Who is Sensitive to Religious Priming, and How? Factoring Context and Participant Background into the Effects of Priming on Prosocial Behavior

by
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Abstract

Research demonstrating religious concepts’ influence on behavior through priming leaves open questions of who is sensitive to this priming. Though religious participants respond more reliably, nonreligious participants in university samples have responded to primes (Randolph-Seng & Neilsen 2007, Shariff & Norenzayan 2007). Further questions include whether religious priming is a special case and how diverse are the effects of religious concepts. This study tests for effects of primes drawn from three religious narratives, replicating one and testing two new behavioral measures, and includes as predictors not only personal religious affiliation but also experience with religious caretakers. Results fail to replicate the increased fairness of Shariff & Norenzayan (2007), but do show effects of a Paradise-based priming condition consistent with their predictions (2008, Shariff & Rhemtulla 2011). This failure to replicate may be evidence of an interaction with context or of an “unpriming” effect like that described by Sparrow & Wegner (2006).

Keywords: Religion; priming; prosociality; benevolence; cultural transmission
Dedication

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Chapter 1. Introduction

In a world of radical uncertainty, many people claim access to absolute truth: that a transcendent source of meaning and value exists, and all behavior must be evaluated relative to that source. This reference point may be outside or preceding nature (what-is in the tangible realm of collectively accessible experience). A sociocultural system built around such a reference point is in one branch of that family of sociocultural systems we can call “religions”. In this thesis, the relevance of religious systems is their implications for social life. A transcendent source of meaning and value must have implications for how we understand human life, and not only the most hegemonically successful religion on this continent, Christianity, but also a number of populous and successful traditions, such as Buddhism, advocate universal benevolence. While this stress on disinterested, universal benevolence may not be equally emphasized in every such system or at every moment, it does appear typical of the world's most successful religions, many of which stress commonality and shared moral interest among all persons or even all beings. There is in recent psychological research a movement to explore and explain whether, when, and how relationships exist between religious systems and benevolent behavior. This research takes place in the light of centuries of religiously-motivated conflict, genuine self-sacrifice, and, today especially relevant in the United States' religious/political complex, the persistent notion that people are malevolent unless tempered by God's guidance.

While there is no strict defining line for the category of “religions”, given that there is in some cultural genealogies no distinction between religion and traditional knowledge, the European and colonial-European relationship between scientific and religious hermeneutics encourages the distinction even among people whose cultures of origin, or ancestors' culture of origin, might have included no distinct boundary between “religion” and traditional knowledge. In the former European colonies, then, it can be meaningful to speak generally of “religions” and to ask: are there really differences in how a person
treats other people, outside of ritual contexts, due to these sociocultural systems? Specifically, do we really see more “good Samaritans,” or people who give universally and without prejudice, among those who engage with these systems, versus those who reject them? And, if so, what parts of the system make a difference?

Attempts to demonstrate connections between religiosity and benevolence give mixed results. Although survey reports suggest a positive association between religious belief or religious community membership and charitable giving and volunteer work, this may or may not involve the universal benevolence we hope to see. Galen (2012), reviewing several decades’ survey and laboratory work, points out not only that many charities are themselves religious, meaning that within-group benevolence and universal benevolence are potentially confounded within survey reports of charitable giving, but also that laboratory work tends not to duplicate the relationship between religiosity and helping, whether “religiosity” is coded as community involvement or strength of belief (see also Norenzayan & Shariff, 2008). Galen also argues that religious respondents’ desire to appear consistent with an altruistic image may contribute to false positives, and, given an established link between socially desirable responding and measures of intrinsically motivated religious behavior (e.g. Burris & Navara, 2002), this concern appears valid. Given these reasons for skepticism and the lack of clear laboratory evidence connecting either religious belief or group membership with disinterested benevolence, some researchers have begun to explore more fine-grained and contextual vectors through which religion might be related with benevolence, and to determine whether and when people actually strive for universal benevolence, versus serving in-group cohesion alone. These methods include not only accounting for individual differences in belief but also acknowledging the potential power of religious symbols as semiotic context.

Two 2007 studies opened the door to this line of research: one, by Shariff and Norenzayan, using supraliminal priming to predict fairness in an economic decision game; another, by Randolph-Seng and Neilsen, using subliminal and supraliminal priming to predict honesty in a laboratory task. In each case, participants primed with religious words behaved more fairly, or more honestly, than their counterparts in a control condition. Although this effect was contingent on self-reported religiosity in a
community sample (Shariff & Norenzayan, 2007), religiosity in Shariff and Norenzayan's initial university sample did not predict behavior. Neither did a measure of intrinsic religiosity by Randolph-Seng and Neilsen, consistent with Galen's (2012), and Burris and Navara's (2002) suggestions that a bias toward socially desirable responding creates the apparent links between religiosity and moral behavior. From this, a 2008 paper by Shariff and Norenzayan argues for religions as semiotic systems that take advantage of basic psychological processes – either the “ideomotor effect,” an often-cited but little-explained backdrop for priming effects, or the effects of perceived observation – to increase the likelihood of prosocial behavior in general.

The former explanation refers to ideomotor theories of action, a body of theory with common foundations claiming a tight or unmediated coupling between perception and action, such that the mere thought of an action, or of the sensory experiences anticipated to follow an action, are sufficient to produce that action if no inhibition occurs (Shin, Proctor, & Capaldi, 2010). Priming researchers have not necessarily offered detailed ideomotor explanations of priming effects, but have described priming effects as evidence of a tendency to act in harmony with contextual information (Bargh, 2011). There has been little effort to explore religious priming effects in terms of ideomotor action, a line of exploration that would seem to require detailed knowledge of common associations between religion-related symbols, actions, and their sensory consequences. It might be argued that the supernatural observer hypothesis is also an ideomotor explanation albeit one with multiple steps, most importantly the hypothesized step in which a suggestion of observation triggers the thought, and therefore also the action, of adapting one’s behavior to account for the observer. Despite that possibility, prior papers on religious priming use the term “ideomotor explanation” specifically in reference to the narrower notion that primes evoke behavior consistent with the primed concept.

There has been more direct exploration of the latter hypothesis, the claim that reminders of God influence behavior by implying observation and therefore increasing self-monitoring, or by bringing to mind a person’s existing belief in an observing God. Gervais and Norenzayan reported increases in public self-awareness and socially desirable responding following exposure to God concepts (2012), and experiments have
demonstrated increases in contributions to common resources as a function of signs of observation such as eye spots (Bateson, Nettle, & Roberts, 2006), suggesting that belief in a real observer would not be strictly necessary for religious symbols to influence behavior. Believers' distrust of atheists may also be informed by expectations that people are more trustworthy when they think God is watching them, suggesting that the conscious experience of people who actively believe in God does not contradict this explanation, at least in typical members of the population sampled (Gervais, Shariff, & Norenzayan, 2011). Researchers including Shariff and Norenzayan have argued for the importance of believing in the ostensible observer as a credible source of punishment, and quasi-experimental results show also that belief in God as a wrathful and judgmental figure predicts less cheating in a laboratory task, while general belief in God, or in a loving God, does not (Shariff & Norenzayan, 2011). This argument draws further support from correlative findings on a global scale showing an association between national crime rates and national rates of beliefs in Heaven and Hell: that rates of belief in Hell negatively predict reported crime rates, while rates of belief in Heaven independently and positively predict reported crime rates (Shariff & Rhemtulla, 2012). These independent correlations hold across a number of distinct types of crime.

Overall, the current predominant argument is that religions as sociocultural systems have effects on behavior, and perhaps not character, through changing peoples' semiotic environments to include increased cues of being watched, with the result that people who believe in God as a credible source of punishment behave more prosocially (Shariff & Norenzayan, 2008). This stands in stark contrast to everyday accounts of religious motives and feelings, such as devotion to Christ's mission, emulation of Bodhisattvas and saints, or a genuine compassion motivated by a belief in the common sanctity of life (although the existence of one effect does not rule out another's co-existence). It would also be consistent with evidence from Batson and colleagues, whose lab offered one of the first “no good Samaritans” studies experimentally demonstrating a lack of association between religiosity and costly benevolence in 1973. Batson argues that their and similar research programs suggest morality is a matter of maintaining appearances, a facade people maintain before others or, sometimes, internally, as a form of egoism (Batson, 2011). Given this claim to the social nature of
morality, Batson's work would also predict that feeling observed, or observing oneself more intently, would increase one's motive to appear moral.

This argument includes a number of unclear points. If belief mediates this effect (Shariff & Norenzayan, 2008), then what explains the effect of religious priming on non-believers in the first study of Shariff and Norenzayan's 2007 paper, showing that undergraduate students (but not participants in a later community sample) were sensitive to priming independent of their belief? Randolph-Seng and Neilsen (2007) also demonstrate effects independent of belief and without conscious awareness of primes, and therefore without opportunities for the explicit thoughts of God Shariff and Norenzayan mention in their 2008 review. Reminders of observation may have an effect independent of observation itself, as with the cues in Bateson, Nettle, and Robert's study demonstrating increased contributions to a common resource (a psychology department's fund for coffee supplies) in the presence of an image of eyes, versus an image of flowers (2006). These effects could be due either to a form of unconscious, ideomotor effect, to increases in self-monitoring, or to both effects simultaneously. If religious primes have such general efficacy, though, then what explains their lack of effect in Shariff and Norenzayan's larger community sample (2007)? Perhaps an ideomotor effect could explain the consequences of priming in the absence of belief, but as mentioned earlier, ideomotor effects to date have merely been described: to this student's knowledge, although ideomotor theory is extensive and well developed and although work continues to describe the foundations of ideomotor actions (e.g., Knuf, Aschersleben, & Prinz, 2001), there has never been a satisfactory ideomotor explanation of the priming effects. There is currently little basis on which to make predictions about when and whether differences in sensitivity to ideomotor influence should exist.

There is, however, some basis for predicting when and how a person may be semiotically sensitive. If religious concepts and symbols are culturally and historically contingent instances of a general category, cues that induce people to act as if we are observed, then unless there is some intrinsic relationship between the symbol and the typical human body, there must be some process of sensitization. For strings of characters, the medium of this and prior priming procedures, it seems safe to assume that there is no natural relationship; a word's value as a prime should be contingent on
what behavioral information it has afforded the primed person as a signal in prior lived experience.

This learning process need not resemble education. In the parlance of ecological psychology, an environmental signal (anything from eyespots to a word) has meaning to the extent that it affords more successful behavior in some specific context, relative to some specific goal. When coherent co-variation exists between that signal and differences in opportunity for action, learning can occur, including procedural or implicit learning. The influence of religious signals on behavior should be contingent on what those signals have afforded for a person's behavior in the past. Whether words like “God” remind one of being watched and so increase self-monitoring or imply a specific social context in which one has prior experience, perhaps a context with relatively high levels of monitoring and punishment of deviation from prosocial norms, there must have been some context to establish these relations between signal and behavior. If private thought and conscious action are responsible for the relationship between reminders of God and benevolence or moral action, then personal religiosity should predict the effects of any priming condition. Given inconsistent evidence for that moderation, however, it may be that procedural learning – like from observations of or interactions with religious caretakers and members of a religious community – plays a larger role. If so, and if religion typically influences behavior not through genuine concern for others but through signals of social observation, then experience interacting with people in a position to model and enforce behavioral norms, such as caretakers who practice religion in a group context, could be a better predictor of the efficacy of priming conditions.

Another gap in the current framework that can be addressed experimentally is the question of differences among religious narratives. Although Shariff and Norenzayan showed quasi-experimental evidence for the importance of beliefs in God as a punitive, judgmental being for honest compliance in a laboratory task (2011), there is not yet experimental evidence exploring the roles of different religious narratives, such as afterlife narratives promising rewards and punishments. If religious concepts inform human action through the semiotic process suggested by Shariff and Norenzayan, including the role of the threat of punishment, and if the correlations between crime rates and different afterlife beliefs exist due to those afterlife narratives’ effects on behavior,
rather than the reverse causal order or some third variable, then priming including the symbols of punitive afterlife narratives ought to have effects similar to the generally religious primes used previously. Priming using the symbols of peaceful and paradisaical afterlife narratives ought to have either no effect or, if suggestions of forgiveness reduce self-monitoring or motives to act morally and if ideomotor processes evoking action consistent with primes do not play a major role, a negative effect on measures of fairness and honesty. Assuming that the panopticon effect of religious symbols is constant across narratives, increased prosocial behavior following reminders of paradise, or decreased following reminders of punishment, would conflict with the punitive watcher hypothesis.

The following experiments attempt four major sets of tests. First, these experiments draw on the procedure of Shariff and Norenzayan (2007) to compare the effects of their methods with a comparable participant pool, in a variation on the economic decision game used in that study. Participants in that study played one round of the Dictator Game, given $10 to split between themselves and another, anonymous participant. In the Dictator Game, the first player, the Proposer, simply informs the second, the Receiver, of their split. The Receiver has no agency in the game and may receive nothing at all. The Proposer’s lack of accountability makes this game useful for testing self-imposed standards of behavior.

Our procedure instead uses the Ultimatum Game. In this game, the Proposer makes their offer knowing that the second player, the Receiver, has the right to refuse their offer, in which case neither receives any money. Although classical economic theory would predict short-term self-interest as a ruling motive, such that Receivers accept any amount of money rather than none, this behavior is not typical in populations with socialized labour forces: Receivers typically reject offers below about $3, and Proposers seem to anticipate their behavior, as typical means for offers in highly industrialized societies are above $3, with $5 as a typical modal offer (Gintis, Bowles, Boyd, & Fehr, 2003). Proposers and Receivers seem to act with a prosocial norm in mind, and defer to or insist on that norm even at a cost to themselves. Given arguments that one form prosocial behavior can take is enforcing prosocial norms at a cost to oneself, and given that there is no direct, personal benefit to refusing Ultimatum Game offers, this study
varies the procedure of Shariff and Norenzayan to test whether primed participants will refuse unfair Ultimatum Game offers at a higher rate than participants in a control condition, enforcing the prosocial norm more consistently or expecting a higher standard. We also attempt to replicate their finding of increased fairness as a function of religious priming. Note that, in the Ultimatum Game, behavior indicates self-imposed standards less directly and instead may reflect participants’ expectations of others. If participants respond to religious primes in this context, it may indicate that religious cues influence social behavior through influencing our expectations of others. Prior use of the dictator game means that prior experiments have been more focused on effects internal to a given person, rather than exploring religious cues in more naturalistic conditions allowing social exchange.

Second, in addition to using the original priming materials of Shariff and Norenzayan’s experiment, this procedure introduces two new priming conditions with the same dependent measures: one based on afterlife-reward-related terms, the other based on afterlife-punishment-related terms. According to the punitive watcher hypothesis, these narratives should have divergent effects: while ideas of a punishing supernatural observer should increase prosocial behavior relative to neutral primes, the idea of a loving, forgiving God is predicted to have no positive effect on prosocial behavior and may instead decrease the motive to morally police one’s own behavior. The ideomotor explanation, however, does not predict that reminders of a forgiving supernatural order would decrease moral self-monitoring, but does predict that these primes could increase forgiveness of selfish behavior. In neither case do these hypotheses predict that reminders of supernatural rewards should increase costly prosocial norm maintenance, and in both cases they predict higher acceptance rates of unfair offers. The key test of the hypothesis that cues related to a forgiving supernatural order decrease moral self-monitoring and increase selfishness is therefore final offers in the Ultimatum Game: will they be more, or less fair on average than offers in the other conditions?

Third, we asked participants to report their and their caretakers’ religious group membership, if any, to test whether caretaker history or personal religiosity predicts the efficacy of priming or predicts behavior independent of condition. This allows for a test of
whether personal religiosity has no predictive power, consistent with prior studies, and of whether caretaker behavior either informs behavior directly or sensitizes participants to religious priming (e.g., through modeling social norms and through establishing an anticipatory social value for religious cues). Finally, the procedure tests whether priming effects extend to a yet-untested domain, cheating at the expense of one's peers, by introducing a lottery incentive in which participants had ample opportunity to skew the odds in their favor.
Chapter 2. Methods

2.1. Participants:

This series of experiments took place on the campus of Simon Fraser University during March and April 2013, drawing exclusively from undergraduate psychology students recruited through the department's Research Participation System to complete course requirements. The study was advertised as a “financial decision-making task”, and we informed potential participants that they could earn a small compensation of up to $5 beyond course credit. The study used procedures approved by the SFU Office of Research Ethics.

Of the 110 participants recruited for this study, we excluded 4 due to missing data and 2 due on the basis of their suspicion that offers in the Ultimatum Game had not been made by real fellow participants. The remaining 104 participants ranged in age from 17 to 25 years (mean of 19.6). We controlled recruitment so as to balance participation by sex. Thirty-six participants (34.6%) reported practicing an organized religion, and of those, 23 reported some form of Christianity (10 Catholic, 1 Lutheran, 12 unspecified), 7 reported Sikh, 4 reported Buddhism, and 2 reported Islam. Fifty-seven participants (54.8%) reported a caretaker's practicing a form of organized religion, 37 of them reporting Christianity (17 Catholic, 1 Serbian Orthodox, 1 Jehova’s Witness, 18 unspecified), 10 Buddhism, 7 Sikh, 3 Islam; one person reported both Sikh and Catholic caretakers. Nearly every participant reporting a personal religious practice reported a caretaker in the same tradition. Seventy-seven participants (74%) reported speaking English from childhood, which may reflect SFU’s 25% international undergraduate enrollment. The local population also includes many immigrant families from a variety of language backgrounds.
Each participant performed the computer-based tasks in a separate, sound-insulated room at their own machine, with a door between them and the person guiding them through the procedure; participants completed paper demographics forms separately, in a room apart from the computer suites, where the consent and debriefing processes also took place.

2.2. Measures and Procedure:

2.2.1. Lottery Cheating Task:

Our participants' first task after priming was to assign themselves a number of raffle tickets for a $100 raffle as a participation incentive. Using an online simulated dice roller while alone in a testing room, participants determined a number between 1 and 100 and reported their own results. We determined whether participants cheated, either by re-rolling or by fabricating results, by comparing that report to the activity log of the website.

2.2.2. Ultimatum Game:

The Ultimatum Game immediately followed the cheating task in the same computer suite. This and all other computer-based measures were implemented in ePrime. Participants were told that they had a chance to earn money based on their choices in the game. Participants played through twenty rounds as a Receiver: they see an offer from another (fictitious) player, who had been asked to split $10 between themselves and the current participant. The participant judges whether they would accept or reject the offer, knowing that if they reject the offer, neither player would receive any money for the round. The twenty offers ranged between $.25 and $5 in all intervening 25-cent increments and appeared in an order randomized for each participant.

After these twenty rounds, each participant had the chance to take the Proposer's role. Participants were asked to report how they would offer to split ten dollars with an anonymous future participant, knowing that they could not receive money for the round
unless that player accepted. Like Shariff and Norenzayan, but unlike traditional Ultimatum Game procedures, we allowed participants to offer $0.

**Levenson Psychopathy Score:**

Although results are not analyzed in this study, participants next completed the Levenson Psychopathy Test (Levenson, Kiehl, & Fitzpatrick, 1995) as a computer task.

**Self-Report & Demographics:**

Participants answered a series of self-report questions regarding general political orientation, religious belief, and their expectations of members of their religious community of origin as compared to people on average or, if they were not raised as members of a practicing religious community, their expectations of members of the most common religious group in the community where they were raised. Answers to these questions were in a seven-point, Likert style format. Finally, we asked participants to leave the computer testing room, allowing experimenters to check their results and determine their payments, to complete a pencil and paper demographics form including age, sex, education, self-identified ethnicity, religious affiliation (yes/no and, if yes, which group), childhood caretakers' religious affiliation (yes/no, and, if yes, which group), and family socio-economic status, personal SES relative to the SFU community, and SES relative to the population outside SFU, rated on the MacArthur Scale of Subjective SES (Adler, Epel, Castellazzo, & Ickovics, 2000; Cundiff, Smith, Uchino, & Berg, 2013).

**Design and Procedure:**

Upon arrival, following a brief welcome and a request for consent describing their role in a “financial decision-making game” in a social context, each participant moved to a single-occupancy computer room and received one of four scrambled sentence priming tasks. An experimenter gave out the scrambled sentence task on a single page of paper, asking participants to complete it alone in the computer testing room and to signal them when they had completed it.

We sorted participants into one of four conditions based on arrival order, repeating the pattern: 1, 2, 3, 4, 4, 3, 2, 1. Each condition varied only in exposure to
decontextualized words related to supernatural concepts: a Control and a generic “Religious” condition from Shariff & Norenzayan (2007), a “Punishment” condition, and a “Paradise” condition. Our Control and Religious conditions are adopted directly from Shariff & Norenzayan, 2007. These authors describe the task as comprising ten five-word items, each of which can be unscrambled to one or more four-word sentences (e.g., “dessert divine was fork the”). In the experimental condition, half of these five-word scrambles include one word associated with a range of religions: “spirit,” “divine,” “God,” “prophet,” and “sacred”. The scrambled sentence task was not originally designed for priming but may be a useful tool for exploring effects of supraliminal priming by giving participants an occasion with which to interact with the words before a task. See Appendix P for Priming Materials.

Shariff & Norenzayan identified “spirit,” “divine,” “God,” “prophet,” and “sacred” as their manipulation items, but the Religious and Control primes differed in additional ways. The five sentences not containing religion-related words varied between conditions, and in some cases the variation may be relevant to the supernatural watcher hypothesis. In the Religious (but not the Control) condition, the task included a scramble not included in the control condition and containing the word “presence,” a word related to the observer hypothesis but not restricted to religious meanings. The religious-condition scramble containing “God” also contains “evil,” a word related to character in a way relevant to the punishment aspect of the supernatural watcher hypothesis. The control primes also included “holiday“, which is etymologically related to religious events, though not in everyday use. While noting these features, we presented the Control and Religious priming materials exactly as presented in Shariff & Norenzayan, 2007.

The novel priming materials replace the five prime-containing scrambles from Shariff & Norenzayan’s experimental condition with scrambles associated with narratives of either afterlife rewards or afterlife punishments, but not restricted to a specific religious tradition: “celestial,” “halo,” “paradise,” “bless,” and “heaven”; “condemn,” “hell,” “inferno,” “abyss,” and “damn”. Although we found it difficult to escape the terms of Christian mythology, terms such as “heaven” and “hell” have been popularly used to communicate narratives of any given punitive or rewarding afterlife to English-speaking audiences, so we hope for equivalence and acknowledge the limited, exploratory nature of these
conditions' materials. The non-priming words in each scramble do not necessarily appear in all conditions: scrambles vary to accommodate the condition-specific words (e.g., the words of the scramble in which “celestial” can be used to create a valid sentence, “are celestial simple suns bodies” (“Suns are celestial bodies”), do not appear in other conditions).

The two behavioral tasks immediately followed priming. Because the cheating task is brief and presented to participants as an incentive rather than as an experimental task, we performed the two tasks back to back.

After behavioral tasks, participants completed the Levenson test and a set of demographics questions on the computer. In order to check their results and determine any payment, we asked participants to leave the computer room to step into another room, where they completed the final demographics questions and a simple suspicion probe to explore whether participants believed themselves to be interacting with real peoples' offers in the Ultimatum Game. Rather than pay every participant for every offer they accepted, we had participants naively select a number between 1 and 21, checked their choice in that round, and paid them what they would have received for that round. If the participant had accepted an offer during that trial, or if their turn as Proposer was selected, they received money equal to either the offer they accepted or, for the final trial, the amount they had offered.

2.3. Coding and Analysis

2.3.1. Coding

Cheating Task

We defined cheating in the lottery task as fabricating a number or “rolling” multiple times and detected cheating by cross-referencing the dice roller website's activity log with our participation records.
**Ultimatum Game**

We coded participants' Ultimatum Game behavior as Receivers in terms of the total number of offers they accepted above and below the $2.50 mark. It was clear from the raw data that participants did not necessarily have a consistent threshold point below which they rejected all offers: in some cases, people accepted offers lower than others they rejected. A look at the raw data also suggested that there was little variation above, but much below, the median point. To capture acceptance of unfair offers specifically, we split results into the number of offers accepted out of the ten above and ten below the midpoint ($2.75 and above, $2.5 and below), a choice also informed by prior research suggesting that Receivers expect an even split but tend to accept lower offers to a point, especially in the $3 to $5 range (Gintis, Boyd, Bowles, & Fehr, 2003). Although $3 might thus seem a more natural cutoff point, the range of offers in this procedure is more unfair than a naturalistic range would be, justifying a glance downward. In case variation occurred above the median, analysis includes rejections of above median offers as well. Final offers are recorded in dollar amounts.

### 2.3.2. Planned analysis

To test whether either acceptances or final offers varied as a function of priming condition, the first and principle comparisons were three Tukey's HSD tests, used without a preceding ANOVA to directly test all relevant hypotheses while reducing the total number of tests, thus keeping the chance of type I error lower across multiple comparisons while holding α at 0.05 for each test. The comparison of rejected below-median offers served to test the hypothesis that priming would increase acceptances of offered money relative to Control, except in the Paradise condition. The final offer comparison served to test whether priming would still lead to increases in fair treatment of anonymous partners under these conditions, and whether the Paradise condition would be an exception. The test of rejected high offers did not directly relate with the study's hypotheses but, given that some participants had demonstrated inconsistency in their threshold for rejections, seemed necessary in order to rule out unexpected conditional differences in acceptance rates of above-median offers.
To compare the relevance of participant traits and personal history, I tested also for the effects of two variables: personal religiosity and childhood exposure to religious activity, measured by proxy in two questions: whether the participant, and whether any of the participant's childhood caretakers, had been a member of any organized religion, asked as a yes/no binary. Although we collected reports of the specific religion in question, variation was too great to include religious denomination as a factor. Previous research suggests that priming effects in students in this area of the world may not require personal belief, an oddity if one's explanation for priming's effects includes self-aware thoughts of God or other believed-real observers. Caretaker history has not previously been studied. If contextual information like the words representing religious concepts influences behavior through associatively cued changes in anticipation, then exposure to conditions that afford building those anticipations ought to be a necessary precursor to successful priming effects. In that case, caretaker history should be a moderating variable between priming and behavior. Although the self-report used here is a rough, exploratory measure, the presence of an interaction effect should not occur unless caretaker behavior is a vector for learning to respond to religious cues, and a main effect should not occur unless early life modeling of religious groups' social norms has a consistent effect on participants' prosocial tendencies.

Both these terms and their interactions with condition were dummy-coded and included in regression analyses. To test for the possibility that either the personal or the upbringing factor moderated effects of condition on participants' behavior, I first tested for correlations between each factor and both final offers and acceptance rates of below-median offers, then, for each predictor variable, dummy-coded the categories and separately tested for the predictive value of personal and of caretaker religiosity for main and moderating effects using a stepwise regression model. Because of the high correlation between personal and caretaker membership in organized religion, these two regression models are separate. In each case, $\alpha$ for probability tests is 0.05.
Chapter 3. Results

3.1. Cheating

Against expectations, not a single participant cheated in the lottery task. See the Discussion section for some speculations on factors that may have steered participants away from opportunistic behavior.

3.2. Ultimatum Game

3.2.1. Planned Comparisons:

Conditional differences in participants' mean acceptance rates of below-median offers (Control: $M = 1.67, \sigma = 2.69$; Religious: $M = 2.75, \sigma = 3.77$; Punishment: $M = 1.92, \sigma = 2.9$; Paradise: $M = 2.21, \sigma = 2.75$), and mean acceptance rates of above-median offers (Control: $M = 6.81, \sigma = 2.84$; Religious: $M = 7, \sigma = 2.62$; Punishment: $M = 5.96, \sigma = 2.82$; Paradise: $M = 7.17, \sigma = 2.48$) were likely due to chance, according to two Tukey's HSD tests (below-median offers: one homogenous subset, $p = 0.586$; above-median offers: one homogenous subset, $p = 0.378$). See Tables A1, A2, and A3 for descriptive statistics.

Among participants' final U.G. offers, the overall difference in fairness seen in previous comparisons of the Religious to Control priming conditions did not occur: in fact, mean offers were highest in the Control condition. While six participants in the Religious priming condition offered less than $1, five of those offering $0, only one Control participant offered less than $3, and Control included the most hyperfair offers ($>5$). Although the Paradise condition included many offers below $1, the Religious condition included the most $0 offers. A Tukey's HSD test revealed a significant difference between the mean final offers in the Control ($M = 4.73, \sigma = 1.59$) and the
Paradise (M = 3.3, σ = 1.78) conditions, with means in Punishment (M = 3.63, σ = 1.9) and Religious (M = 3.72, σ = 2.08) conditions falling indeterminately between them. See Table A4 for details of the comparison tests.

### 3.2.2. Regression Models:

The purpose of the regression analysis was to test personal and caretaker religious group membership for predictive value and to test for conditional interaction effects.

Modeling conditional effects on acceptances of below-median offers showed little effect of condition on acceptance rates, as established by prior comparisons (R = 0.03, \(R^2 = -0.009\); F = 0.089, p = 0.766). Caretakers' religiosity added to the model's predictive value, and that change was statistically significant (R = 0.233, \(R^2 = 0.054\); F = 5.725, p = 0.019); adding personal religiosity to the condition-only regression model also increased predictive power, but not so consistently that chance variation could be ruled out (R = 0.192, \(R^2 = 0.037\); F change = 3.755, p = 0.055). In neither case did an interaction term substantially improve the model (Caretaker: \(R^2 \text{ change} > 0.001\), F change 0.028, p change = 0.868; Personal: \(R^2 \text{ change} > 0.001\), F change 0.034, p change = 0.855). Although priming appeared to have little consistent effect on participants' judgment of proposed offers, caretaker history was a significant predictor of behavior in this section of the Ultimatum Game task, but not through increasing sensitivity to primes.

Modeling the same variables' effects on final offers revealed a different pattern, in which condition was the strongest predictor (R = 0.259, \(R^2 = 0.067\); F = 7.334, p = 0.008). While adding caretaker religiosity to the model marginally increased predictive power, the change was not statistically significant (R = 0.289, \(R^2 = 0.084\); F change = 1.835, p change = 0.179), and adding personal religiosity to the condition-only model brought negligible change (R = 0.260, \(R^2 = 0.067\); F change = 0.045, p change = 0.833). Neither interaction term contributed significantly to the model (Caretaker: \(R^2 \text{ change} = 0.013\), F change = 1.359, p change = 0.246; Personal: \(R^2 \text{ change} = 0.01\), F change = 1.074, p change = 0.303). Despite the lack of consistent priming effects on behavior during the first twenty rounds of the Ultimatum Game, priming condition predicted
participants' final offers independent of participant's personal religious group membership and exposure to caretakers' religious practice.
Chapter 4. Discussion

Overall, with the exception of the Paradise cues’ effect on Ultimatum Game offers, the predictive value of caretaker religious group membership, and the lack of personal religiosity’s predictive value, this study’s results are inconsistent with prior predictions. Not only was there no conditional effect on acceptance rates of unfair offers, but also this procedure failed to replicate the fundamental prior finding that religious priming increased fairness in the Ultimatum Game. The lottery-based cheating task produced no conditional differences whatsoever, as no participants cheated. It may be that our sample was not broad enough to include people both capable of spotting the opportunity and motivated to cheat. There may instead be procedural explanations for the lack of cheating, and for the lack of the religious primes’ effects.

In the lottery task, no participants reported an inflated number of lottery tickets, despite being unobserved while determining their number of tickets for the $100 incentive. It may be that aspects of our procedure discouraged cheating. Given that this task took place after the procedure had begun, it may simply have been obvious to participants that their behavior was under scrutiny as part of the study, even if the means of observation was not obvious. Participants completed the lottery task alone, but after completing a priming task and receiving instructions from a person dressed in a white lab coat. Our stating that we would be unable to retrieve illegible responses via the website may have occasioned participants to consider how easily those results would be to check if that statement were untrue. The complete uniformity of participants’ behavior, however, seems to call for a simple, sweeping explanation. Perhaps it is simply uncalled-for to expect any person to cheat their neighbors.

Why the religious primes from Shariff and Norenzayan (2007) had no clear effect is a more pressing question. Because that study took place at the nearby University of British Columbia, student demographics should be roughly comparable, meaning that
there is no reason to expect participant variables to account for the difference in the absence of direct evidence otherwise. That study did draw a larger sample pool, but while this sample’s size might obscure effects, it ought not to reverse the direction of a conditional difference. Fewer than 50% of this study’s participants reported a religious affiliation, but in prior university samples, religiosity did not predict conditional effects (Randolph-Seng & Neilsen, 2007; Shariff & Norenzayan, 2007), and regression analysis suggests that religiosity in this sample did not significantly predict behavior, ruling out this factor. This leaves two major differences between the present study and the procedure from which these primes are drawn: the intervening lottery task, and the twenty rounds played as Receiver.

In the present study, the lottery task immediately intervenes between priming and ultimatum game procedures, and so it is conceivable that the task may have had some effect on subsequent behavior. Sparrow and Wegner (2006) performed a series of experiments testing whether primes’ effects on behavior could be extinguished by intervening tasks. Their priming differed from this in both form and content. In Sparrow and Wegner’s experiments, the effect under study was a pattern in which participants asked to respond to simple binary-response questions as if at random instead tend to respond correctly at above-chance levels (mean rates of correct answers being reliably close to 68%). They described this pattern as a consequence of priming, assuming that the experience of seeing a simple question and recognizing the answer is a natural phenomenon parallel to the constructed phenomenon of priming via external cues. Finding that they could reliably extinguish the effect, reducing correct answering to chance levels, by giving participants a chance to answer the question correctly before responding “at random”, they described their results as “unpriming”.

In the present procedure, participants’ refraining from cheating when they obviously have the chance to do so may serve a similar function. If priming triggers an anticipation or motive that can be discharged, as in Sparrow and Wegner’s experiments, by a behavior that satisfies whatever impulse the prime has triggered, then the impulse triggered by primes motivating moral self-monitoring might be satisfied by a simple action like observing oneself refraining from cheating. If it were experimentally demonstrated that similar intervening tasks disrupt the effects of religious primes in
populations where these primes ordinarily produce predictable effects, then it would be
fair to argue that, whatever behavioral changes religious priming facilitates, their effects
may last no longer than one’s first opportunity to impress oneself with one’s goodness.

The unpriming explanation may account for a lack of difference between
Religious and Control conditions, but an explanation of this study’s priming effects would
not be complete without accounting for final offers in the Paradise condition’s being
significantly lower than those in Control. Predictions based on the punitive supernatural
watcher included that the Paradise condition should, if it had any effect, decrease
participants’ vigilance in monitoring their own behavior to maintain prosociality. Because
this predicted effect would not be satisfied by refraining from cheating on the lottery task,
there is no reason to expect an unpriming effect in this condition. The presence of a
negative effect in this condition is therefore in line with the unpriming hypothesis.

This procedure also differs from past work in that participants played through
twenty rounds of the Ultimatum Game in which they viewed every possible offer in 25-
cent increments between $.25 and $5, randomly ordered, framed as offers from previous
participants. This range results in a distribution of offers implying a participant population
in which a sweeping majority make uneven offers and most offer less than the $3
threshold observed to be typical of samples from highly socialized monetary economies.
Some participants interpreted these offers from a benevolent and forgiving perspective,
allowing that they could not know whether these fellow participants were in need of the
money, as one Sikh woman described her thinking in her response to the suspicion
probe. Others, however, may have been insulted by the lack of consideration. It is
possible that the primes influenced participants’ responses to the environment of offers
so as to reverse or interfere with the expected effects of the Religion and Punishment
cues. Although conditional differences in acceptance rates appear due to chance,
participants’ experiences of these unfair offers may have differed in systematic ways that
affected their expectations of how their offers would be received. Carryover from
frustrations or fear of rejection based on one’s own standards for acceptance might have
informed these choices. In the case of Paradise primes, if these primes did successfully
cue notions of a forgiving and merciful supernatural order, this backdrop would account
for the participants’ anticipating success with less balanced offers. If future research in
religious priming occurs, one worthwhile line of testing would be to differentiate possible effects of context from possible unpriming effects. It may even be the case that null results from the Ultimatum Game do not reflect unpriming or inadequate sampling but the qualitative difference between this procedure and the Dictator Game, a possibility that could be tested by a larger study varying the presence or absence of the lottery task to test the unpriming hypothesis.

Although priming condition had no tangible effect on participants’ behavior as Receivers, caretaker religiosity did predict behavior, positively predicting acceptance rates of below-median offers. Acceptances of below-median offers are an ambiguous measure. Although accepting an unfair split is, in classical economic terms, self-interested behavior in that one receives some money instead of none, the same behavioral observations could be motivated by overcoming one’s reaction to the unfair offer by thinking of the other player’s needs. The relationship between caretaker experience and motivation is thus unclear.

That this measure produced results despite its gross simplicity deserves note. The measure of caretaker religiosity is a dichotomous response to the question of whether one or more of the participants’ parents or other caretakers practiced as a member of an organized religion. This measure fails to capture details such as how and to what extent the caretaker was involved, for what time range, the nature of their practice and belief, whether the participant joined them, how much time of what types they spent together, or other details of the caretaker-child relationship and each of their relationships with the larger social group. There is also no measure of the internal or extended family’s involvement, if any, with those caretakers’ practices. Given all that it does not capture, the measure affords a low potential for false positives but a high potential for false negatives. This positive result suggests that further, and more precise, investigation should yield useful information for the question of how caretaker history informs a young adult’s behavior.

One last note of caution: the peculiar behavior of some players must cast doubt on whatever results issue from their actions. In all conditions save Control, one or more players offered zero dollars in their round as Proposer. Perhaps they are especially bold,
but in the Ultimatum Game, unlike the Dictator Game, the Receiver is in a position to prevent such a player from receiving their prize with no loss to themselves. Why a Proposer would take such a risk is baffling, unless they either misunderstood the rules or did not take the game seriously. Either case would make their behavior unfit for comparison to the behavior of participants playing in good faith. For this reason, and to gain a more qualitative understanding of participants’ mindset and emotional condition during the Ultimatum Game, it would be wise for future experiments to use a more thorough suspicion probe than the single question used in this study. A traditional funnel-style suspicion probe would be more effective at learning whether participants believed that the premises of the games were true, and in the process, filler questions could obscure the nature of the probe while gathering valuable information about motive, mindset, and mood.

This study provides the first experimental evidence in support of the claim that the concept of a forgiving, embracing supernatural order does, in fact, promote self-interested action rather than promoting benevolence. These results must be taken with caution: both this and the prior quasi-experimental study of character attributions to God as predictors of honesty took place in the greater Vancouver area, and while this city is home to a wide range of cultural and economic backgrounds, there is no reason to expect that these samples are fully representative of all religious communities. Trend-based evidence from small samples can also obscure heterogeneity of motive and behavior. While exceptionally benevolent or “saintly” people may merely be more sensitive to self-monitoring cues or the perceived inevitability of supernatural judgment, there may be qualitative differences in individual relationships with benevolence, as there are qualitative differences in relationships with religion and spirituality in general (e.g., see Francis, 2007). There is growing evidence, however, that, as has been argued of moral behavior in general (Batson, 2011), religiously motivated prosociality is less a matter of conviction than of circumstance.
References


Appendix A.

Extended Results

Table A1: Below-Median Offers Accepted (out of 10)

<table>
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<tr>
<th>Condition</th>
<th>N Participants</th>
<th>Mean</th>
<th>S.Dev.</th>
<th>Confidence Interval (95%)</th>
</tr>
</thead>
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<td>Paradise</td>
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<td>2.21</td>
<td>2.75</td>
<td>{0.60, 2.73}</td>
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<td>Punishment</td>
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<td>1.92</td>
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<td>{1.29, 4.21}</td>
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<td>2.75</td>
<td>3.77</td>
<td>{0.72, 3.12}</td>
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<tr>
<td>Control</td>
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<td>1.67</td>
<td>2.69</td>
<td>{1.05, 3.37}</td>
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Table A2: Above-Median Offers Rejected (out of 10)

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<th>Mean</th>
<th>S.Dev.</th>
<th>Confidence Interval (95%)</th>
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Table A3: Final Offers

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<td>(J) Condition</td>
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<td>Std. Error</td>
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* The mean difference is significant at the .05 level.
Appendix B.

Materials

This appendix contains all materials essential to the present study, in the order in which they occur in the study’s procedure.

Consent Form

CONSENT FORM (Participant’s Copy)

You are being invited to participate in a psychological research study. This study is conducted by Samuele Zilioli and Foster Ranney, from BEL (Behavioral Neuroendocrinology Lab).

There are no known risks if you decide to participate in this research study. There are no costs to you for participating in the study. The information you provide will help us to clarify the role of contextual factors in social behavior. The information collected may not benefit you directly, but the information learned in this study should provide more general benefits.

In this study, you will be asked to answer questions about yourself, your background, your beliefs, and your experience in this study. First, you will be asked to perform short verbal tasks and a ten-minute decision-making task in which you can earn a small amount of money. In exchange for your participation in this study, we will also award you tickets for a lottery in which there is a chance of winning up to $100.

After the experiment, all data will be kept in digital format in a locked filing cabinet in our research laboratory (7401 RCB) at SFU Burnaby. Only the study researchers named in this document will have access to the cabinet. Each participant will be identified with an ID number, the code to which will also be stored in a locked cabinet. We are going to retain the data for a period of 7 years at which time it will be destroyed.

Your participation in this study is voluntary. If you choose to participate, please sign below and follow carefully the research assistances instructions during the experiment.
When the tasks are done if you have any questions about the study, please contact Samuele Zilioli or Foster Ranney from the Behavioral Neuroendocrinology Laboratory of Department of Psychology at Simon Fraser University (mailing address: 8888 University Drive, Burnaby, BC V5A 1S6; phone number: 778-782-7656 or 778-918-4992; email: [email protected] or [email protected]).

If you have any questions or concerns about being in this study, you may contact Dr. Hal Weinberg, Director, Office of Research Ethics at [email protected] or 778-782-6593.

The participant shall fill in this area. Please print legibly.

Participant Family Name: _______________ Participant Personal Name: _______________
Participant’s Signature: __________________________________________________________
Participant’s Identification Number (to be filled by the research assistant): _______
Date (Use MM/DD/YY format): __________________________________________________

Scrambled Sentence Task:

Full Text of Control Materials

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:
When you have completed all ten sentences, please bring this paper to the research assistant.
You will then have a short break before the next stage of the experiment.

Experimental Scrambled Sentences:

“Paradise” Sentences:
1. was worried heaven she always:
2. shoes bless replace old the:
3. retrace good have vacation a:
4. more paper it once do:
5. send I over it mailed:
6. saw hammer he the paradise:
7. yesterday it finished track he:
8. halo was bright refer the:
9. are celestial simple suns bodies:
10. prepared somewhat I was retired:

“Punishment” Sentences:
1. was worried condemn she always
2. shoes hell replace old the:
3. retrace good have vacation a:
4. more paper it once do:
5. send I over it mailed:
6. saw he the inferno hammer:
7. yesterday it finished track he:
8. abyss was dark refer the:
9. gives who simple a damn:
10. prepared somewhat I was retired:

“Religious” Sentences:

1. felt she eradicate spirit the:
2. dessert divine was fork the:
3. appreciated presence was imagine her:
4. more paper it once do:
5. send I over it mailed:
6. evil thanks give God to:
7. yesterday it finished track he:
8. sacred was book refer the:
9. reveal the future simple prophets:
10. prepared somewhat I was retired:

Lottery Task Text:

We want to thank you for volunteering your time for our study! Each participant is entered into a lottery drawing for a cash prize. Each participant gets several tickets, and the more tickets you have in the lottery, the greater the chance that you’ll win our prize.

To determine how many tickets you get, please use the computer and go to diceroller.org. There is a random number generator on the front page of the website. Please leave the upper range at “100” and leave the lower range at “1”. Then click on “Generate” and write down your result. We can’t recover your result from the website, so please be sure to write it down clearly.

When you are done, write the number here:

Fold this piece of paper and bring it to the research assistant. Then it will be time to begin the next phase of the study.
Ultimatum Game Instruction Screens:

**Text of Instruction Screen 1:**

In this game, you will interact with several anonymous players. Each player is a previous participant in the study, and we've randomly assigned a few of them to play with you.

You will play with one person at a time, and you will interact with that person only once. Each other player has been given an initial $10. This money is to be split between you and the other player, but they have control over how much of the $10 they choose to offer you.

You get to choose whether to accept or reject each player's offer. Their offers have been recorded in advance.

Press SPACE to continue

**Text of Instruction Screen 2:**

Your job is to either accept or reject their offer. If you accept an offer, then both you and your partner will be rewarded with your respective share of the money. If you reject an offer, neither of you will get any of the money.

Your performance on this task will partially determine your final monetary award!

Take your time to reply to your partner, don't rush.

Do you have any questions? If so ask the research assistant before pressing SPACE

Press SPACE to start

**Text of Game Slide 1:**

Connecting to Next player…

**Text of Game Slide 2:**

[name] offers you:

$[offer]

Accept or Reject?

(F) or (J)

**Text of Game Slide 3:**

You [choice] [name]'s offer.

You get: $[amount]

[name] gets: $[amount2]
**Text of Instruction Slide 3:**
Well done. Now you get to decide how much to offer.
However, this time it will be a one shot game (you will get to offer money only once).
Your partner will be someone who has not yet played the game with you.
Press SPACE to continue.

**Text of Instruction Slide 4:**
So now you have $10!
The same rules apply as before: if your partner accepts the offer that you make then you get the remainder of the $10; if they reject the offer, then both of you receive nothing.
Your performance on this task will partially determine your final monetary award!
Take your time, remember you have only one shot.
Press SPACE to make your offer.

**Text of Game Slide 4:**
*Please type in your offer (without the “$” before). Use the point to separate the unit from the decimals. When you are done press ENTER (note the value won’t appear but will be registered anyway)*

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**Demographics Instructions and Questions:**

**Text of ePrime Questionnaire**
Please answer the following questions to the best of your ability. Your responses are strictly confidential and will be used for research purposes only. Your responses will be identified only by a confidential participation number.
1. How committed to your religion do you consider yourself?

1 (not at all religious) 2 3 4 5 6 7 (very religious)

Think about the members of your religious community or, if you did not grow up within an organized religion, think of the members of whatever religious community was most typical in the community where you grew up. Compared to an average person, how do you expect a typical member of that religious community to act?

2. How honest do you expect a member of that community to be, compared to an average person?

1 (very dishonest) 2 3 4 5 6 7 (very honest)

3. How much self-control do you expect a member of that community to have, compared to an average person?

1 (very low self-control) 2 3 4 5 6 7 (very high self-control)

4. Speaking generally, what is your political orientation?

1 (conservative) 2 3 4 5 6 7 (liberal)

5. It is likely that God, or some other type of spiritual non-human entity, controls the events in the world?

1 (strongly disagree) 2 3 4 5 6 7 (strongly agree)

6. Do you believe in Heaven?

1 (strongly disbelieve) 2 3 4 5 6 7 (strongly believe)

7. Do you believe in Hell?

1 (strongly disbelieve) 2 3 4 5 6 7 (strongly believe)

**Text of Paper Demographic Questionnaire**

Please answer the following questions to the best of your ability. Your responses are strictly confidential and will be used for research purposes only. Your responses will be identified only by a confidential participation number.

1. Age: ________

2. What is the highest level of education you have completed? (Please check one)

_______ High school graduate

_______ At least one year of college, university, or specialized training

_______ College or university graduate

3. Did you grow up speaking English? ___ No ___ Yes

4. What is your ethnicity/ies? ________________________________
5. Do you practice any organized religion? Check “Yes” or “No”. If you do practice an organized religion, then please name your religion.

___ No ___ Yes, and that religion is: _____________________________________________

6. Did one of your caretakers during childhood practice any organized religion?

Check “Yes” or “No”. If they did practice an organized religion, then please name that religion.

___ No ___ Yes, and that religion is: _____________________________________________

**MacArthur Subjective Socioeconomic Status Scale**

The MacArthur SES Scale, inserted as a separate image file, is presented with the original text exactly as seen by participants. The suspicion probe follows.
7a. Imagine that this ladder pictures how Canada society is set up.

- At the top of the ladder are the people who are the best off—they have the most money, the highest amount of schooling, and the jobs that bring the most respect.
- At the bottom are people who are the worst off—they have the least money, little of no education, no job or jobs that no one wants or respect.

Now think about your family. Please tell us where you think your family would be on this ladder. Fill in the circle that best represents where your family would be on this ladder.

7b. Now assume that the ladder is a way of picturing your group of SFU peers.

- At the top of the ladder are the people among your SFU peers with the most respect, the highest grades and the highest standing.
- At the bottom of the ladder are people among your SFU peers who no one respects, no one wants to hang around with, and have the worst grades.

Where would you place yourself on this ladder? Fill in the circle that best represent where you would be on this ladder.
7c. Now assume that the ladder is a way of picturing your group of peers outside SFU.

- At the top of the ladder are the people among your peers with the most respect, the most money, and the highest standing.
- At the bottom are people among your peers who no one respects, no one wants to hang around with, and have no money.

Where would you place yourself on this ladder? **Fill in the circle hat best represent where you would be on this ladder.**

8. Please answer the following questions to the best of your ability. It is fundamental that your responses truly reflect your feelings and thoughts. Please write 2-3 sentences where you give us a feedback about your experience in the financial decision-making task.