosity, Superstition; Free-will vs. Determinism, Regret Aversion as potential mediators*

As we discussed in the earlier section, we set out to investigate the effect of religious and superstition primes on perceived likelihood of negative outcome after tempting fate. However, we also suspect that certain mediating variables might influence the effect of our primes on the outcome variable. First of all, we will use GRS and BIGL scores to obtain baseline measures of religiosity and superstition. Subjects' religiosity and level of superstition may alter the effects of our primes on the outcome, as previous research has demonstrated (Shariff et al., 2016; Zhang et al., 2019). Furthermore, as our extended research questions, we want to investigate the influence of people's free will beliefs as well as their level of regret aversion, measured by the Free Will Score and Regret Scale respectively, on our outcome variable. Since the original study does not include any mediating analyses, we will employ the mediation package in R (Tingley et al., 2014) to examine if these variables indeed mediate the relationship between our main interventions (primes) and the outcome measures, and the degree to which they do so.

- Regression model

The regression model will help us identify and understand demographic variables that we will collect in the post-survey influence our main outcome variable, although we do not expect any statistically significant effects. The simple linear regression model is as shown below:

$$PL = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \beta_3 X_3 + \varepsilon$$

Where

PL = Perceived Likelihood (in religious and superstition primes conditions)

β_0 = Coefficient intercept

β_1 and β_2 = Coefficient estimate

X_1 = Gender

X_2 = Age

X_3 = Religion

ε = Error term

X_i is a set of demographic variables consisting of individual-level covariates. These explanatory variables we will include in our regression are age, gender, and religion. Age is a discrete ratio variable measured in whole years. The variable will range from 18 years old, the minimum age a subject must be to participate in our study, to any age that is reasonable. Gender is measured as a categorical variable. Subjects will specify if they are male, female, or other (with an open-response box for personal identification). Subjects will also indicate their religion identification as a categorical variable. This will allow us to explore the differences in the impacts observed between different subgroups.

Limitations

Due to resource constraints, our proposed study will have some resulting limitations. Firstly, neither the original study, nor the replications adhere to Induced Value Theory (IVT). In our study, due to financial constraints, we will be giving a flat rate show up fee (see Cost Structure Scenario 2), which would not be 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.

Also, the original study had undergraduate students as participants (Risen & Gilovich, 2008). Our replication will be conducted within a more general population using MTurk, and thus the scenario of being 'called on in class' may therefore not be as relatable to our participants. However, since this effect would be seen across all our treatment conditions, we do not think our MTurk sample population will interfere significantly with our results. An additional limitation of conducting the experiment online is that participants have to imagine themselves in certain scenarios, rather than actually experiencing them. This leads to lack of control as the primes and the scenarios may not be as salient.

Given the funding limitations intrinsic to our current work, both our study's sample size and the power of our findings will be reduced. These same constraints may also result in higher attrition

rates by MTurkers due to our limited ability to incentivize their survey participation. We will attempt to mitigate high participant attrition using the strategies outlined in our ‘Projected sample size’ section above. Furthermore, both budgetary and logistical constraints limit our ability to test any potential interaction between religious-priming and superstitious-priming conditions. Though we will include a robustness check to interrogate subjects’ conscious understanding of the priming target – and exclude participants who identified with an incorrect priming allocation – further work could examine whether there could be any interaction between priming conditions, perhaps via a within-subject design.

Lastly, the survey options available within Qualtrics do not allow for a click-and-drag option for our priming activity involving unscrambling themed, scrambled sentences. We were instead required to include free-text answers for this priming exercise. As we have significant time constraints for study completion, we will not be analyzing free-text answers for accuracy, but rather limiting participants’ survey progress using free-text character counts alone. Though we will be interrogating subjects’ priming target recognition, as mentioned above, it would be preferable to perform the unscrambling exercise using more robust controls for answer accuracy.

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Appendices

Appendix A **Informed Consent Form**

You are invited to take part in this study on tempting fate. The purpose of this research study is 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 10 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 10 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

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 B

Bot Check

Before you take the survey, we would like you to answer a question to make sure you are not a “bot”.

January is the first month of the year. What is the fifth month of the year?

- February
- March
- April
- May
- June
- July

Appendix C
Instructions Page

Hello! In the following experiment you will be provided with 10 jumbled-up sentences. You will be required to unscramble them to form new sentences.

They will appear one at a time, and you will be required to complete the previous sentence before moving on to the next one.

Upon completion of this task, you will be required to read a short passage. You will then be asked to answer a series of questions about yourself to complete the experiment.

Click the button below to begin.

Appendix DReligious Beliefs Questionnaire - The General Religiousness Scale - (GRS) (Rowatt et al., 2009)

1. How religious do you consider yourself to be?
 - a. not at all religious
 - b. not too religious
 - c. somewhat religious
 - d. very religious
2. How often do you attend religious services?
 - a. Never
 - b. Less than once a year
 - c. Once or twice a year
 - d. Several times a year
 - e. Once a month
 - f. 2–3 times a month
 - g. About weekly
 - h. Weekly
 - i. Several times a week
3. How often do you read the Bible, Koran, Torah or other sacred book?
 - a. Never
 - b. Less than once a year
 - c. Once or twice a year
 - d. Several times a year
 - e. Once a month
 - f. 2–3 times a month
 - g. About weekly
 - h. Weekly
 - i. Several times a week
4. How often do you pray or meditate outside of religious services?
 - a. Never
 - b. Only on certain occasions
 - c. Once a week or less
 - d. A few times a week
 - e. Once a day
 - f. Several times a day

Appendix E**Superstitious Beliefs Questionnaire - Belief in Good Luck Scale (Darke & Freedman, 1997)**

Indicate the extent of your agreement with the following statements.

- 1-Strongly disagree
- 2-Somewhat disagree
- 3-Slightly disagree
- 4-Slightly agree
- 5-Somewhat agree
- 6-Strongly agree

1. Luck plays an important part in everyone's life.
2. Some people are consistently lucky, and others are unlucky.
3. I consider myself to be a lucky person.
4. I often feel like it's my lucky day.
5. Nobody can win at games of chance in the long-run.
6. I consistently have good luck.
7. I tend to win games of chance.
8. It's a mistake to base any decisions on how lucky you feel.
9. Luck works in my favor.
10. I don't mind leaving things to chance because I'm a lucky person.
11. Even the things in life I can't control tend to go my way because I'm lucky.
12. I consider myself to be an unlucky person.
13. There is such a thing as luck that favors some people, but not others.
14. Luck is nothing more than random chance.

Appendix F
Free Will Slider*

Schooler, J., Nadelhoffer, T., Nahmias, E., and Vohs, K. D. (2014). "Measuring and manipulating beliefs and behaviors associated with free will. the good, the bad, and the ugly," in *Surrounding Free Will: Philosophy, Psychology, Neuroscience*, ed. A. R. Mele (Oxford: Oxford University Press).

I have free will

0 _____ 100
(Strongly disagree) (Strongly Agree)

*starting position of slider will be set on the midpoint (50)

Appendix G**Regret Scale (to measure regret aversion)**

On a scale of 1 to 7, please determine how much you agree or disagree with the following statements, where:

1. Strongly disagree,
2. Disagree,
3. Somewhat disagree,
4. Neither agree nor disagree,
5. Somewhat agree,
6. Agree,
7. Strongly agree

1. Whenever I make a choice, I'm curious about what would have happened if I had chosen differently
2. Whenever I make a choice, I try to get information about how the other alternatives turned out
3. If I make a choice and it turns out well, I still feel like something of a failure if I find out that another choice would have turned out better
4. When I think about how I'm doing in life, I often assess opportunities I have passed up
5. Once I make a decision, I don't look back

Appendix H

Primes

Implicit Control Prime (for conditions 1 and 2):

Unscramble the following groups of words to make four (4)-word phrases or sentence by dropping the irrelevant word:

For example the phrase “high winds the flies plane” would transform to the sentence “ the plane flies high” after dropping the word “winds”

1. Fall was worried she always
2. Shoes give replace old the
3. Retrace good have holiday a
4. More paper it once do
5. Send I over it mailed
6. Saw hammer he the train
7. Yesterday it finished track he
8. Sky the seamless blue is
9. Predictable he shoes his tied
10. Prepared somewhat I was retired

Implicit Religious Prime (for condition 3):

Unscramble the following groups of words to make four (4)-word phrases or sentence by dropping the irrelevant word:

For example the phrase “high winds the flies plane” would transform to the sentence “ the plane flies high” after dropping the word “winds”

1. Felt she eradicate spirit the
2. Dessert divine was fork the
3. Appreciated presence was imagine her
4. More paper it once do
5. Send I over it mailed
6. Evil thanks give god to
7. Yesterday it finished track he
8. Sacred was book refer the
9. Reveal the future simple prophets
10. Prepared somewhat I was retired

Implicit Superstitious Prime (for condition 4):

Unscramble the following groups of words to make four (4)-word phrases or sentence by dropping the irrelevant word:

For example the phrase “high winds the flies plane” would transform to the sentence “ the plane flies high” after dropping the word “winds”

1. Star upon sun a wish
2. Fortune brave the land favours
3. It beginner's love luck is
4. More paper it once do
5. Send I over it mailed
6. Third fun charm a time's
7. Yesterday it finished track he
8. For to meant be it's
9. Take I'll Chase Gamble The
10. Prepared somewhat I was retired

Appendix I

Demographics

We will now ask you a few brief questions.

Please enter your age, in years:

What is your gender?

- Male
- Female
- Other (Open-response)

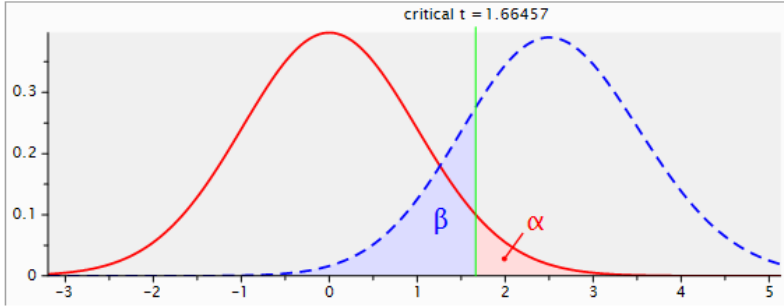
What is your religious affiliation, if any?

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

Appendix J

A Priori G*Power Calculations

Central and noncentral distributions Protocol of power analyses



critical t = 1.66457

Test family: t tests Statistical test: Means: Wilcoxon-Mann-Whitney test (two groups)

Type of power analysis: A priori: Compute required sample size - given α , power, and effect size

Input Parameters

Parameter	Value
Tail(s)	One
Parent distribution	Normal
Determine =>	
Effect size d	0.5635066
α err prob	0.05
Power (1 - β err prob)	0.8
Allocation ratio N2/N1	1

Output Parameters

Parameter	Value
Noncentrality parameter δ	2.5234479
Critical t	1.6645699
Df	78.2140913
Sample size group 1	42
Sample size group 2	42
Total sample size	84
Actual power	0.8041860

☐ $n1 \neq n2$

Mean group 1	0
Mean group 2	1
SD σ within each group	0.5

☒ $n1 = n2$

Mean group 1	2.93
Mean group 2	1.90
SD σ group 1	2.16
SD σ group 2	1.42

Calculate Effect size d: 0.5635066

Calculate and transfer to main window

Central and noncentral distributions Protocol of power analyses

t tests – Means: Wilcoxon-Mann-Whitney test (two groups)

Options: A.R.E. method

Analysis: A priori: Compute required sample size

Input:

Tail(s)	= One
Parent distribution	= Normal
Effect size d	= 0.5635066
α err prob	= 0.05
Power (1- β err prob)	= 0.80
Allocation ratio N2/N1	= 1

Output:

Noncentrality parameter δ	= 2.5234479
Critical t	= 1.6645699
Df	= 78.2140913
Sample size group 1	= 42
Sample size group 2	= 42

Clear Save Print

Test family: t tests Statistical test: Means: Wilcoxon-Mann-Whitney test (two groups)

Type of power analysis: A priori: Compute required sample size – given α , power, and effect size

Input Parameters

Tail(s)	One
Parent distribution	Normal
Determine => Effect size d	0.5635066
α err prob	0.05
Power (1- β err prob)	0.80
Allocation ratio N2/N1	1

Output Parameters

Noncentrality parameter δ	2.5234479
Critical t	1.6645699
Df	78.2140913
Sample size group 1	42
Sample size group 2	42
Total sample size	84
Actual power	0.8041860

☐ n1 != n2

Mean group 1	0
Mean group 2	1
SD σ within each group	0.5

☒ n1 = n2

Mean group 1	2.93
Mean group 2	1.90
SD σ group 1	2.16
SD σ group 2	1.42

Calculate Effect size d: 0.5635066

Calculate and transfer to main window

The following data from Risen and Gilovich (2008)'s Study 6 was used to calculate the effect size:

- Mean Group 1 (had not done the reading): 2.93
- Mean Group 2 (had done the reading): 1.90
- Standard Deviation Group 1 (had not done the reading): 2.16
- Standard Deviation Group 2 (had done the reading): 1.42