

Religion and Self-Control

by

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A thesis submitted to the Department of Psychology  
in fulfillment with the requirements for the degree of  
Doctor of Philosophy

Queen's University

Kingston, Ontario, Canada

September, 2015

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**Abstract**

Unconscious reminders of religion boost people's ability to exercise self-control even after they have completed an earlier self-control task. However, other research has shown that when people initially engage in a task does not involve self-control, people who are high in religiosity or who engage in personal prayer do not engage in greater self-control than do people who are low in religiosity or do not pray. The goal of the current research was to see if first completing a task that does not involve self-control similarly would undermine the positive effects of a religious prime on subsequent self-control and prosocial behavior. In the first three studies, following a task that purportedly required self-control, people who were primed with religious concepts consistently exhibited greater self-control (Study 1) or prosocial behavior (Studies 2a or 2b) on a subsequent task than did people who were primed with neutral concepts. However, after completing a task that should not necessitate self-control, the religious and neutral prime condition participants either did not differ in their performance on a subsequent task (Studies 1 and 2a) or the religious prime participants exhibited less self-control than did the neutral prime participants (Study 2b). In Study 3, a distraction task was administered in between the prime task and the dependent measure. This methodological change was sufficient to yield the religious priming effect consistently found in prior research and the first three studies regardless of whether or not the first task required self-control.

**Co-authorship**

I assumed sole responsibility for the conceptualization, design, and execution of the research reported in this dissertation. In recognition of their assistance with the data collection and writing, my supervisor, Dr. Jill A. Jacobson, will appear as second author and Dr. Albert Lee will appear as third author on the final manuscript.

### **Acknowledgements**

This has been the most difficult journey I have ever embarked. I have sacrificed so much to get here. In hindsight, the strain it caused on my most cherished relationships was not worth it. No degree or amount of accolades is. I say to aspiring PhD students – “Think twice”. And without the support of many I would not have made it.

Foremost, I would like to thank my family and friends for their understanding and patience with my isolation and estrangement. I hope that my absenteeism can be forgiven and I hope we can rekindle our relationships in spite of the elapsed time and distance. I specifically want to thank: My mother who has been one of my biggest cheerleaders and constant source of encouragement and love. Despite your own struggles you never hesitated to listen when I needed an ear and be there for me. I could never have gotten this far without the love, friendship, and support from Dana and Al. Thank you both for encouraging me to stick it out, for allowing me to just pop-in when I needed a break, and for opening up your home and family to me. On that note, I want to thank my Aunt Sue and Al’s family for their love and supporting words of encouragement. Ron, Linda, Erick, and Rebecca have truly withstood the test of blood: I appreciate that you have not yet forsaken me as I have you on many special occasions. I appreciate your opening your home for me while I travelled and did my whirlwind tours of family and friends. To all I say: Trust that my love and thoughts were always with you and my wish was that I could have had more than just fleeting passages of time with you all.

Having special friends that stand the test of time and distance to help support this long and arduous trek was essential. I would be remiss if I did not give a special thank you to Lindsay for her contribution to where I am today – thank you so much for believing in

me and putting up with my neuroticism. And of course I need to thank my friends that have stood by me through thick and thin: the Axis and Allies boys (just the thought of geeking it up was sufficient to motivate me at times), my boyz from Windsor, Montreal, Amherstburg, and Pet. Lastly, I would like to thank my QU friends – Erica and Val especially – as our evenings out were absolutely essential in maintaining my sanity.

I would like to acknowledge my supervisor, Jill, for her support, encouragement, and her help in getting me to this point. I struggled recently to see the light at the end of the tunnel and in this time of need you came through for me. Without your guidance and dedication, I likely would have taken another year to reenergize myself for this finale. I appreciate all the time, understanding, invaluable mentorship, latitude to explore new areas of research, and support you have given over the past several years. I hope it goes without saying that I have learned a great deal from you and will take to heart all that you have tried to teach me. I would also like to thank the members of my committee, Rod and Li-Jun for your constructive criticism, guidance, and experience. Thanks to Chris and Ken sitting on my committee and providing me with valuable feedback and insightful comments that will surely help to make this more publishable. And thank you to those I still consider as being instrumental in getting me to this point –Ken, Danielle, and Janice.

Last, but certainly not least, I would like to thank Angie for opening up her heart, home, and family to me. Thank you for your love, your understanding when I needed to sequester myself, and for your constant enthusiastic encouragement. I feel like we have known each other our whole lives. It is just easy; as it should be. A new path has emerged and I am hopeful as our future looks bright. I am excited to start writing several new chapters with you and Evelyn: a new job, a new relationship, and a new family.

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## Chapter 1: Introduction

Self-control, also referred to as willpower, is the capacity to suppress impulses or personal preferences to bring one's behaviors in line with other, generally more socially acceptable or less selfish, goals and standards. Baumeister and Exline (2000) argued that religion may be a cultural mechanism designed to promote self-control. Certainly, in times of famine and hardship, human beings needed to have some societal mechanism to help promote delayed gratification, prosocial tendencies, and a whole host of other socially adaptive behaviors. Indeed, according to Baumeister, Bauer, and Lloyd (2010), religion can "provide a solution to the self-regulation dilemmas inherent in cultural life; it helps people to control selfish impulses that might harm group interests, to subordinate short-term temptations to long-term goals, to strengthen inner restraints" (p. 67). Consequently, religion may facilitate self-control, which, in turn, plays a crucial role in promoting many adaptive behaviors and tendencies that confer individual and/or group fitness (McCullough & Carter, 2011). For example, religion may help us to control selfish impulses that might harm group interests, to subordinate short-term temptations to long-term goals, and to strengthen inner restraints to overcome some of the hardships of living in large societies.

A great deal of correlational research links religion and self-control (for a comprehensive review, see McCullough and Willoughby, 2009), but recently researchers have attempted to examine the link between religion and self-control empirically. People who are more religious, are implicitly reminded of religious concepts, or engage in personal prayer are more likely to persist at impossible or uncomfortable tasks (Rounding, Lee, Jacobson, & Ji, 2012; Toburen & Meier, 2010; Watterson & Geisler, 2012), are better at a Stroop task (Friese, Schweizer, Arnoux, Sutter, & Wänke, 2014; Friese & Wänke,

2014; Rounding et al., 2012), are better able to resist temptation (Laurin, Kay, & Fitzsimons, 2012), and are more likely to delay gratification (Rounding et al., 2012) than are their corresponding counterparts (i.e., people who either score lower on religiosity, are primed with neutral concepts, or do not engage in prayer). In the current research, we continue to examine the link between religion and self-control to further refine our understanding of the effects of religious priming.

### **Theories of Self-Control**

In social psychology, the dominant theory of self-control for the last decade or more has been Baumeister and his colleagues' resource model of self-control or ego-depletion theory (e.g., Baumeister, 2014; Baumeister, Bratslavsky, Muraven, & Tice, 1998; Baumeister, Vohs, & Tice, 2007). According to this model, self-control is a limited resource that can be exhausted like a muscle after too much exertion. Thus, initial self-control efforts can deplete this resource making it difficult to engage in self-control in the future. Demonstrating an effect referred to as the dual- or sequential-task paradigm, numerous studies have shown that after performing an initial task that required self-control, people performed worse at a subsequent task that also required self-control than did people who had not engaged in prior self-control efforts.

Haggard, Wood, Stiff, and Chatzisarantis (2010) meta-analyzed 83 studies and found considerable evidence in support of the resource model of self-control. In just over a decade and a half, several moderators to the ego-depletion effect also have been explored including spheres of control, implicit theories of self-control, conservation, and training. When participant experience a subjective feeling of mastery over the subject of the self-control tasks, or when tasks elicit the application of heuristics, participants do not exhibit

the typical depletion effect. Moreover, those told they will need self-control on a future task, or are allowed to train for the second task, also are less apt to exhibit depletion. These authors also reviewed several competing theories have been put forward to explain the reduction in task performance over time including skill, fatigue, self-efficacy, and motivation; none of these theories is incompatible with the resource theory of self-control. The latter theory, motivation, has gained traction recently and resulted in a revision of the original ego-depletion theory to account for the possibility that motivation serves an important moderator. Baumeister argues that although self-control is depleted after the initial task, it is not completely depleted or exhausted and moderators like motivation can augment subsequent exertions of self-control (e.g., Muraven & Slessareve, 2003; see Baumeister & Vohs, 2007).

More recently, Carter and McCullough (2013, 2014) and their colleagues (Carter, Kofler, Forster, & McCullough, in press) have conducted a series of meta-analyses that yielded very negative conclusions about the resource model of self-control. They argue that when the meta-analysis is restricted to only the most common operationalizations of self-control (i.e., food temptation tasks, hand grip tasks, possible and impossible anagrams or puzzles, Stroop tasks, standardized tests, and working memory tasks), then the average combined effect size is reduced to a moderate or small effect and there is a significant publication bias (but see Gervais, 2015, and Hilgard, 2015, for criticism of the PET-PEESE method used in these meta-analyses).

However, even before the conflicting meta-analytic results, other studies identified potential problems with or limitations of ego-depletion theory (for more thorough reviews, see Inzlicht & Schmeichel, 2012, in press). For example, Job, Dweck, and Walton (2010)

found that people who believed that self-control was exhaustible showed the typical depletion effect, but people who believed that self-control was unlimited did not perform more poorly on a second self-control task. Other studies have shown that incentives (Muraven & Slessareva, 2003), self-affirmation (Schmeichel & Vohs, 2009), positive mood (Tice, Baumeister, Shmueli, & Muraven, 2007), smoking a cigarette (if a smoker; Masicampo, Martin, & Anderson, 2014), watching television (Derrick, 2013), and meditating (Yusainy & Lawrence, 2015) can counteract the depletion effect. And particularly germane to the current research, temporary religious reminders like a religious prime (Rounding et al., 2012, Study 3) or praying (Friese et al., 2014; Friese & Wänke, 2014) seem to overcome the deleterious effects of an initial self-control task on subsequent self-control.

If self-control is a limited resource, then how do these moderators refuel or maintain it? It seems highly unlikely that holding a different implicit theory or being exposed to words like God and divine could somehow generate more glucose in the brain, which Gaillot et al. (2007) purport to be the biological basis of the resource that is depleted and refueled (but see Beedie & Lane, 2012; Kurzban, 2010; Molden et al., 2012, for arguments against this biological explanation). Even if Baumeister and colleagues' resource model is not dependent on the glucose explanation (see Baumeister, 2014, for a revision that recognizes that glucose is an unlikely biological determinant), the original theory still cannot explain how focusing on one's accomplishments or praying, for example, prevents depletion from occurring or restores the resource so quickly. Moreover, it has a difficult time explaining why minimal expenditures of effort result in measurable downstream effects on the second task (Halali, Bereby-Meyer, & Meiran, 2014).

These inconsistencies have led to alternative accounts of the depletion effect. Inzlicht and Schmeichel's (2012, in press) process or shifting priorities model currently appears to be the best contender because it can reconcile the discrepant findings and subsumes other approaches (e.g., Kurzban, Duckworth, Kable, & Myers, 2013; Mann & Ward, 2007). According to this theory, people perform more poorly on the second of the two self-control tasks because completing the first self-control task causes changes in their motivation (e.g., changes in the opportunity costs of continuing to exert self-control, decreases in willingness to engage in more effort, prioritizing reward over self-control, etc.). Inzlicht and Schmeichel contend that the changes in motivation following self-control exertion are accompanied by shifts in attention, emotion, and cognitive processes as well. For example, gratification cues may become more salient and better remembered than self-control cues.

Because it is quite new, few studies have directly tested the Inzlicht and Schmeichel's (2012, in press) process model of depletion. Empirical support for the role of motivation can be derived from Muraven and Slessareva's (2003) findings that manipulated differences in motivation moderate the depletion effect. Still, no research has yet shown that measurements of self-reported motivation, emotion, etc. are statistical mediators of the effect (Inzlicht & Schmeichel, in press). Recently, however, Job, Bernecker, Miketta, and Friese (in press) found that people's implicit theories about self-control can lead them to adopt a rest goal after an initial self-control task, and such a shift in goal activation is consistent with the basic tenets of the process model.

How would the process model account for the effect of religious primes on self-control? Activating religious concepts may motivate a person to be more persistent or

patient at difficult or impossible tasks. As already mentioned, one reason religion may have developed was to increase people's willingness to engage in self-control (e.g., Baumeister & Exline, 2000). Consequently, ideas about religion and self-control may be closely connected, and thus, priming one construct easily activates the other. Religious concepts also may increase self-monitoring, which is integral to self-control (McCullough & Willoughby, 2009). One conceptualization of God is all knowing or an entity that sees and evaluates one's behavior (Bering & Johnson, 2005), and recent research has shown that even very subtle activation of the feeling of being watched is sufficient to reduce people's selfish behavior (e.g., Bateson, Nettle, & Roberts, 2006; Haley & Fessler, 2005; Bering, McLeod, & Shackelford, 2005).

### **Non-Depleting Tasks**

The process and resource models of self-control focus primarily on the effects of the depletion condition, and little, if any, discussion involves the potential effects of initially completing a task that should not involve self-control (i.e., the non-depletion condition). Of particular relevance to the current research is the third study presented in Rounding et al. (2012). In this study, in the depletion condition, participants retyped a journal abstract on statistics omitting all *es*, *ss*, spaces, and punctuation while listening to a 30-second heavy metal guitar riff played on repeat until the participants completed the task (at least seven experiments have used a similar transcription activity as the initial task; see Carter et al., in press). Next, participants were randomly assigned to complete Shariff and Norenzayan's (2007) scrambled sentence task that contained either neutral or religious words. A control condition also was included in which participants simply retyped the paragraph while listening to the music, and then they completed the neutral scrambled

sentence task. Finally, all participants completed several puzzle tracing tasks (at least 16 experiments have used impossible puzzles as the second self-control task; see Carter et al., in press). Out of the six, only the first two puzzles were solvable and of increasing difficulty, and the measure of self-control on the second task was how long participants persisted on the puzzles. Replicating previous depletion studies, among the neutral prime condition participants, people who completed the initial self-control task spent significantly less time on the puzzles than did participants who completed the initial task that did not require self-control. Rounding et al. also found that participants who completed the initial depletion task and were exposed to a religious prime persisted significantly longer on the puzzles than did participants who completed the same initial self-control task but were exposed to a neutral prime. In fact, the performance of the participants in the depletion, religious prime condition was on par with participants in the non-depletion, neutral prime condition.

Rounding et al. (2012, Study 3) examined only three conditions (depletion paired with either a neutral or religious prime and non-depletion with a neutral prime), but Watterson and Geisler (2012) and both studies by Friese and his colleagues (Friese et al., 2014; Friese & Wänke, 2014) included a non-depletion condition paired with either high religiosity or personal prayer. First, Watterson and Geisler (2012) used a 2 (religiosity: high versus low) x 2 (depletion versus not) design. Replicating Rounding et al., Watterson and Geisler found that among participants whose self-control resources initially were depleted (using a handgrip task), high religiosity participants spent significantly more time on a subsequent impossible anagram task than did low religiosity participants. Unexpectedly, among participants who simply sat in the same room as the handgrip (i.e.,

participants who completed an initial non-depleting task), high religiosity participants did not differ from low religiosity participants in their persistence on the anagram task. Moreover, high religiosity participants in the non-depletion condition also did not differ in their persistence on the anagrams from high religiosity participants in the depletion condition.

Second, both Friese et al. (2014) and Friese and Wänke (2014) used a 2 (prayer vs. free thought) x 2 (depletion versus not) design. The two studies differed in the nature of the depletion task (thought suppression or emotion suppression) and the timing of the depletion manipulation (before or after the prayer manipulation), but the results were the same in both studies. Replicating Rounding et al. (2012) and Watterson and Giesler (2012), Friese and his colleagues found that among participants in the depletion condition, people in the prayer condition exhibited significantly less Stroop interference than did participants in the free thought condition. Similar to Watterson and Giesler (2012), among participants assigned to the non-depletion task, people in the prayer and free thought conditions did not differ in their Stroop performance. Moreover, depleted and nondepleted participants in the prayer condition also did not differ in their Stroop performance.

Some have argued that a difference should not be expected in the non-depletion condition because “it is difficult to improve self-control above baseline levels in the short-term” (Friese et al., 2014, p. 91; see also Schmeichel & Vohs, 2009, p. 771, for nearly the exact same wording). Indeed, almost all of the studies that used the sequential task paradigm to investigate moderators of the depletion effect have failed to find a significant difference between the moderator and control conditions when participants' initial task was

intended to be non-depleting (e.g., Gailliot et al., 2007; Muraven & Slessareva, 2003; Schmeichel & Vohs, 2009; Tice et al., 2007).

However, in the three studies conducted by Rounding et al. (2012), one study conducted by Laurin et al. (2012, Study 4), and one study by Toburen and Meier (2010) in which no initial task was completed, exposure to religious primes consistently yielded greater self-control than did exposure to neutral primes. One would expect the same pattern if participants completed an initial task that did not involve self-control because in both cases, participants' self-control resources should not have been affected. Indeed, one reason we did not include a non-depletion, religious prime condition in Rounding et al.'s Study 3 is because we did not think it was necessary to show a fourth time that religious reminders bolstered self-control over neutral primes when people's self-control was not depleted. Unfortunately, by not including that condition, we do not know if we would replicate the null results in the non-depletion condition observed in Watterson and Giesler (2012) and the two studies by Friese and his colleagues (2014) or if religiosity and personal prayer differ in some important way from religious priming.

As a preliminary attempt to address this question, we collected data from 23 new participants ( $M_{\text{age}} = 18.61$ ,  $SD_{\text{age}} = 1.34$ ; females = 7) all of whom were assigned to a non-depletion, religious prime condition. Using planned contrasts, we then compared their responses to archival data from Rounding et al. (2012, Study 3). Among the participants in the non-depletion condition, contrary to our predictions, people in the religious prime condition did not spend more time on the puzzle task ( $M = 973$  s) than did people in the neutral prime condition ( $M = 696$  s),  $t(79) = 0.21$ ,  $p = .83$ . Furthermore, among the religious prime participants, people in the non-depletion and depletion ( $M = 1,186$  s)

conditions did not differ,  $t(79) = -1.49, p = .14$ . Thus, similar to Watterson and Geisler's (2012) findings for religiosity and Friese et al.'s (2014) results for prayer, we did not find a benefit of religion priming on self-control when people first completed a task that did not involve self-control.

This preliminary attempt relied on archival data for our comparison conditions. Consequently, one potential alternative explanation for our results is simply that the participants in the new data collection were different in some important way from the earlier participants. Indeed, the new data collection did occur much later in the school year than did Rounding et al. (2012), which may or may not be associated with differences in students and their performance in experiments (Ebersole et al., 2015). In addition, although we did not find that gender yielded any significant interactions, the proportion of men (65%) in the newly collected data was much higher than in the conditions from our archival data, which were on average around 31%. Still, perhaps we did not think to measure some other variable on which our new and earlier participants differed that could account for our results. To resolve these issues, we embarked on a series of studies to better determine how and why completing an initial mundane task might affect the relationship between primed religious constructs and self-control.

### **The Current Research**

In the current research, we sought to further examine the effects of a religious prime on self-control when participants initially engage in a non-depletion task. We wanted to determine if first completing a non-depletion task yields the same results that we and others obtained for religious priming when no initial task was completed. This pattern of results would be expected if the non-depletion task truly does not involve self-control, but it would

be counter to the findings obtained when other operationalizations of religion were used. One problem in comparing the results of the previous religion and self-control studies is that religious primes, religiosity, and personal prayer may function differently, which could contribute to the potential discrepancies between the studies. Religiosity and personal prayer likely do not activate similar religious concepts as do religious primes; The goal of the present series of studies is to specifically examine the effect of religious primes on self-control in a non-depletion task. Consequently, for all of the studies presented below, we decided to use at least a 2 (Prime: Religious vs. Neutral) x 2 (Initial Task: Depletion vs. Non-Depletion) design.

We assumed that an initial non-depleting task should function similarly to not completing any initial task because in both cases, participants have not engaged in a previous exertion of self-control. Furthermore, Rounding et al. (2012, Studies 1, 2, and 4), Laurin et al. (2012, Studies 3, 4 and 5), and Toburen and Meier (2010) all found that religious primes yielded greater self-control when no initial task was completed. Therefore, we expected that, contrary to the findings for religiosity and personal prayer (and our initial data collection reported above), a religious prime should yield greater subsequent self-control regardless of the nature of the initial task.

In sum, we expected to replicate Rounding et al. (2012, Study 3) and find that among participants who engaged in an initial self-control task, people in the religious prime condition should engage in more self-control on a second task than should people in the neutral prime condition, and they also should engage in similar levels of self-control as neutral prime participants who completed an initial non-depletion task. Finally, to address our core research question for the current studies, we expected that religious-reminded

participants who first completed a non-depletion task should exert significantly more self-control than participants in any of the other three conditions.

**Prosocial behavior.** A secondary goal of the current research was to extend our findings into the realm of prosocial behavior. DeWall, Baumeister, Gailliot, and Maner (2008) argued that acting in a prosocial manner requires the exertion of self-control. Accordingly, research showing that religious-reminders boost prosocial behavior (for a review and meta-analysis, see Shariff, Willard, Andersen, & Norenzayan, in press, but for a meta-analysis with very different conclusions, see Gomes & McCullough, in press<sup>1</sup>) actually could be evidence that religion boosts self-control because acting unselfishly or in a prosocial manner would necessitate exerting self-control. Indeed, compared to people who are not depleted, depleted participants are less likely to help strangers by donating food or money or by volunteering their time (DeWall et al., 2008, but see Janssen, Fennis, Pruyn, & Vohs, 2008, who found that depleted people donated more to a charity).

After Study 1, we switched our dependent measure from performance on a self-control task to cooperative behavior. We expected the same basic pattern of results for prosocial behavior that we predicted for self-control: People in the religious prime condition should be more cooperative than people in the neutral prime condition especially when the latter participants first complete a self-control task.

**Overview of the procedure.** To activate religious concepts without conscious awareness, each experiment presented below used an implicit priming procedure developed by Shariff and Norenzayan (2007) and used by Rounding et al. (2012) and Toburen and

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<sup>1</sup> Gomes and McCullough (in press) report a failed replication of Shariff and Norenzayan (2007, Study 2) plus a meta-analysis of all studies that examined the effects of a religious prime on monetary distributions. They concluded that religious primes do not affect prosocial behavior on such tasks; however, their conclusions are largely based on the results of the PET-PEESE method, which, as mentioned above, may not yield valid conclusions (Gervais, 2015; Hilgard, 2015).

Meier (2010). In this task, participants unscramble each of 10 five-word sentences by dropping an irrelevant word. Half of the sentences contained only neutral words, and the remaining five contained religious target words such as *God*, *spirit*, or *divine* (e.g., “dessert divine was for the” becomes “the dessert was divine”). In a neutral/control condition, all of the target words were neutral in valence and did not pertain to any shared construct (e.g., “sky the seamless blue is” becomes “the sky is blue”). Before starting this task, participants learned there were no incorrect answers, but that each sentence should make logical sense. Participants also were instructed to reflect on each sentence before moving on to ensure the sentences were grammatically correct.

Across all experiments reported below, participants were mostly female (74.5%) and of European (72.5%) or East Asian (12.4%) descent. The majority of participants reported being Christian (47.6%), Agnostic (22.6%), or Atheist (13.6%). See Table 1 for demographic information for each study. As in Rounding et al. (2012), the proportions of religious denominations were equal across all conditions, and neither age nor gender interacted with the prime or depletion manipulations. Additionally, level of religiosity as measured by the Brief Multidimensional Measure of Religiosity and Spirituality (BMMRS; Idler et al., 2003) did not interact with the manipulations; therefore, the effect of prime condition did not differ depending on whether someone was a theist or atheist,  $F_s \leq 2.09$ ,  $p_s \geq .11$ . Consequently, we will not discuss these variables further.

Table 1. Demographic and sample characteristics for all studies.

Characteristics		<i>Study 1</i>		<i>Study 2a</i>		<i>Study 2b</i>		<i>Study 3 Pilot</i>		<i>Study 3</i>		<i>Total</i>
Age	<i>Mean</i>	18.0		19.4		18.3		18.2		36.5		22.1
	<i>Standard Dev.</i>	0.72		8.50		0.74		1.27		10.77		4.40
Characteristics		<i>N</i>	<i>%</i>	<i>N</i>	<i>%</i>	<i>N</i>	<i>%</i>	<i>N</i>	<i>%</i>	<i>N</i>	<i>%</i>	<i>%</i>
Sex												
	Female	61	79.2	67	73.6	70	76.9	231	81.3	92	59.0	74.5
	Male	16	20.8	24	26.4	21	23.1	53	18.7	64	41.0	25.5
Ethnicity												
	African	3	3.9	1	1.1	2	2.2	2	0.7	9	5.8	2.4
	East Asian	16	20.8	5	5.5	11	12.1	51	18.0	4	2.6	12.4
	European	52	67.5	46	50.5	70	76.9	213	75.0	126	80.8	72.5
	Middle Eastern	0	0.0	0	0.0	1	1.1	4	1.4	1	0.6	0.9
	South Asian	3	3.9	2	2.2	3	3.3	0	0.0	0	0.0	1.1
	Other/Missing	3	3.9	37	40.7	4	4.4	14	4.9	16	10.3	10.6
Religious Denomination												
	Agnostic	20	26.0	23	25.3	23	25.3	62	21.8	30	19.2	22.6
	Atheist	12	15.6	11	12.1	13	14.3	42	14.8	17	10.9	13.6
	Buddhist	3	3.9	2	2.2	2	2.2	7	2.5	3	1.9	2.4
	Christian	27	35.1	39	42.9	36	39.6	144	50.7	87	55.8	47.6
	Hindu	1	1.3	1	1.1	1	1.1	1	0.4	3	1.9	1.0
	Jewish	4	5.2	5	5.5	7	7.7	10	3.5	3	1.9	4.1
	Muslim	1	1.3	1	1.1	2	2.2	8	2.8	5	3.2	2.4
	Sikh	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0	0.0
	Other/Missing	9	11.7	9	9.9	7	7.7	10	3.5	8	5.1	6.2

*Notes:* Mean = mean age of study sample. Standard Dev. = standard deviation of study sample mean. N = total number of participants, per study, per category. % = percent of total sample, per study, per category.

## Chapter Two: Study 1

In this study, we used the same basic procedure as Rounding et al. (2012, Study 1) but added an initial task that was either depleting or non-depleting. Specifically, we used a 2 (religious vs. neutral prime) X 2 (depletion vs. non-depletion) design with tolerance of discomfort (operationalized as drinking foul-tasting beverages) as our dependent measure. Again we predicted that religious reminders would augment self-control, regardless of the depletion condition.

### Method

#### Participants

In this study, participants ( $N = 77$ ) primarily indicated being either atheist/agnostic (43.3%) or Christian (35.1%), of European (67.5%) or East Asian (20.8%) descent, female (79.2%), and approximately 18.0 years old ( $SD = 0.72$ ). Consistent with the denominational data, participants' self-reported religiosity measured during a prescreening session at the beginning of the year was below the mid-point ( $M = 1.56$ ,  $SD = .50$ ; scale from 1, *strongly disagree*, to 4, *strongly agree*). Three participants were excluded from the non-ego-depletion condition because they indicated feeling greater self-efficacy after being successful at the puzzle task. An additional 10 were excluded from the ego-depletion condition for a similar reason: they indicated wanting to “make up” for their failure by drinking more of the juice. Three participants were excluded because they mathematically determined that the ego-depletion puzzles were impossible to complete. No participant indicated knowledge of the influence of the religious prime. The same pattern of results emerged when including all participants, and therefore, we report on the data including all participants.

**Procedure**

The experimenter told participants that they would complete two separate studies. Before they started these studies, they engaged in a warm-up task consisting of a few puzzles that were designed “to get [them] thinking.” These tracing tasks required participants to retrace two geometric shapes without lifting their pencils from the paper and without drawing over any line more than once. Participants were randomly assigned to complete either an easy or impossible puzzle-tracing task and given up to 5 minutes to complete the task. The impossible tracing task had no solutions, and participants were interrupted after 5 minutes and told to continue to the next task. Next, participants were randomly assigned to either the religious or neutral priming task.

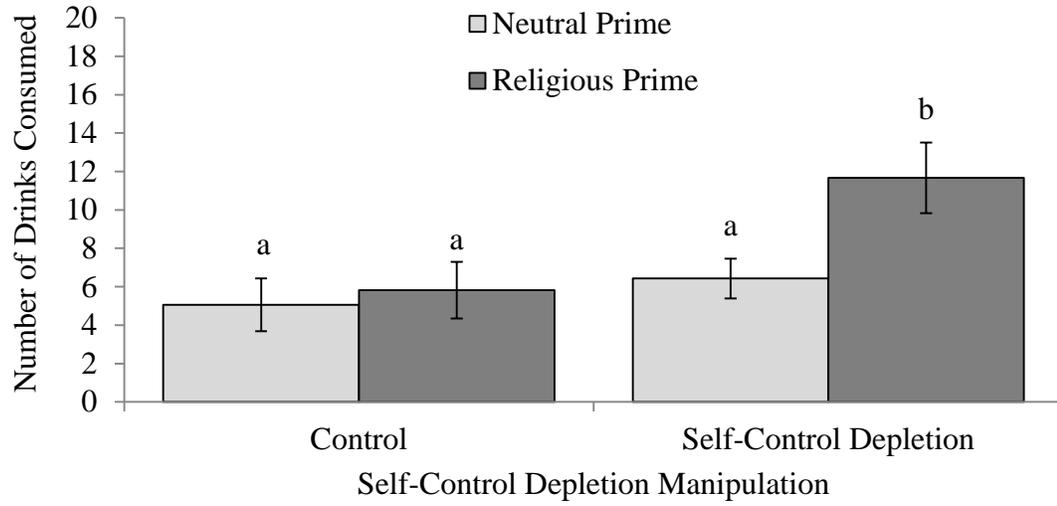
After completing the scrambled sentence task, participants engaged in the same unsavory drink task described in Rounding et al. (2012). For this task, all participants were directed to a table that held 20 small cups, each of which contained 1 oz of a mixture of orange juice and vinegar. Participants learned that this second experiment was an investigation of motivation. They further were told that each cup contained a mixture that most people find unsavory, that all the mixtures were identical, that we would pay participants a nickel for each ounce they drank, and that they could stop at any time. After the experimenter left the room, participants were given unlimited time to drink as many of the cups as they liked. Once finished, participants summoned the experimenter back into the room, and the experimenter then recorded the number of ounces each participant had drank. This number was our measure of self-control, with greater consumption representing greater exertion of self-control. After participants completed a measure of suspicion, they

were compensated with either partial course credit or \$5, a nickel for each ounce of juice drank, were fully debriefed, and thanked for their time.

### Results and Discussion

To examine differences in the number of drinks consumed, we conducted a 2 (Prime: Religious vs. Neutral) x 2 (Initial Task: Depletion vs. Non-Depletion) analysis of variance (ANOVA). See Figure 1. Overall religious prime participants ( $M = 9.05$ ,  $SD = 7.92$ ) consumed significantly more drinks than did neutral prime participants ( $M = 5.82$ ,  $SD = 5.17$ ),  $F(1, 73) = 4.117$ ,  $p = .046$ , and participants in the depletion condition ( $M = 8.93$ ,  $SD = 7.25$ ) consumed significantly more drinks than did participants in the non-depletion condition ( $M = 5.45$ ,  $SD = 5.72$ ),  $F(1, 73) = 5.968$ ,  $p = .017$ . Although the interaction was not significant,  $F(1, 73) = 2.291$ ,  $p = .134$ , we conducted a series of planned contrasts to examine the number of drinks consumed as a function of the prime and the initial ask. Replicating Rounding et al. (2012), among participants in the depletion condition, people in the religious prime condition consumed significantly more of the unsavory drinks ( $M = 11.67$ ,  $SD = 8.40$ ,  $n = 21$ ) than did people in the neutral prime condition ( $M = 6.43$ ,  $SD = 4.98$ ,  $n = 23$ ),  $t(73) = 2.705$ ,  $p = .008$ ,  $d = .76$ . They also consumed more than did their religious prime counterparts in the non-depletion condition ( $M = 5.82$ ,  $SD = 6.07$ ,  $n = 17$ ),  $t(73) = 2.795$ ,  $p = .007$ , and furthermore, the depleted, religiously reminded participants consumed more of the drinks than all of the other participants combined,  $t(73) = 3.581$ ,  $p = .001$ . Still consistent with findings for religiosity and prayer and the extension of Rounding et al. (2012, Study 3) described above, among participants in the non-depletion condition, people in the religious prime condition did not differ from people in the neutral prime condition ( $M = 5.06$ ,  $SD = 5.50$ ,  $n = 16$ ),  $t(73) = 0.341$ ,  $p = .734$ .

Participants in the neutral prime condition did not differ between the non-depletion and depletion conditions,  $t(73) = 0.658, p = .513$ . Thus we may have reason to doubt the effectiveness of our initial depletion task. Still at least one other study that examined moderators of depletion effects also failed to find a significant difference between the depletion and non-depletion conditions in the control condition and did not interpret this null effect as a problem (cf. Muraven & Slessareva, 2003, Study 3). In the current study, perhaps the retracing task in the intended non-depletion condition was sufficiently taxing that self-control also was required or perhaps the intended depletion task was not taxing enough. Carter et al. (in press) identified 16 experiments that have used impossible puzzles, but in those studies, this activity was always the second rather than the first task. Thus maybe the effect of the impossible puzzles task is affected by the timing of the task. We do know that the two puzzle tasks must have differed in some way because the effectiveness of the religious prime, or at least its association with subsequent self-control, did differ depending on the first task. To try to rectify this problem, in subsequent studies, we used a different task for the depletion versus non-depletion conditions. Also, from this point on, we changed our dependent measure to a cooperative task similar to the one used by Sharif and Norenzayan (2007) to further demonstrate that expenditure of self-control affects later cooperative behavior.



**Figure 1.** Mean number of drinks consumed as a function of condition. Conditions sharing subscripts are not significantly different at  $p < .05$  level.

### **Chapter Three: Studies 2a and 2b**

Aligned with idea that religion may have emerged as an adaptation to promote prosocial behavior via greater self-control, we sought to test if religious reminders would boost cooperative behavior after initial self-control resources were depleted. We hypothesized that if self-control is necessary to engage in prosocial behavior as DeWall et al. (2008) found, then among participants in the neutral condition, people who first completed a depletion task should exhibit lower levels of prosocial behavior than would people who first completed a non-depletion task. We also hypothesized that among participants who completed a depletion task, people in the religious prime condition would exhibit more cooperative behavior than would people in the neutral prime condition. Indeed, overall we expected that participants who are reminded of religious concepts would be more cooperative regardless of the depletion condition.

Potential problems with the depletion manipulation and dependent measure in our first attempt (Study 2a) led us to conduct a conceptual replication (Study 2b) with a different depletion manipulation and some changes to the dependent measure. Because the design and purposes of the two studies were so similar, we have presented the details about both studies together below.

### **Method**

#### **Participants**

**Study 2a.** Of the 97 participants who completed Study 2a, three participants were excluded because they guessed the hypothesized relationship between the religious words and the cooperative game, and three more were excluded because they indicated having conscious recognition of the primed words, as operationalized as having recalled more than

two prime words.<sup>2</sup> Thus, our final sample for Study 2a ( $N = 91$ ) consisted of moderately religious participants ( $M = 4.46$ ,  $SD = 1.95$ ; scale from 1 = *strongly disagree* to 8 = *strongly agree*), with an average age of 19.41 years ( $SD = 8.50$ ). The pattern of results was the same when including all participants.

**Study 2b.** Of the 113 participants who completed Study 2b, two were excluded because they indicated not having understood the cooperation task directions properly and believed their partner could request more than the available \$50. Four participants were excluded because they guessed the hypothesized relationship between the religious words and the cooperative game, and two more were removed because they indicated having just experienced a significant emotional event just prior to completing the study. And lastly, participants indicated having conscious recollection of the primed words ( $n = 14$ ), as operationalized as having recalled more than two prime words, were excluded from the analyses. The remaining participants in Study 2b ( $N = 91$ ) were moderately religious ( $M = 3.86$ ,  $SD = 1.62$ ; scale 1, *strongly disagree*, to 8, *strongly agree*), with an average age of 18.30 years ( $SD = 0.74$ ).

## Procedure

Participants were led to believe that this study consisted of two separate experiments: one on language usage that included two tasks (the depletion task and the scrambled sentence task) and the other on economic distribution. In Study 2a, the depletion task was a thought-listing task (at least 17 experiments have used thought suppression as the first self-control task [Carter et al., in press] as did Friese et al. [2014] in their study on the effects of personal prayer). Participants were randomly assigned to write

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<sup>2</sup> All participants who recalled at least two primed words listed God as one of the words. We believe that God may be too obvious of a prime-word and consequently decided to include participants who recalled that prime-word only.

their thoughts for five minutes while either not thinking about a white bear (depletion) or with no white bear instructions (non-depletion). Participants in the depletion condition indicated on a piece of paper each time they inadvertently thought of a white bear, which was an average of 2.88 times ( $SD = 2.96$ ) and significantly different from zero,  $t(59) = 7.54, p < .001$ . Study 2b used the same depletion task as Rounding et al. (2012, Study 3). Participants randomly assigned to the non-depletion condition simply retyped a paragraph, whereas participants in the depletion condition also had to omit all *es*, *ss*, spaces, and punctuation. To make the task more demanding, the depletion participants also listened to loud music while they typed.

When time expired for the depletion task, participants were randomly assigned to either religious or neutral scrambled sentence task. Once participants finished unscrambling the sentences, they moved on to the supposed second study on economic distribution. In this pool-resource task, participants learn that they have been randomly paired with another participant who is completing the same study (cf. Shariff & Norenzayan, 2007; Sosis & Ruffle, 2003). To maintain the guise and ensure that participants could not collude with a friend in the same session, participants always were led to believe that at least three people were present for each session.

Participants were informed that they and their anonymous partner had joint access to an envelope containing either \$25 in Study 2a or \$50 in Study 2b. They were told that they could request as much or as little money as they would like from this envelope. If the sum total of both their request and their partner's request exceeded the amount in the envelope (i.e., was greater than \$25 in Study 2a or \$50 in Study 2b), then neither would receive any money. If not, they would each receive the amount they requested. However,

if any money remained in the envelope, it would be doubled and split evenly between the two partners. Thus, greater cooperation would be exemplified by leaving more money (requesting less) in the envelope. Participants learned that their decision would remain confidential, and because it was recorded on the computer, even the experimenter would not know of their decision. To further maintain the deception, participants were told that on the following day the experimenter would tally the responses and inform participants as to when they could return to pick up their money (if applicable). Once participants read the instructions and made their decision using a sliding scale from \$0 to \$25 in Study 2a or \$50 in Study 2b in \$1 increments, they then answered suspicion questions, were fully debriefed, and compensated.

## Results and Discussion

### Study 2a

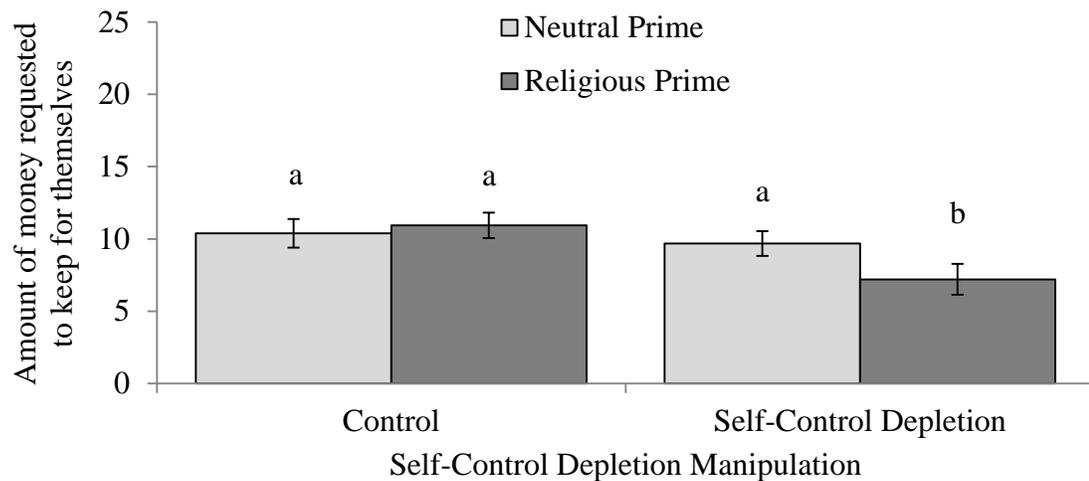
To examine differences in the amount of money requested, we conducted a 2 (Prime: Religious vs. Neutral) x 2 (Initial Task: Depletion vs. Non-Depletion) ANOVA and only the main effect of task was significant,  $F(1, 87) = 4.963, p = .028$  (all other  $ps > .325$ ). See Figure 2. Consistent with our hypothesis that religious reminders would bolster prosocial behavior, among participants in the depletion condition, people in the religious prime condition requested significantly less money from the shared envelope ( $M = 7.20, SD = 5.32, n = 25$ ) than did participants in the neutral prime condition ( $M = 9.68, SD = 4.76, n = 31$ ),  $t(87) = 1.992, p = .049, d = .49$ . They also requested less money than did their religious prime counterparts in the non-depletion condition ( $M = 10.94, SD = 3.60, n = 17$ ),  $t(87) = 2.572, p = .012$ , and furthermore, religious prime participants in the depletion condition exhibited significantly more prosocial behavior than all of the other conditions

combined,  $t(87) = 2.857, p = .005$ . However, once again consistent with previous religion and self-control research and Study 1 above, among participants in the non-depletion condition, religious prime participants did not differ from the neutral prime participants ( $M = 10.39, SD = 4.19, n = 18$ ),  $t(87) = 0.353, p = .725$ .

As in Study 1, it is difficult to put these results in perspective because once again we have evidence that our depletion manipulation was not effective. Specifically, among the neutral prime participants, people in the non-depletion and depletion conditions did not differ in how much money they kept,  $t(87) = 0.519, p = .605$ . Still we should note that as in Study 1, despite the lack of differences in the neutral prime condition, religious prime participants who were in the intended depletion condition did behave differently and in the expected direction of being more cooperative. It again remains unclear why the religious prime has a positive effect in the depletion condition but not the non-depletion condition if the depletion manipulation does not yield differences among the control participants.

As before, it is not clear if the failure to find a significant difference among the neutral prime participants is due to the non-depletion condition being too taxing or the depletion condition not being taxing enough. Anecdotally, a number of participants in the depletion condition indicated that the white bear task was not very difficult or mentally draining. We did not expect this reaction because Friese et al. (2014) did find a significant difference among their control participants for the depletion manipulation using this same task, and in the meta-analysis by Carter et al. (in press), only two other activities (an attention video and crossing out letters) have been used more frequently as the initial depletion task. Still for Study 2b, we decided to return to a depletion task, the transcription task, which has worked effectively with our samples in our past research.

Alternatively, the null effect could just mean that, contrary to our hypothesis and the findings of DeWall et al. (2008), self-control does not play a role in prosocial behavior. Across conditions, several participants indicated the sum of money being offered was of little consequence to them. Thus perhaps the problem was not with the depletion manipulation but with the dependent measure. That is, we might have set up a situation in which resisting selfish motives was too easy. To address this problem, we changed the amount of money for the cooperation task to \$50 in Study 2b to make the amount potentially more consequential to our participants and thus more tempting.

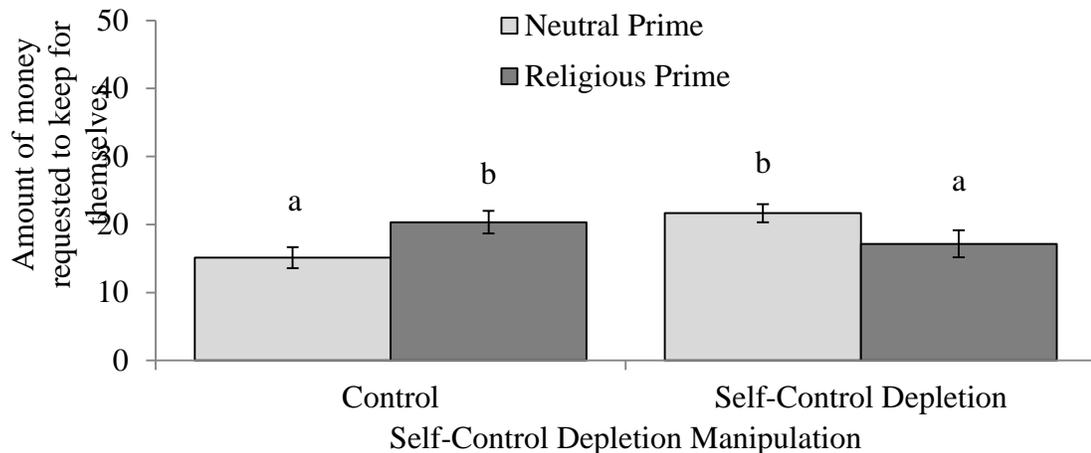


**Figure 2.** Mean amount of money requested to keep for themselves as a function of condition. Conditions sharing subscripts are not significantly different at  $p < .05$  level.

### Study 2b

To examine differences in the amount of money requested, we conducted a 2 (Prime: Religious vs. Neutral) x 2 (Initial Task: Depletion vs. Non-Depletion) ANOVA, and only the interaction was significant,  $F(1, 87) = 8.269, p = .005$  (all other  $ps > .325$ ). See Figure 3. Consistent with our hypothesis and Study 2a, among participants in the depletion condition, people in the religious prime condition requested significantly less

money ( $M = 17.15$ ,  $SD = 8.92$ ,  $n = 20$ ) than did participants in the neutral prime condition ( $M = 21.67$ ,  $SD = 6.86$ ,  $n = 27$ ),  $t(87) = 1.966$ ,  $p = .052$ ,  $d = .57$ , and among the religious prime participants, people in the depletion condition and the non-depletion condition ( $M = 20.33$ ,  $SD = 6.40$ ,  $n = 15$ ) did not differ in the amount of money requested,  $t(87) = 1.197$ ,  $p = .235$ .



**Figure 3.** Mean amount of money requested to keep for themselves as a function of condition. Conditions sharing subscripts are not significantly different at  $p < .05$  level.

Moreover, contrary to the studies on religiosity and prayer and Studies 1 and 2a, we did find a significant difference between the religious and neutral prime participants in the non-depletion condition; however, the pattern was the opposite of the depletion condition results. That is, not only did we fail to show that religion boosted prosocial behavior in the non-depletion condition, but among participants who completed the non-depletion task first, people in the religious prime condition kept significantly more of the money for themselves (i.e., were significantly *less* cooperative) than were people in the neutral prime condition ( $M = 15.14$ ,  $SD = 8.37$ ),  $t(87) = 2.098$ ,  $p = .039$ ,  $d = .66$ . In other words, when not depleted, being reminded of religious concepts led to greater greed rather than greater

cooperation in Study 2b. However, because we did not replicate this effect in Study 2a, we caution against drawing too many conclusions for this single result.

Finally, unlike Study 1 and 2a, we have evidence that the depletion manipulation was successful because among the neutral prime participants, people in the depletion condition requested significantly more money (i.e., were less prosocial) than were people in the non-depletion condition,  $t(87) = 3.136, p = .002, d = .84$ . These results also are consistent with previous research that self-control depletion affects subsequent prosocial behavior.

**Chapter Four: Study 3**

Why would purportedly simple tasks, such as retyping a paragraph or sitting in a room with a handgrip, undermine the effect of religion on self-control? To apply Inzlicht and Schmeichel's (2012, in press) process or shifting priorities model, clearly something is occurring in the non-depletion condition to undermine the motivating effects of the religious prime in the current research and the other moderators of the depletion effect in past research. One possibility is that the non-depletion tasks often are very boring, and after engaging in several minutes of such tedious work, participants simply may disengage from the experiment before the primary dependent measure is presented. Alternatively, first completing a mindless task may make participants approach all subsequent tasks, including the priming task, in a mindless fashion. Consequently, religion is not activated and/or not seen as applicable to the final task, and concepts that are both available and accessible will not be utilized in subsequent decision making if they are not seen as relevant (Higgins, 1996; Schwarz, Bless, Wänke, & Winkielman, 2003).

Another possibility is specific to the methodology of the religion priming procedure. The majority of past literature using the religious prime, including Rounding et al. (2012), presented it as a warm-up task (anecdotally, if not in print), whereas in the current studies, we adopted a procedure more similar to other priming research and presented the scrambled sentence task as an ostensibly separate experiment. Furthermore, in Rounding et al. (2012), the experimenter returned in between the prime task and the presentation of the dependent variable to give instructions for the final task. In the current studies, although participants were told the tasks were separate, a computer automatically

advanced them from the prime task to the dependent variable task, and no overt attempts were made to emphasize that the activities were distinct studies.

Perhaps participants drew a link between the prime and the second task and attempted to correct for the influence of the prime. This conclusion is consistent with past work on priming in general, which has shown that people who are still aware of the primed material or see a connection between the prime and dependent variable tasks contrast away from the primed concepts, attempting to correct for the influence of the primed material on their responses (Bargh & Chartrand, 2000).

### **The Current Research**

In this study, we explored the methodological explanation for the null findings in the non-depletion condition by reverting back to the procedure used for the prime task in Rounding et al. (2012). In previous religious priming research, participants were told the prime was a warm-up task, whereas in Studies 1, 2a, and 2b, they were told that it was a separate experiment. Also, in Rounding et al. (2012), participants completed the priming material and then called the experimenter back into the room to receive verbal instructions for their final task. Thus, participants were distracted from the primed material, albeit shortly, to listen to a scripted set of instructions before continuing on to complete the dependent variable. However, in Studies 1, 2a, and 2b, the computer gave instructions for the next task and automatically advanced participants to the next series of studies. Before we present the information on Study 3, we first discuss a pilot study conducted to determine the nature of the methodological changes required.

### **Pilot Study**

In this pilot study, we omitted the depletion condition, so we could better

understand the role of the methodological differences discussed above on our findings.

## Method

**Participants.** Participants ( $N = 318$ ) were recruited from an Introductory Psychology class and compensated with partial course credit. Eleven percent of the sample were excluded from the final analyses because they either: guessed the hypothesis ( $n = 5$ ), reported significant life events prior to the experiment that may have affected their responses ( $n = 3$ ), recalled two or more prime words ( $n = 2$ ), experienced a computer malfunction that resulted in no music being played or the program freezing mid-way through the experiment ( $n = 12$ ), or were  $\pm 3$  SD away from the mean on the dependent variable ( $n = 12$ ). The remaining 284 participants were only slightly religious ( $M = 1.86$ ,  $SD = 0.91$ ; scale 1, *not at all religious*, to 4, *very religious*), with an average age of 18.2 years ( $SD = 1.27$ ).

**Procedure.** Participants were randomly assigned to one of three conditions: warm-up task/interruption, warm-up task/no interruption, or separate task/interruption. In the warm-up task conditions, the prime was presented as a warm-up task “designed to get you ready for the upcoming experiment” before presenting the dependent measure task. Of the participants in the warm-up conditions, half were assigned to the Interruption instructions and told to call the experimenter back into the room when they finished with the prime task to receive further instructions for the study. Upon returning to the room, the experimenter, blind to the hypotheses, collected the prime material and read the following script:

You’re all finished? OK, this experiment is on economic distributions. In this experiment, we are interested in how people make decisions involving anonymous

partners. Everything is entirely on the computer, so please just follow the instructions on the screen. If you have any questions, please let me know.

For the other half of the warm-up condition participants, these instructions were presented on the screen adapted for computer vernacular.

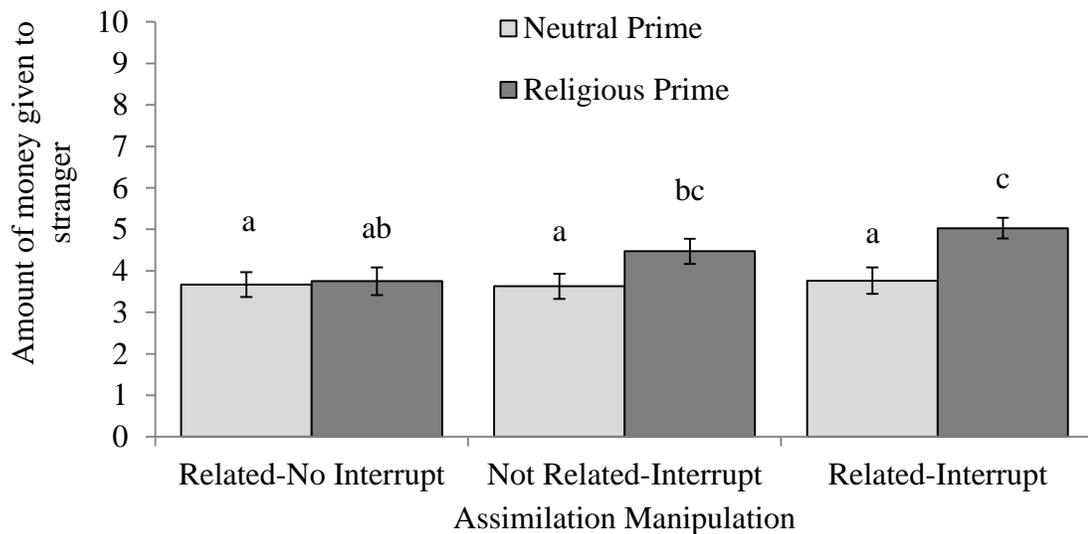
Participants in separate task/interruption condition were informed that they would take part in two different studies (prime task and dependent measure). In between the two studies, they were to let the experimenter know they were finished, and the experimenter read the same script as above but with the following minor deviation to maintain consistency with the cover story: “You’re all finished? Do you have any questions about that experiment? No? OK, this second experiment is on...”

For the dependent measure, participants completed the dictator game (Kahneman, Knetsch, & Thaler, 1986). We opted to utilize a different, more popular, measure of prosocial behavior to address concerns that religious reminders may make people more cooperative but not necessarily more prosocial in the sense of altruistic giving to an anonymous person. The dictator game is frequently used measure of prosocial behavior (Levitt & Dubner, 2009). In it, participants were presented with \$10 and asked to make a decision whether or not to give any amount of that \$10 to an anonymous stranger that will complete the experiment the following day. They were told they could keep as much or as little of the money for themselves as they wanted and to indicate on the computer how much they would like to give (if any) to this anonymous partner. Once participants indicated their decision, they completed the BMMRS, suspicion questions, and were debriefed.

## Results and Discussion

We conducted a 3 (Instructions: Warm-up Task-No Interruption, Separate-Interruption, vs. Warm-up Task-Interruption) x 2 (Prime: Neutral vs. Religious) ANOVA. See Figure 4. Participants in the religious prime condition ( $M = 4.37$ ,  $SD = 2.13$ ) gave significantly more money to the stranger than did participants in the neutral prime condition ( $M = 3.68$ ,  $SD = 2.14$ ),  $F(1, 278) = 8.277$ ,  $p = .004$ , and no overall difference was observed as a function of the instruction manipulation ( $p = .100$ ). Although the interaction was not significant,  $F(2, 278) = 1.825$ ,  $p = .163$ , we used planned contrasts to examine the prime differences as a function of the instruction conditions. Among the Warm-Up Task/Interruption condition participants, people in the religious prime condition gave more to their partner ( $M = 5.03$ ,  $SD = 1.48$ ) than did people in the neutral prime condition ( $M = 3.76$ ,  $SD = 2.07$ ),  $F(1, 278) = 6.915$ ,  $p = .009$ ,  $d = 0.69$ . Among the Warm-Up Task/No Interruption condition participants, people in the religious prime condition ( $M = 3.75$ ,  $SD = 2.30$ ) did not differ significantly in the amount of money given than did people in the neutral prime condition ( $M = 3.67$ ,  $SD = 2.20$ ),  $F(1, 278) = 0.039$ ,  $p > .843$ ,  $d = .04$ . Furthermore, among the religious prime participants in the two Warm-Up conditions, people who were in the Interrupt condition were more generous than were people in the No Interrupt condition,  $t(278) = 2.91$ ,  $p = .005$ ,  $d = 0.64$ . Among the Separate Task/Interruption condition participants, people in the religious prime condition ( $M = 4.47$ ,  $SD = 2.22$ ) gave more money than did participants in the neutral prime condition ( $M = 3.63$ ,  $SD = 2.17$ ),  $F(1, 278) = 4.124$ ,  $p = .043$ ,  $d = 0.38$ . None of the other comparisons were significant,  $F_s < 0.048$ ,  $p_s > .84$ .

In sum, providing an interruption between the prime and the dependent variable is the key experimental difference to replicating Rounding et al. (2012) who observed the beneficial effect of the religious prime on the second task. Even if the prime is presented as a warm-up task following the procedure of Shariff and Norenzayan (2007), the religious and neutral primes do not yield different effects if the computer automatically moves the participant to the dependent measure portion of the study.



**Figure 4.** Mean amount of money given to an anonymous stranger as a function of condition. Conditions sharing subscripts are not significantly different at  $p < .05$  level.

One alternative explanation is that it was not the interruption caused by having to call the experimenter back into the room that made the difference but rather further interaction between the participant and the experimenter or demand was key. Experimenters knew what prime condition participants were in because they had to provide the right scrambled sentence sheet, but they did not know the specific hypotheses. To address this issue, we conducted Study 3 online and used an interruption task that did not require any social exchange.

## Method

### Participants

We recruited 191 participants from MTurk and paid each \$2 for approximately 30 minutes of their time. Eighteen percent of the total sample were excluded from the final analyses because they either: experienced error on their computer resulting in no music being played during the depletion manipulation ( $n = 2$ ), correctly guessed the hypotheses ( $n = 2$ ), consciously recalled two or more prime words and indicated that religious words were predominant in the scrambled sentence task ( $n = 4$ ), had participated a similar study by the authors or one utilizing similar methodology and/or prime materials ( $n = 16$ ), spent 19 hours and 59 seconds on the study ( $n = 1$ ), or were  $\pm 3 SD$  from the mean on the dependent variable ( $n = 10$ ). The remaining 156 participants were moderately religious ( $M = 2.14$ ,  $SD = 1.10$ ; scale 1, *not at all religious*, to 4, *very religious*), and 36.5 years old ( $SD = 10.78$ ).

### Measures

**Brief Multidimensional Measure of Religiosity/Spirituality.** The Brief Multidimensional Measure of Religiosity/Spirituality (BMMRS; Idler et al., 2003) is a measure of R/S that taps into eight orthogonal dimensions, such as daily spiritual experiences, gratitude, collaborative religious coping (both positive and negative), forgiveness, religious or spiritual meaning, religious commitment, organized religious practices, and religious self-ranking. Two dimensions (religious commitment and gratitude) were single-variable indices. Scores on the BMMRS were summed across these dimensions revealing an internally reliable total score ( $\alpha = .96$ ; see Fetzer National Institute on Aging [NIA], 1999 for the psychometric properties of the scale). Higher scores indicate

higher levels of R/S. Previous research has shown that the BMMRS is effective in measuring R/S across religious denominations (Fetzer NIA, 1999).

### **Procedure**

Those responding to a request to take part in a short 30 minute study on MTurk were redirected to a Qualtrics Survey where participants learned that they would take part in two separate studies: One examining verbal abilities when distracted (the depletion manipulation) and the other examining economic distributions (the prime and dependent measure). After providing informed consent, participants completed the same procedure as described in Study 2a, with one exception: (1) the prime was presented as a warm-up task (i.e., part of the economic distributions study), and (2) a distraction task was introduced between the prime and the dependent measure. For the distraction task, participants to rated several aspects of the scrambled sentence tasks (e.g., the prose, simplicity in solving them, whether their answers were in first or third person) and then for two minutes, they provided directions from their home to their usual grocery store or their place of work (cf. Weary, Jacobson, Edwards, & Tobin, 2001, Study 3; Jacobson, Weary, & Lin, 2008, Study 3). Once finished, participants indicated their decision for the \$25 cooperation game used previously in Study 2a, and then completed the BMMRS, suspicion questions, and were debriefed.

## **Results and Discussion**

### **Manipulation Check**

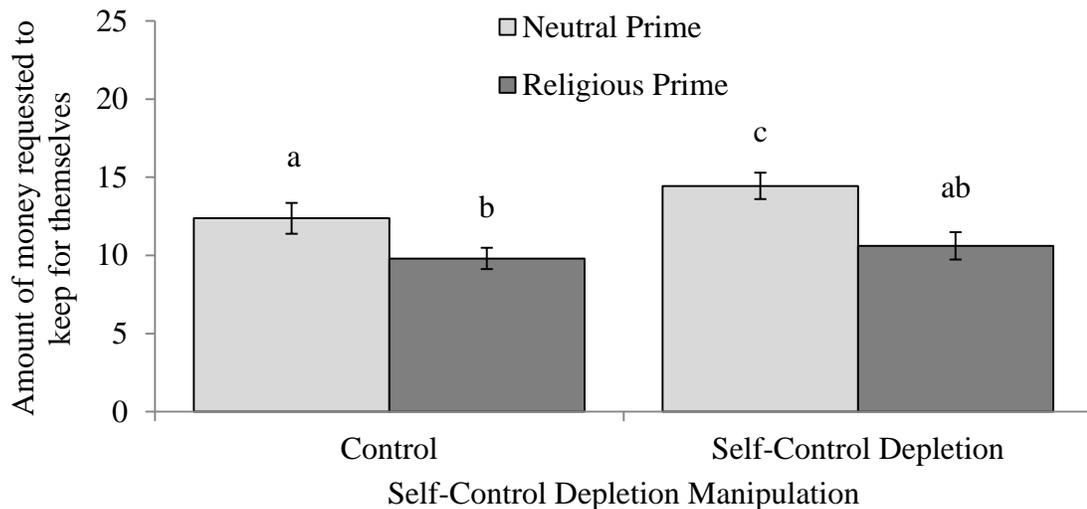
Because we were not able to determine if the null results in the non-depletion conditions in Studies 1 and 2a were due to the non-depletion and depletion tasks not being sufficiently different in difficulty, we added a manipulation check in Study 3 to gather

information on how the tasks were perceived. Using a scale from (1) *Extremely Easy* to (5) *Extremely Difficult*, participants in the non-depletion condition reported the task as significantly less difficult ( $M = 3.21, SD = 0.95$ ) than participants in the depletion condition ( $M = 4.00, SD = 0.92$ ),  $F(1, 151) = 27.201, p < .001, \eta^2 = .153$ .

### **Primary Dependent Measure**

To examine differences in the amount of money requested, we conducted a 2 (Ego-Depletion: Easy Retyping vs. Hard Retyping) x 2 (Prime: Neutral vs. Religious) ANOVA. See Figure 5. As we predicted, overall religious prime participants ( $M = 10.22, SD = 5.06$ ) requested significantly less money than did neutral prime participants ( $M = 13.34, SD = 5.75$ ),  $F(1, 152) = 13.712, p < .001, \eta^2 = .083$ . Although neither the depletion main effect,  $F(1, 152) = 2.763, p = .099$ , nor the depletion by prime interaction effect,  $F(1, 152) = 0.528, p = .469$ , were significant, we conducted a series of planned contrasts, so we could more directly compare our results to Studies 1, 2a, and 2b. Among participants in the depletion condition, people in the religious prime condition ( $M = 10.62, SD = 5.69$ ) requested significantly less money than did people in the neutral prime condition ( $M = 14.44, SD = 4.98$ ),  $F(1, 152) = 9.648, p = .002, d = 0.71$ , and among the religious prime participants, people in the depletion condition were as prosocial as were people in the non-depletion condition ( $M = 9.81, SD = 4.32$ ),  $F(1, 152) = 0.456, p = .501$ . Further evidence that the methodological changes can account for the null effects in the non-depletion condition in Studies 1 and 2a, we found that among non-depleted participants, people in the religious prime condition requested significantly less money than did people in the neutral prime condition ( $M = 12.38, SD = 6.25$ ),  $F(1, 152) = 4.505, p = .035, d = 0.47$ .

Unfortunately, as in Studies 1 and 2a, we have some evidence that our depletion manipulation was not successful because among the neutral prime participants, people in the non-depletion condition ( $M = 12.38$ ,  $SD = 6.25$ ) did not differ from people in the depletion condition ( $M = 14.44$ ,  $SD = 4.98$ ),  $F(1, 152) = 2.743$ ,  $p = .100$ . The difference is that this time the null effect was obtained using an initial self-control task that had been successful in Study 2b and our previous research. Furthermore, the manipulation check question supported that the non-depletion task was perceived as being easier than the depletion task. We will address this problem further in the General Discussion.



**Figure 5.** Mean amount of money requested to keep for themselves as a function of condition. Conditions sharing subscripts are not significantly different at  $p < .05$  level.

### **Chapter Five: General Discussion**

Our primary goal was to examine the effect of religious priming on self-control after participants initially engage in a non-depletion task. In prior research when no initial task was completed (e.g., Laurin et al., 2012; Rounding et al., 2012; Sharif & Norenzayan, 2007; Toburen & Meier, 2010), people exposed to religious primes engaged in greater self-control or prosocial behavior than did people exposed to neutral primes. Thus, we expected that after engaging in an initial task that should not involve self-control, we would again see a positive relationship between activating religious concepts and self-control or prosocial behavior. Instead, we found that first completing a task that supposedly did not involve self-control undermined the positive effects of religion on self-control. Across our first three studies, when participants first completed a task that should not have been depleting, people in the religion and neutral prime conditions either did not differ significantly on a subsequent task (Studies 1 and 2a) or the religious prime participants actually exhibited less self-control than the neutral prime participants (Study 2b). These results are largely consistent with Watterson and Giesler (2012) who found no difference between high and low religiosity participants in their non-depletion condition and with Friese and his colleagues (Friese et al., 2014; Friese & Wänke, 2014) who found no difference as a function of the prayer manipulation in the non-depletion condition. Indeed, as already discussed above, the vast majority of studies on moderators of the depletion effect have not shown a difference between the moderator and control conditions when the first task does not involve self-control.

In our first three studies, however, when we added the initial task, we changed our instructions and procedure slightly to try to incorporate the new initial task into our

experimental cover story. In Study 3, when we returned to labeling the prime task as a warm-up task for the dependent measure and also took steps to ensure separation between the prime material and the dependent variable task by adding a distractor task, then we were able to replicate the religious priming effect we had observed in the depletion condition in the non-depletion condition as well. The importance of including such a task or break between the prime and the dependent measure has been well established for over a decade because it lessens the likelihood that participants will see a link between the prime and their responses (cf. Bargh & Chartrand, 2000).

Bargh and Chartrand (2000) argue that a distraction task decreases conscious awareness of the link between the primed concepts and dependent variable. In sequential-task paradigms, we speculate that the sequential and automated nature by which participants are progressing through the experiment may have resulted in the contrasting effect that saw the religious primed material being recognized and then discounted in their prosocial decision making. We propose that religious beliefs may be a sensitive topic and especially susceptible to socially desirable responding, such that when participants are consciously aware of the link between the prime and the dependent variable, they contrast away from it. However, we submit that if participants engage in a sufficiently difficult dependent variable (e.g., solving for anagrams or completing voluminous Stroop tasks) they may draw on the activated religious concepts as being informational or as having utility.

Indeed, according to more recent views on priming like Loersch and Payne's (2014) situated inference model, people must misattribute the source of the prime content to themselves for assimilation to occur, and such misattributions are more likely to occur

when steps are taken to ensure that people do not perceive a connection to the priming task. Hence, this model can explain why a distraction task was necessary for assimilation to occur: The distraction task helps to diminish the connection between the priming task and subsequent salient religious concepts. Perhaps the reason that some studies have failed to find an effect of religious priming or failed to replicate previous studies is because they have deviated from standard priming procedure. For example, Gomes and McCullough (in press) who did not replicate Shariff and Norenzayan (2007) presented all of their materials on a computer that, according to their Method section, moved participants directly to the economic game as soon as they finished with the priming task.

Although we did find it was necessary when no prior task was completed (Study 3 Pilot Experiment), the methodological changes were not necessary to yield assimilation to the religious prime in the depletion condition (Studies 1, 2a, 2b, and 3). According to Loersch and Payne's (2014) situated inference model, factors like cognitive load or multi-tasking, which have been used in other research to operationalize the use of self-control, can make the effects of a prime stronger again because they make it more likely for people to misattribute the primed content to themselves. Hence, in the depletion condition, it was not necessary to take extra steps to separate the prime task from the dependent measure because misattribution of the source of the primed content already was facilitated by self-control exertion.

The interpretation that a difficult task may tax cognitive resources allowing for assimilation is also consistent with the literature on assimilation and contrast. Schwarz and Bless (2007) argue that the application of filters that determine whether information is utilized or applied in subsequent decision making require attention and cognitive capacity.

They argue that “assimilation effects are more likely than contrast effects under conditions of low cognitive resources” (p. 19). Thus, after completing a cognitively and self-regulatory difficult depletion task, participants simply do not possess the cognitive capacity to ask themselves whether the primed religious concepts are irrelevant or are redundant.

These data may also suggest that there is something special about including the supposedly inert non-depletion task that psychologically modifies the experience for the participants. We speculate that menial tasks preceding the prime may decrease motivation, attention, or trigger the conservation of self-regulatory resources. On the other hand, a particularly challenging dependent task could cause an increase in motivation, attention, or trigger the expenditure of self-regulatory resources.

### **Possible Mechanisms**

The process model suggests that ego-depletion effects could be a result of changes in attention, emotion, and/or cognitive processing (Inzlicht & Schmeichel, 2012, in press), and the religious prime likely operates through one or more of these mechanisms. For example, religious concepts often are associated with thoughts of supernatural or omniscient Gods that are watching over our actions (Norenzayan & Shariff, 2008), which could elicit self-monitoring, a critical component of self-regulation. Furthermore, most religious tenets espouse goals or standards of conduct, some of which are related to cooperative and prosocial tendencies (McCullough & Willoughby, 2009; McCullough & Carter, 2011). Consequently, the religious prime may motivate participants to behave in a cooperative manner in fear of offending God. Along the same lines, religious beliefs often prescribe ultimate and abstract goals that are related to salvation and damnation (Geyer &

Baumeister, 2005), and religious reminders may shift attention towards these goals motivating individuals to act in accordance with them.

In addition, participants completing repeated simple tasks may experience heightened positive mood derived from the ease with which they accomplished the tasks (Schwarz & Bless, 2007). Biss and Hasher (2011) found that when participants were primed with positive mood, they incorporated more peripheral information into their recall of events. Thus positive mood may make religious concepts more salient and, thus, more likely to be corrected for. Alternatively, positive mood could alter attention such that the focus for the remainder of the experiment is on gratifying their hedonistic desires yielding less cooperation in the non-depletion condition.

From a cognitive processing perspective, subtle religious reminders could elicit religious conceptions related to work ethic or morality and reorient the perceived cost-benefit ratio of certain tasks (Geyer & Baumeister, 2005). For example, participants may perceive additional benefit to working hard (i.e., protestant work ethic) or costs associated with not working hard (i.e., not fulfilling their obligation to the researcher). Alternatively, religious concepts are associated with long-range perspectives and delayed gratification, which should facilitate the exertion of self-control (Baumeister et al., 2010).

### **Self-Control and Cooperative Behavior**

Finally, across four data collections, we consistently found that after first completing a task that involved self-control, people who were primed with religious themes exercised greater self-control, including greater cooperative behavior, than did people primed with neutral concepts. These results replicated Rounding et al. (2012, Study 3). Based on the consistency of our current and past research, we can conclude that when

people first engage in a task that involves self-control, people engage in less self-control or prosocial behavior on a subsequent task, but one reliable moderator of this pattern is exposure to religious concepts, which yield greater self-control or prosocial behavior under these conditions.

Some people may argue that exercising self-control to engage in a cooperative task is not the same as acting in a prosocial manner, and that the type of cooperation task we utilized in these studies is not indicative of prosocial behavior because of the economic advantage to cooperating. The type of cooperation game utilized in these series of studies belongs to the category of common-pool resource dilemmas in which two (or more) individuals can consume resources until depletion, and maintaining the common-pool requires self-control (Sosis & Ruffle, 2003). We chose this type of cooperation task because it is aligned with evolutionary arguments for the origins of religion.

That is, religion may have emerged as a cultural adaptation that evolved to promote self-control, which would have been crucial for the development and sustainability of a society that is dependent upon agriculture and subsistence living (Baumeister & Exline, 2000; Norenzayan & Shariff, 2008). Religion may have evolved as a solution to the dilemmas inherent in cultural life because religion helps people control impulses that are harmful to evolutionary fitness and to subordinate short-term for long-term goals (Baumeister et al., 2010). In other words, human beings had to have some societal mechanism to help promote delayed gratification and cooperative behavior as we pooled resources, making self-restraint more important (McCullough & Willoughby, 2009). Thus, although cooperating has an economic advantage in our studies, self-restraint still is needed

to overcome initial selfish urges to request more money from the common-pool and to quell desires to receive immediate rewards.

Additionally, that religious reminders boosted giving in the dictator game, despite not utilizing an ego-depletion paradigm, provides additional evidence that these data are, indeed, indicative of prosocial behavior and not simply a tendency to engage in cooperative behavior when economic advantages are salient. Still more evidence is needed to elucidate the effect on self-control across various prosocial paradigms before we could conclude definitely that prosocial behavior necessitates the utilization of self-control.

### **Limitations**

In three of the current studies, the depletion manipulation did not yield differences among the control participants indicating that our manipulation of depletion may not have been effective. Unfortunately, this is not the first time that we have found that established depletion tasks did not seem to be difficult and/or lengthy enough to actually deplete self-control resources in our participants. For instance, in piloting for Rounding et al. (2012), we used a transcription task that had worked previously in other self-control research conducted in our lab (Passey, 2009). However, just a few years later, we found that it was necessary to add distracting music and create more rules for the task to be sufficiently depleting. Perhaps our samples differ in some important way from the participants in other studies that have effectively used the same manipulations as the ones in the current study. Alternatively, maybe these tasks need to change over time. For instance, people text more than they used to, so it might not be as taxing to type words that are missing letters because they practice that all of the time. Also, the obsessive use of smart phones has increased, so

multi-tasking might not require as much self-control as it once did because now it is a habit.

Although the sample sizes in the current research were not very large, they were two to three times larger than the number of participants in some previous research in this area (e.g., Laurin et al., 2012; Rounding et al., 2012; Shariff & Norenzayan, 2007). Furthermore, the beneficial effect of the religious prime was replicated in all of our comparisons, except for the first three studies in the non-depletion condition, and other researchers have replicated this positive effect of religion on a variety of dependent measures and using other operationalizations of religion. Indeed, in their meta-analysis of religious priming studies, Shariff et al. (in press) found a small-to-medium average effect of the difference between the religious prime and neutral/control groups, Hedges'  $g = 0.40$ ,  $p < .0001$ . They also did a  $p$ -curve that indicated that the effect of religious priming contains evidential value,  $\chi^2(134) = 201.98$ ,  $p = .0001$ , and is not due to  $p$ -hacking or publication bias.

Finally, Ritter and Preston (2013) pointed out that in religious priming studies, little attention has been given to exactly what representation of religion or God is being activated. This criticism also can be levied at our research. We do not know what aspect of the religious prime is driving our effects -- notions of religion or God, some other theistic or spiritual belief, or potentially some other related concepts that are not religious per se. Because over a third of our sample self-reported as agnostic or atheist, we likely our activating a more cultural-based religious concepts. That is, if the concepts being activated were familiar only to the devout, we would not expect the non-theists to respond to the prime.

**Conclusion**

Consistent with recent theorizing such as Loersch and Payne's (2014) social inference model, initial exertions of self-control increase the use of the primed religious constructs even when extra steps are not taken to ensure a connection is not made between the prime and response. However, under typical conditions (i.e., no prior task or prior task that does not require self-control), religious priming is subject to the same conditions that are known to be important for assimilation in priming studies, such as the necessity of disrupting the link between the prime task and the dependent measure. Although this methodological account is derived from priming research, future research should examine if adding a distractor task or other disruption could apply to at least some other moderators of self-control depletion effects for which null effects also have been observed in the non-depletion condition (e.g., self-affirmation, prayer, mood, etc.).

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Appendix A – GREB Approval



August 08, 2011

Mr. Kevin Rounding  
Master's Student  
Department of Psychology  
Queen's University  
Kingston, ON K7L 3N6

**GREB Romeo ref. #: 6005412**  
**Title: GPSYC-495-10 Religiosity as a Mechanism for Self-Control**

Dear Mr. Rounding,

The General Research Ethics Board (GREB) has reviewed and approved your request for renewal of ethics clearance for the above-named study. This renewal is valid for one year from September 7, 2011. Prior to the next renewal date you will be sent a reminder memo and the link to ROMEO to renew for another year.

You are reminded of your obligation to advise the GREB of any adverse event(s) that occur during this one year period. An adverse event includes, but is not limited to, a complaint, a change or unexpected event that alters the level of risk for the researcher or participants or situation that requires a substantial change in approach to a participant(s). You are also advised that all adverse events must be reported to the GREB within 48 hours. Report to GREB through either ROMEO Event Report or Adverse Event Report Form at <http://www.queensu.ca/ors/researchethics/GeneralREB/forms.html>

You are also reminded that all changes that might affect human participants must be cleared by the GREB. For example you must report changes in study procedures or implementations of new aspects into the study procedures. Your request for protocol changes will be forwarded to the appropriate GREB reviewers and / or the GREB Chair. Please report changes to GREB through either ROMEO Event Reports or the Ethics Change Form at <http://www.queensu.ca/ors/researchethics/GeneralREB/forms.html>

On behalf of the General Research Ethics Board, I wish you continued success in your research.

Yours sincerely,

A handwritten signature in cursive script that reads "Joan Stevenson".

Joan Stevenson, Ph.D.  
Professor and Chair  
General Research Ethics Board

c.c.: Dr. Jill Jacobson, Supervisor  
Albert Lee, Graduate Student, Co-investigator  
Dr. Leandre Fabrigar, Chair, Unit REB  
Marie Tooley, Dept. Admin.

**Appendix B – Scrambled Sentence Tasks**

**Neutral Scrambled Sentence Task**

**Instructions:**

Unscramble the following groups of words to make a four word phrase or sentence by dropping the irrelevant word. For example,

high winds the flies plane → the plane flies high

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 \_\_\_\_\_

**Religious Scrambled Sentence Task**

**Instructions:**

Unscramble the following groups of words to make a four word phrase or sentence by dropping the irrelevant word. For example,

high winds the flies plane → the plane flies high

1. felt she eradicate spirit the \_\_\_\_\_
2. dessert divine was for 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 \_\_\_\_\_

**Appendix C – Brief Multidimensional Measure of Religiosity and Spirituality**

Instructions:

The following questions deal with possible spiritual experiences. To what extent can you say you experience the following:

- |  |   |                       |
|--|---|-----------------------|
| 1. I feel God’s presence.  | 1 | Many times a day      |
| 2. I find strength and comfort in my religion.   | 2 | Every day             |
| 3. I feel deep inner peace or harmony.   | 3 | Most days             |
| 4. I desire to be closer to or in union with God.                                      | 4 | Some days             |
| 5. I feel God’s love for me, directly or through others.                               | 5 | Once in a while       |
| 6. I am spiritually touched by the beauty of creation.                                 | 6 | Never or almost never |
| 7. I feel thankful for my blessings.   |   |                       |
| 8. I believe in a God who watches over me.   | 1 | Strongly agree        |
| 9. I feel a deep sense of responsibility for reducing pain and suffering in the world. | 2 | Agree                 |
| 10. The events in my life unfold according to a divine or greater plan.                | 3 | Disagree              |
| 11. I have a sense of mission or calling in my own life.                               | 4 | Strongly disagree     |

Instructions:

Because of my religious or spiritual beliefs:

- |   |   |                         |
|---|---|-------------------------|
| 12. I have forgiven myself for things that I have done wrong. | 1 | Always or almost always |
| 13. I have forgiven those who hurt me.                        | 2 | Often                   |
| 14. I know that God forgives me.                              | 3 | Seldom                  |
|   | 4 | Never                   |

Instructions:

The following questions deal with possible spiritual experiences. To what extent can you say you do the following:

- |  |   |                        |
|--|---|------------------------|
| 15. How often do you pray privately in places other than at church or synagogue? | 1 | More than once a day   |
| 16. Within your religious or spiritual tradition, how often do you meditate?     | 2 | Once a day             |
| 17. How often do you watch or listen to religious programs on TV or radio?       | 3 | A few times a week     |
| 18. How often do you read the Bible or other religious literature?               | 4 | Once a week            |
|  | 5 | A few times a month    |
|  | 6 | Once a month           |
|  | 7 | Less than once a month |
|  | 8 | Never                  |

19. How often are prayers or grace said before or after meals in your home?
- 1 At all meals
  - 2 Once a day
  - 3 At least once a week
  - 4 Only on special occasions
  - 5 Never

Instructions:

Think about how you try to understand and deal with major problems in your life. To what extent is each of the following involved in the way you cope?

20. I think about how my life is part of a larger spiritual force.
21. I work together with God as partners.
22. I look to God for strength, support, and guidance.
23. I feel that stressful situations are God's way of punishing me for my sins or lack of spirituality.
24. I wonder whether God has abandoned me.
25. I try to make sense of the situation and decide what to do without relying on God.
26. To what extent is your religion involved in understanding or dealing with stressful situations in any way?
- 1 A great deal
  - 2 Quite a bit
  - 3 Somewhat
  - 4 Not at all
- 1 Very involved
  - 2 Somewhat involved
  - 3 Not very involved
  - 4 Not involved at all

Instructions:

These questions are designed to find out how much help the people in your congregation would provide if you need it in the future.

27. If you were ill, how much would the people in your congregation help you out?
28. If you had a problem or were faced with a difficult situation, how much comfort would the people in your congregation be willing to give you?
- 1 A great deal
  - 2 Some
  - 3 A little
  - 4 None

Instructions:

Sometimes the contact we have with others is not always pleasant.

29. How often do the people in your congregation make too many demands on you?
- 1 Very often

30. How often are the people in your congregation critical of you and the things you do?
- 2 Fairly often
  - 3 Once in a while
  - 4 Never

Instructions:

The following questions deal with your current and historical religiosity/spirituality:

31. Did you ever have a religious or spiritual experience that changed your life?
- 1 No
  - 2 Yes
- IF YES: How old were you when this experience occurred?
32. Have you ever had a significant gain in your faith?
- 1 No
  - 2 Yes
- IF YES: How old were you when this occurred?
33. Have you ever had a significant loss in your faith?
- 1 No
  - 2 Yes
- IF YES: How old were you when this occurred?
34. I try hard to carry my religious beliefs over into all my other dealings in life.
- 1 Strongly agree
  - 2 Agree
  - 3 Disagree
  - 4 Strongly disagree
35. How often do you go to religious services?
- 1 More than once a week
  - 2 Every week or more often
  - 3 Once or twice a month
  - 4 Every month or so
  - 5 Once or twice a year
  - 6 Never
36. Besides religious services, how often do you take part in other activities at a place of worship?
37. What is your current religious preference?  
IF Protestant: Which specific denomination is it?
38. To what extent do you consider yourself a religious person?
- 1 Very religious
39. To what extent do you consider yourself a spiritual person?
- 2 Moderately religious
  - 3 Slightly religious
  - 4 Not religious at all

**Appendix D – Instructions for Studies****Studies 1 & 2**

You will now take part in a series of two different studies. In each of these short studies you may be asked to complete some psychological measures. You may have seen some of these questionnaires before, but we ask that you complete them again anyway.

In between each study you will see a page indicating that you are switching to the next study. At this stage, please take a minute to refocus on this new task.

You should see a piece of paper face down on your desk. Please keep this facing down until you are asked to turn it over.

*When you are ready to start, please click "Continue"...*

You will now take part in the first in this series of short experiments.

In this experiment on word usage, we are studying how people use words naturally in a sentence. Unlike most experiments that constrain people to writing a paragraph on a specific topic, and then analyzing how they use words in sentence form, your task will be to generate your own sentences.

*When you are ready to start, please click "Continue"...*

Now it is time to turn your attention to the piece of paper on your desk. Please flip it over.

On this page you will notice some scrambled sentences with blanks next to them. Using the space provided to write your answer, try to unscramble the sentences

in the most logical manner by dropping the irrelevant word. There is no right or wrong answer per se, but the unscrambled sentences should make sense logically.

*When you are finished with each of the problems and have verified that each makes sense, please click "Continue"...*

### **Study 3**

PARTICIPANT: I understand that I have been asked to participate in a couple of different tasks. I will be asked to retype a paragraph while listening to distracting music, play a scrambled sentence game, and be asked to make an economic decision involving an anonymous partner. I will also complete some personality measures in each session. This experiment will take a maximum of one hour to complete.

*When you are ready to start, please click ">>"...*

You have now completed Study 1. You will now take part in STUDY 2 of this multi-study session. Please take a moment to refocus yourself and prepare for the next study.

*When you are ready to proceed, please click ">>"...*

Before you start this experiment, you will complete a warm-up task. This task is to prepare you for the upcoming study.

In this task, you will see sets of 5 words at a time. You should try to unscramble these words into a grammatical 4-word sentence as quickly as you can. You can type the

grammatical sentence in the space provided. There is no right or wrong answer per se, but the sentences should make sense logically.

*When you are finished with each of the problems and have verified that each makes sense, please click "Continue"...*