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# Can Religious Priming Induce Truthful Preference Revelation?

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## **Abstract**

We examine whether religious priming can induce more truthful preference revelation in valuation research. Using induced value second price Vickrey auctions in both hypothetical and non-hypothetical contexts, our results suggest that religious priming can indeed induce more truthful bidding and eliminate hypothetical bias in hypothetical contexts. In non-hypothetical contexts where there are real economic incentives, religious priming induces similar truthful bidding as the absence of religious priming, implying that the use of real economic incentives is sufficient in producing truthful valuations. Our findings have significant implications for the use of religious priming in stated preference or contingent valuation studies.

**Key words:** willingness-to-pay (WTP), Vickrey auction, hypothetical bias, religious priming

**JEL codes:** C9, C91, D12

## **I. Introduction**

Eliciting people's preferences and valuations for various goods has been central in the economics literature. However, the gap between real and hypothetical valuations remains a big challenge for applied practitioners and is the predominant concern in stated preferences methods. Evidence from the field and the lab show that people tend to misrepresent their

preferences (i.e., usually overstate their values) when there are no real economic commitments. For example, several experimental studies showed that hypothetical referenda were likely to generate biases in the estimation of willingness to pay (WTP) values (Cummings, et al., 1997). A meta-analysis of 29 experimental studies by List and Gallet (2001) revealed that subjects on average overstate their preferences by a factor of 3 in hypothetical settings. Little and Berrens (2004) reconfirmed these results of List and Gallet using an expanded sample of studies. Given the generally robust findings on hypothetical bias in stated preference studies, the validity of WTP results, particularly from contingent valuation (CV), has been questioned and has been an important area of research in the economics literature.

Evidence of hypothetical bias are widespread in the literature. For example, Burton et al. (2007) and Mozumder and Berrens (2007) found evidence of hypothetical bias in induced value experiments. Neill et al. (1994) concludes that hypothetical values have little predictive power since the ratio of hypothetical to actual bids was 9:1 in their study. Loomis et al. (1996) have shown that hypothetical willingness to pay is significantly greater than actual willingness to pay, with differences hovering between 9:1 and 1.8:1. There are, however, exceptions. For example, Vossler and McKee's (2006) findings support the notion that hypothetical bias might not arise in induced values contexts and that decisions do not vary systematically when payment is hypothetical or real. More recently, Jacquemet et al. (2011) using French subjects found no evidence of hypothetical bias in induced value hypothetical contexts as compared to a context with real monetary incentives.

Our aim in this study is to test whether exposure to religious concepts could activate honesty among subjects and in turn mitigate hypothetical bias in valuation or WTP studies. Our basic premise is that motivation to respond honestly or truthfully can be primed through

exposure to words related to religiousness. Psychologists call the technique that implicitly stimulates certain behaviors as “priming”. Priming refers to an increased sensitivity to certain stimuli due to prior experience and activation of particular representations or associations in memory. Psychologists have found that stereotyping behavior can be stimulated by priming a social category. For example, Bargh et al. (1996) primed participants with elderly stereotype and then observed that these subjects walked more slowly down the hall as compared to a control group. Bargh et al. (2001) also showed that participants who have been incidentally exposed to certain words that activated the goal to perform well actually performed better on subsequent tasks. Several studies in the psychology literature have primed subjects in experiments with various concepts including soccer hooliganism and stupidity (Dijksterhuis & van Knippenberg, 1998), religiosity (Johnson, et al., 2010), honesty (Rasinski, et al., 2005), affiliation (Over & Carpenter, 2009), and conformity (Epley & Gilovich, 1999).

In this paper we examine whether religious priming could reduce hypothetical bias in an induced value second-price auction. We hypothesize that subjects primed into religiosity will activate norms toward honesty and thus reveal their preferences sincerely. Religious priming has been shown to influence self-evaluation concerns (Balwin, 1990) as well as honesty (Randolf-Seng & Nielsen, 2007) as evidenced by less cheating in a subsequent task. Priming can be either supraliminal (conscious) or subliminal (nonconscious). In our study, we used a subliminal type of priming technique where participants were given a “scrambled sentence test”. Scrambled Sentence Test priming techniques have been used by researchers (Srull & Wyer, 1979) in the past to activate either the goal of impression formation or of memorization. In this priming task, participants constructed grammatically correct sentences out of sets of five words presented in a scrambled order. In our study, participants were asked to make grammatically correct sentences out of the set of words given to them.

In addition to testing whether religious priming can reduce hypothetical bias in WTP studies, we also explore whether religious priming affects either all people or only those who consider themselves religious. The induced value setting provides the opportunity to use the results from non-priming treatments as a benchmark and in turn observe whether behavior is consistent with the benchmark across different treatments.

## **II. Religion and preference elicitation**

The impact of religion on human lives is evident throughout history. More recently, some scholars have paid closer attention to the issue of religious beliefs and consumers' preferences. Here, we define religion as a belief system which includes God and/or a supernatural being (Bernardin, 2006). Religiosity then, can be defined as a belief in God accompanied by a commitment to follow certain principles set by God (McDaniel & Burnett, 1990). Religions contain plenty of rules, norms and prohibitions that believers are asked to respect; hence religion provides perspective norms about what to do and when to do it (Silberman, 2005). The cornerstone of religious orthodoxy is the reference to an external authority in order to influence people's attitudes (Deconchy, 1980).

So, can religion influence preference elicitation? Effects of religion on individual consumer behaviour have been identified in several psychological studies. Religiousness tends to espouse values such as charity, honesty and tolerance, especially towards fellow adherents (Schoenfeld, 1978). For example, religious individuals are more likely to respond to an appeal for charity than non-religious individuals (Malhotra, 2010).

Argyle (2000) showed that religious people are more prone to prosocial behaviour in religious contexts such as towards fellow church members or church donations. Shariff and Norenzayan (2007) also found that religious primes increased prosocial behavior in an

anonymous Dictator game. Perrin (2000) suggests that religiosity effects are stronger when people self-report prosocial behavior or feel that others might be watching. They also found that religious college students cheat less when given the opportunity to be dishonest. Similarly, Mazar et al. (2008) found that drawing people's attention to moral standards can reduce dishonest behavior. Other studies have found a negative link between religiousness and cheating on income taxes (Grasmick, et al., 1991). Pichon et al. (2007) examined the impact of priming religious concepts on prosocial behavior and found that prosocial intentions were stronger when people had been previously subliminally primed.

Benjamin et al. (2010) studied the effect of religious primes in a series of economic games including a public goods game, a dictator game, risk/time preference tasks and labor market tasks. They found significant effects for contributions in the public good game, risk aversion and labor market reciprocity but none on discount rates and on altruistic generosity. They also found that results differ between religious groups such as Protestants, Catholics and Jews. Similar effects were found in Ahmed and Salas (2008) where implicit priming of religious concepts significantly increased prosocial behavior in Dictator and Prisoner's Dilemma games. In a quasi-experimental study, Ahmed (2009) found that highly devout students who were preparing to enter the clergy were significantly more cooperative in a public goods game and were significantly more generous in a dictator game than other students.

In addition, research has shown that religious representations can influence behaviour even if these were not related to intrinsic religiosity, level of devotion or belief in God. For instance, Randolph-Seng and Nielsen (2007) found that participants with primed religious representations cheated significantly less in a subsequent task while participant's intrinsic religious orientation had no influence on rates of cheating. Shariff and Norenzayan (2007)

could not associate a trait measure of self-reported religiosity with prosocial behavior which, on the other hand, was significantly influenced by religious primes. A more recent study showed that overall level of religiosity was unrelated to cheating as well but that viewing God as a more angry and punishing agent predicted more honest behavior (Shariff & Norenzayan, 2011).

In summary, religion has been found to be associated with several types of prosocial behaviour such as honesty, trust, and cooperation, which are at the heart of most religions' doctrines. Thus, the issue of hypothetical bias provides an important motivation for the testing and application of religious priming in economic valuation research.

### **III. Experimental design**

#### Overview

In light of the above cited findings, the main research question we would like to address is whether religious priming can lead to more sincere preference revelation in valuation research. Since we conduct an induced value experiment, by sincere bidding we refer to bids that are close to induced values. Our subjects took part in a multi-round auction in which they submitted bids for their assigned induced values under real (i.e., non-hypothetical) and hypothetical treatments. Prior to the auction, we carefully explained how a second price auction works. For the induced value procedure, we sold an unspecified “good” and resold it to the experimenter at the market clearing price (2<sup>nd</sup> highest bid) at the end of the procedure. Our induced value auction experiment closely follows the procedures used by Jacquemet et al. (2010) and Shogren et al. (2001).

We examine how religious priming affects bidding behavior in induced value auctions using the following treatments: (1) hypothetical auction without religious priming, (2) hypothetical auction with religious priming, (3) hypothetical auction with neutral priming, (4) non-hypothetical auction without religious priming, (5) non-hypothetical auction with religious priming, and (6) non-hypothetical auction with neutral priming. Neutral priming treatments are necessary to ensure that effects do not arise purely due to the nature of the descrambling task but rather due to activation of religious representations. With the exception of the participants in treatments (1) and (4), subjects were either exposed to a set of neutral words (in the neutral priming treatments) or to a combination of neutral words and religion-related words (in the religious priming treatments) prior the conduct of the induced value auctions. At the end of the auctions session, participants were then asked to complete a questionnaire containing a series of questions about personal attitudes, religiosity, and demographics. Table 1 exhibits our experimental design.

### Experimental procedures

We only used one proctor (i.e., one of the authors) for all sessions. A conventional lab experiment was conducted using the z-Tree software (Fischbacher, 2007) . Subjects consisted of undergraduate students at XX university (removed for peer review; to be adjusted upon publication) and were recruited using the ORSEE recruiting system (Greiner, 2004). The nature of the experiment was not mentioned during the recruitment. Each subject participated in one session only. The size of the groups was exactly 8 subjects per session. Each subject in every round was endowed with a different induced value. The sets of induced values were randomly drawn from 8 values. The induced demand curve is identical in all treatments and is defined by: {1.68, 2.84, 3.41, 4.26, 5.49, 6.62, 7.23, 8.70}. All monetary values are



expressed in Euros (€). The auction was repeated eight times (i.e., 8 rounds) permitting all possible permutations among individual induced values. Each bidder experienced each induced value once, and the entire demand curve was induced in every round. None of the bidders knew anything about the other bidders' induced value or the induced demand curve. (Hypothetical) profits in the (hypothetical) real treatments are equal to the difference between the induced value and the price the winning bidder pays for the good (the second highest bid). If a bidder did not purchase the good, profits were zero for that round. The only information posted between rounds was the loss/profit of the previous round, if any. Each session lasted less than an hour. In total, 96 subjects participated in our six treatments.

Each session consisted of different phases: the training phase, the priming manipulation phase (except for the non-priming treatment), the auction phase and the post-auction phase. After arriving at the lab, subjects were randomly assigned to a computer. In both the real and hypothetical treatments, each subject received a 5€ show-up fee and a 10€ participation fee. All transactions were completed at the end of the experiment. We made clear to participants in the hypothetical treatments that payments were fixed while participants in the real treatments were told that their payments depend on their decisions. Before the actual auction, subjects were given detailed instructions on how a second price auction works and a numerical example of how bids will be sorted. Subjects also participated in a practice auction (i.e., hypothetical two-round induced value auction) to fully familiarize themselves with the procedure.

#### Priming manipulation phase

This phase was administered in all the treatments except the no-priming treatments, just before the eight-round auction took place. Subjects were provided a paper and pencil

word-descrambling task and were given an example of how this task works and what do they have to do. The task presented subjects with a series of 10 scrambled sentences. Each sentence consisted of five words. Subjects were then told that their task was to read each scrambled sentence carefully and form a sentence that makes sense by deleting one of the words. In the religious priming treatments, participants were presented with 10 sentences, half of which contained words related to religion (i.e., spirit, divine, God, sacred, prophets). Participants in neutral priming treatments were presented with the same task except that the five religion related sentences were replaced with neutral sentences unrelated to religiousness. Appendix A illustrates the religious priming manipulation procedure. These scrambling sentence tasks have been used in Inzlicht and Tullett (2010) and Shariff and Norenzayan (2007) to prime religiosity.

As is standard practice in experiments of implicit priming manipulation, subjects were asked after the end of the experiment if they noticed “a theme” in the words to which they were initially exposed. All subjects reported unawareness of the goal-activation manipulation.

## **IV. Results**

### **Descriptive analysis**

We first consider aggregate behavior by round in each treatment. Table 2 provides raw data on observed behavior by treatment and round. In each treatment, aggregate induced demand (ID) equals 80.46€ (this is the sum of induced values). Table 2 shows the aggregate revealed demand (RD) which equals the sum of observed bids as well as the ratio of RD/ID in percentage points. Figure 1 illustrates the RD/ID ratio graphically. The red solid line signifies perfect demand revelation and is the benchmark. It is evident from Table

2 and Figure 1 that there is a significant difference between the hypothetical and real no priming treatments (standard induced value auctions). When real monetary incentives are introduced, average demand revelation drops significantly from 143.5% to 102.1%, which is almost perfectly demand revealing. Table 3 summarizes aggregate bidding behavior by treatment and induced value. Demand revelation improves in both hypothetical and real treatments with increased induced values.

A similar picture can be drawn from Table 4 which shows the summary statistics of experimental data. In the no-priming conditions, mean bids in the real treatments were only slightly larger than mean induced values by 0.10€ (S.D.=2.55€) while the ratio of bids to induced values is 1.04. On the other hand, mean bids in the hypothetical treatments are 2.19€ larger than mean induced values (S.D.=2.91) and the ratio of bids to induced values is 1.51. Figure 2 shows the distribution of bids by treatment. Solid lines represent perfect demand revelation. It is evident that when there is no priming, there is a higher dispersion with hypothetical bids than with real bids. Bids in the real treatment are distributed around the solid line.

The significant difference between real and hypothetical treatments in the no-priming condition demonstrates the existence of hypothetical bias. Results suggest then that monetary incentives are enough to eliminate this bias; however in practice this is not always feasible in the field (e.g., when dealing with non-market goods or with market goods that have not been developed yet). So can religious concepts induce honesty and thus more honest answers? The corresponding rows in Table 3 show that priming religiousness can indeed improve demand revelation in the hypothetical context since aggregate demand revelation drops from an average of 143.5% in hypothetical without religious priming to 133.8% in hypothetical with religious priming. This improvement in more truthful demand

revelation is, however, still far from the almost perfect demand revelation of 102.1% achieved in the non-hypothetical without priming treatment. To examine whether it is the priming task alone that causes this effect, we compare these findings with those in the neutral priming treatment. We can see that this is not the case since the neutral primes hardly caused any change in the aggregate demand revelation in the hypothetical treatments (i.e., 143.5% under no priming and 143.9% under neutral priming).

The picture is quite different for the non-hypothetical treatments with priming. Religious priming and neutral priming are on aggregate better demand revealing as compared to the hypothetical treatments (i.e., 129.2% for religious priming and 128.9% for neutral priming compared to 133.8% and 143.9% for the hypothetical treatment counterparts, respectively) but not as good as the non-hypothetical with no-priming treatment. So what does this mean? We can only speculate but we believe that this result might be due to a clash between religious norms and market norms. For example, Gneezy and Rustichini (2000) conducted an experiment in a group of day-care centers in Israel where parents were coming later than the due time to collect their children. In one group they introduced a fine for late arrival. The fine, however, had a long-term negative effect; parents chose to be late more often. Once the fine was removed the behavior of the parents did not change – they continued to pick up their children late. Ariely (2009) discussed how these findings support the fact that we live in two worlds: the world of social norms and the world of market norms. Whenever market norms are introduced into the world of social norms, relationships are disturbed and recovering from it may be difficult.

A similar explanation might be in place with our data as well. From our data it looks like when religious norms collide with market norms, market norms partially go away. We say partially because demand revelation improves when compared to the hypothetical

treatment but is worse than the non-hypothetical no-priming treatment. Table 4 indicates that mean bids in the real religious priming treatment differ by 1.47€ (S.D.=2.92) from mean induced values while the difference in the hypothetical treatment is 1.70 (S.D.=3.69). However, demand revelation in both treatments is far worse than demand revelation under monetary incentives alone.

### Econometric analysis

By specifying the bidding function as linear in induced value, we can directly test the assumption of perfect revealing bids.

$$bid_{it} = b_0 + b_1 IV_{it} + \rho_t + u_i + \varepsilon_{it} \quad (1)$$

In (1)  $bid_{it}$  is subject  $i$ 's bid in round  $t$ ,  $IV_{it}$  denotes subject  $i$ 's induced value in round  $t$ ,  $\rho_t$  are fixed round effects,  $u_i$  are individual specific random effects and  $\varepsilon_{it}$  is a period specific error term. Wald tests of  $H_0 : b_0 = 0, b_1 = 1, \rho_t = 0$  can provide a formal test of perfectly demand revealing bids. Econometric results of model (1) are displayed in Table 5 while Wald tests are displayed in Table 6. Equation (1) can easily be extended to account for demographics. We reach similar conclusions when demographics are added. Results are exhibited in Appendix B.<sup>1</sup>

With respect to the hypothetical treatments, Table 6 shows that we reject the null of perfectly demand revealing bids for the case of no priming or neutral priming. However, we cannot reject the null in the treatments where we primed subjects with religious concepts. Consistent with the picture drawn from Figures 1 and 2, religious primes seemed to have

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<sup>1</sup> A one-way analysis of variance for age (F-stat=0.69, p-value=0.63) and Fisher's exact tests for gender (p-value=0.26), income (p-value=0.41) and household size (p-value=0.38) indicate there are no statistically significant differences between treatments, thus we would not expect a significant influence of demographics on our results.

induced more sincere bidding behavior in a hypothetical valuation context. When it comes to the treatments with real economic incentives (i.e., non-hypothetical treatments), our results suggest that neutral priming does not induce sincere bidding. We could not reject the null of perfect demand revelation however for both the no priming as well as the religious priming conditions. In summary, our results based on our econometric analysis imply that religious priming helps improve demand revelation or truthful bidding in hypothetical contexts but is not needed in non-hypothetical contexts.

To further explore the role of religiousness in deviations from induced values and to examine whether belonging in a specific dogma or being an atheist has a differential effect on the priming treatments, we regressed the absolute difference between bids and induced values on a variety of religiosity measures. We asked subjects in all our sessions to indicate the religious dogma they belong to with options ranging from Christian Orthodox, to other Christian denomination, to atheist or none of the above. We also asked our subjects to indicate the extent to which they believe in a God on a 7-point likert scale. About 65% of our subjects indicated being a Christian Orthodox and scored four or higher on the 7-point likert scale asking whether they believe in a God. Subjects who indicated that they are atheist or who scored less than four on the 7-point likert scale asking whether they believe in a God were classified as atheists (Shariff & Norenzayan, 2007). To measure religiosity, we asked subjects to indicate the importance of God in their life, importance of religion on their life as well as how often they pray on a 7-point likert scale. We then formed a measure of personal religiousness following Saroglou, et al. (2009) . Finally, to form a measure of religious donations, we used data on a question asking subjects whether they had donated any money to religious organization or church during the past year (McKay, et al., 2010). We estimated a random effects regression of the form:

$$|bid_{it} - IV_{it}| = \left( \begin{array}{l} b_0 + b_1Hyp + b_2NeutPr + b_3ReligPr + b_4Hyp \times ReligPr \\ + b_5Hyp \times NeutPr + b_6Religiousness \\ + b_7ReligPr \times Religiousness + b_8Christian \\ + b_9ReligPr \times Christian + b_{10}Atheist \\ + b_{11}ReligPr \times Atheist + b_{12}Donation \end{array} \right) + \rho_t + v_i + e_{it} \quad (2)$$

In (2)  $v_i$  are individual specific random effects and  $e_{it}$  is a period specific error term. *Hyp* is a dummy indicating the hypothetical treatment, *NeutPr* is a dummy for the neutral priming treatment, *ReligPr* is dummy for the religious priming treatment and *Religiousness*, *Christian*, *Atheist*, *Donation* are the religiosity measures described above. We also included several interaction terms to check whether religiosity measures have a differential effect between treatments.

Table 7 exhibits regression results. We find that in all cases, religiosity measures cannot explain discrepancies between bids and induced values. Thus, we can be confident that the differences on demand revelation we observed between the treatments can be solely attributed to the treatment alone and not on differences on individual religious traits.

## V. Conclusions

In this article, we tested the impact of priming religious concepts as a truth telling device in eliciting consumer preferences. In our experiments we subliminally primed subjects with religious concepts under both hypothetical as well as real economic incentives. Our results suggest that monetary incentives are adequate in eliminating hypothetical bias. However, since financial incentives are not always possible in empirical practice, we find that priming religiousness can improve demand revelation in hypothetical contexts. All in all, we observed that induced religiosity can be as effective as real

monetary incentives in inducing bids that are close to induced values in the context of a second price Vickrey auction. We also show that the results are not due to differences in religiosity traits between subjects.

Our results are important since in many valuation studies it is not always feasible to place individuals in a truth telling context (e.g., with real monetary incentives). Our findings generally suggest that subliminal religious primes could eliminate hypothetical bias in hypothetical valuation studies. Hence, our findings may open new avenues in non-market valuation research. Another interesting venue would be to examine how priming techniques may affect efficiency in games of cooperation or bargaining in the absence of monetary incentives.



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**Table 1.** Experimental design

	<b>No priming</b>	<b>Neutral priming</b>	<b>Religious priming</b>
<b>Real</b>	(2 sessions) x (8 subjects)	(2 sessions) x (8 subjects)	(2 sessions) x (8 subjects)
<b>Hypothetical</b>	(2 sessions) x (8 subjects)	(2 sessions) x (8 subjects)	(2 sessions) x (8 subjects)

**Table 2.** Induced value bidding behavior by treatment and round

		Round								Total
		1	2	3	4	5	6	7	8	
Aggregate demand		80.46	80.46	80.46	80.46	80.46	80.46	80.46	80.46	643.68
Treatments										
Hypothetical	No primes	102.84	101.83	121.49	120.36	122.94	103.52	120.33	130.28	923.59
	(%)	127.82	126.56	150.99	149.59	152.80	128.66	149.55	161.92	143.49
	Religious primes	98.33	97.79	106.41	118.69	117.49	119.34	109.62	93.27	860.94
	(%)	122.21	121.54	132.25	147.51	146.02	148.32	136.24	115.92	133.75
	Neutral primes	109.74	113.80	112.42	111.90	113.02	124.03	117.99	123.24	926.14
	(%)	136.39	141.44	139.72	139.08	140.47	154.15	146.64	153.17	143.88
Real	No primes	77.09	79.95	87.79	84.25	78.65	85.13	80.49	83.67	657.02
	(%)	95.81	99.37	109.11	104.71	97.75	105.80	100.04	103.99	102.07
	Religious primes	100.93	99.76	104.43	96.66	101.19	112.79	104.13	111.72	831.61
	(%)	125.44	123.99	129.79	120.13	125.76	140.18	129.42	138.85	129.20
	Neutral primes	96.58	90.11	99.78	108.39	111.57	112.89	99.05	111.60	829.97
	(%)	120.03	111.99	124.01	134.71	138.67	140.31	123.10	138.70	128.94

Notes: The aggregate demand row shows the aggregate induced demand (sum of induced values). For each treatment the upper row gives the aggregate revealed demand (sum of bids) in each round (in columns) and across all rounds (in last column). The lower figure (the % row) gives the ratio of the revealed demand to the aggregate induced demand.

**Table 3.** Induced value bidding behavior by treatment and induced value

		Induced value								
		1.68	2.84	3.41	4.26	5.49	6.62	7.23	8.70	Total
Aggregate demand		26.88	45.44	54.56	68.16	87.84	105.92	115.68	139.2	643.68
Treatments										
Hypothetical	No primes	52.93	72.08	82.21	93.94	134.32	148.92	160.83	178.36	923.59
	(%)	196.91	158.63	150.68	137.82	152.91	140.60	139.03	128.13	143.49
	Religious primes	37.57	57.64	89.88	105.43	106.65	130.72	172.88	160.17	860.94
	(%)	139.77	126.85	164.74	154.68	121.41	123.41	149.45	115.06	133.75
	Neutral primes	57.99	66.41	78.56	108.23	126.31	145.83	155.09	187.72	926.14
	(%)	215.74	146.15	143.99	158.79	143.80	137.68	134.07	134.86	143.88
Real	No primes	33.56	44.27	57.77	67.27	85.09	114.80	110.85	143.41	657.02
	(%)	124.85	97.43	105.88	98.69	96.87	108.38	95.82	103.02	102.07
	Religious primes	54.64	65.40	69.01	94.48	117.46	120.13	142.09	168.40	831.61
	(%)	203.27	143.93	126.48	138.62	133.72	113.42	122.83	120.98	129.20
	Neutral primes	49.87	73.35	68.50	89.59	112.74	128.61	142.02	165.29	829.97
	(%)	185.53	161.42	125.55	131.44	128.35	121.42	122.77	118.74	128.94

Notes: The first row shows the induced values assigned to subjects. The second row shows the aggregate induced demand in each treatment (induced value x 16). For each treatment the upper row gives the aggregate revealed demand (sum of bids) for each assigned induced value (in columns) and across all induced values (in last column). The lower figure (the % row) gives the ratio of the revealed demand to the aggregate induced demand.

**Table 4.** Descriptive statistics

	Treatments					
	Hypothetical			Real		
	No primes	Religious primes	Neutral primes	No primes	Religious primes	Neutral primes
	Mean bid (sd)					
Bid	7.22 (3.93)	6.73 (4.50)	7.24 (3.80)	5.13 (3.39)	6.50 (3.73)	6.48 (3.71)
IV	5.03 (2.25)	5.03 (2.25)	5.03 (2.25)	5.03 (2.25)	5.03 (2.25)	5.03 (2.25)
Bid-IV	2.19 (2.91)	1.70 (3.69)	2.21 (2.71)	0.10 (2.55)	1.47 (2.92)	1.46 (2.88)
Bid-to-IV	1.51 (0.70)	1.37 (0.84)	1.52 (0.64)	1.04 (0.56)	1.38 (0.92)	1.37 (0.80)



**Table 5.** Random effects regressions

<b>Variables</b>	<b>Hypothetical</b>			<b>Real</b>		
	No primes	Religious primes	Neutral primes	No primes	Religious primes	Neutral primes
Constant	0.440 (0.814)	0.316 (1.098)	0.844 (0.761)	-0.159 (0.723)	1.113 (0.867)	0.818 (0.834)
$IV_{it}$	1.191 (0.075)	1.159 (0.115)	1.196 (0.066)	0.990 (0.061)	1.033 (0.087)	1.038 (0.080)
Round dummies	YES	YES	YES	YES	YES	YES
$\sigma_u$	2.179 (0.436)	2.314 (0.508)	2.168 (0.425)	2.113 (0.412)	2.004 (0.422)	2.091 (0.427)
$\sigma_\varepsilon$	1.898 (0.132)	2.927 (0.203)	1.674 (0.116)	1.548 (0.107)	2.219 (0.154)	2.038 (0.141)
Log-likelihood	-277.774	-324.439	-264.520	-255.925	-293.175	-284.718

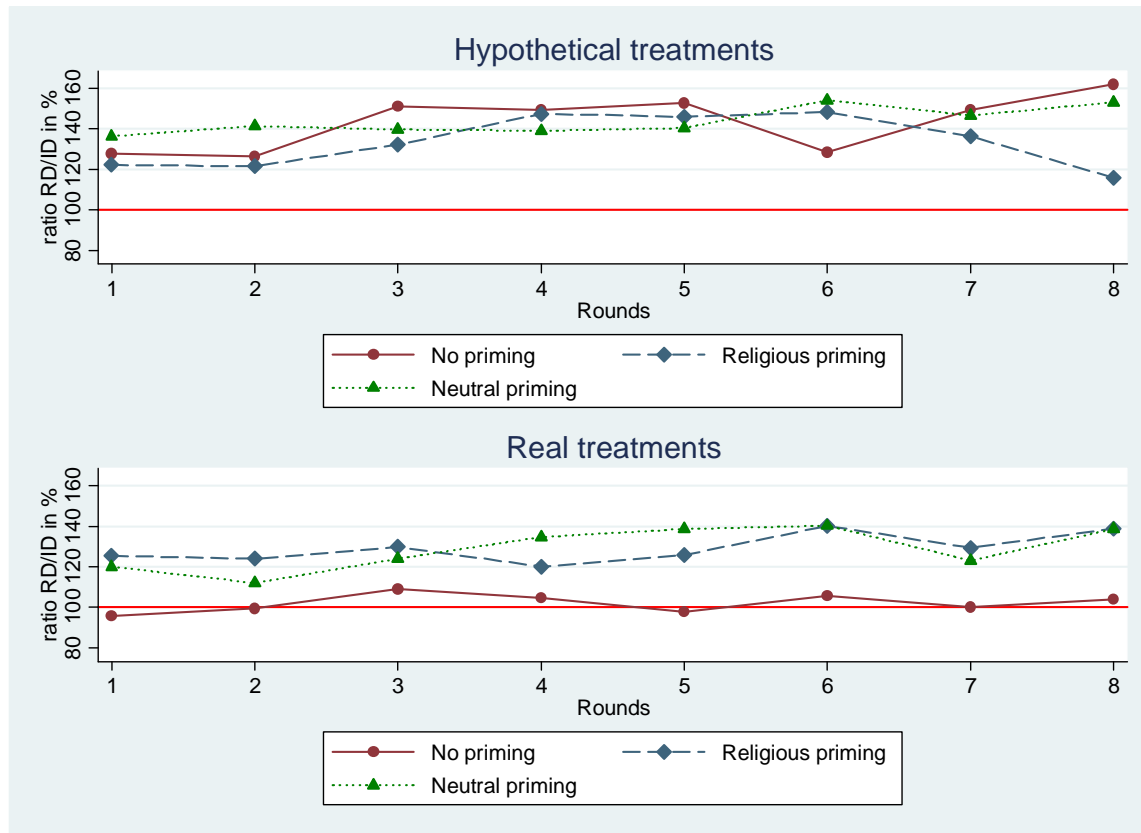
Notes: Round effects are controlled but omitted. Each column indicates a separate regression. Sample size is 128 for each treatment. Standard errors in parenthesis.

**Table 6.** Wald tests

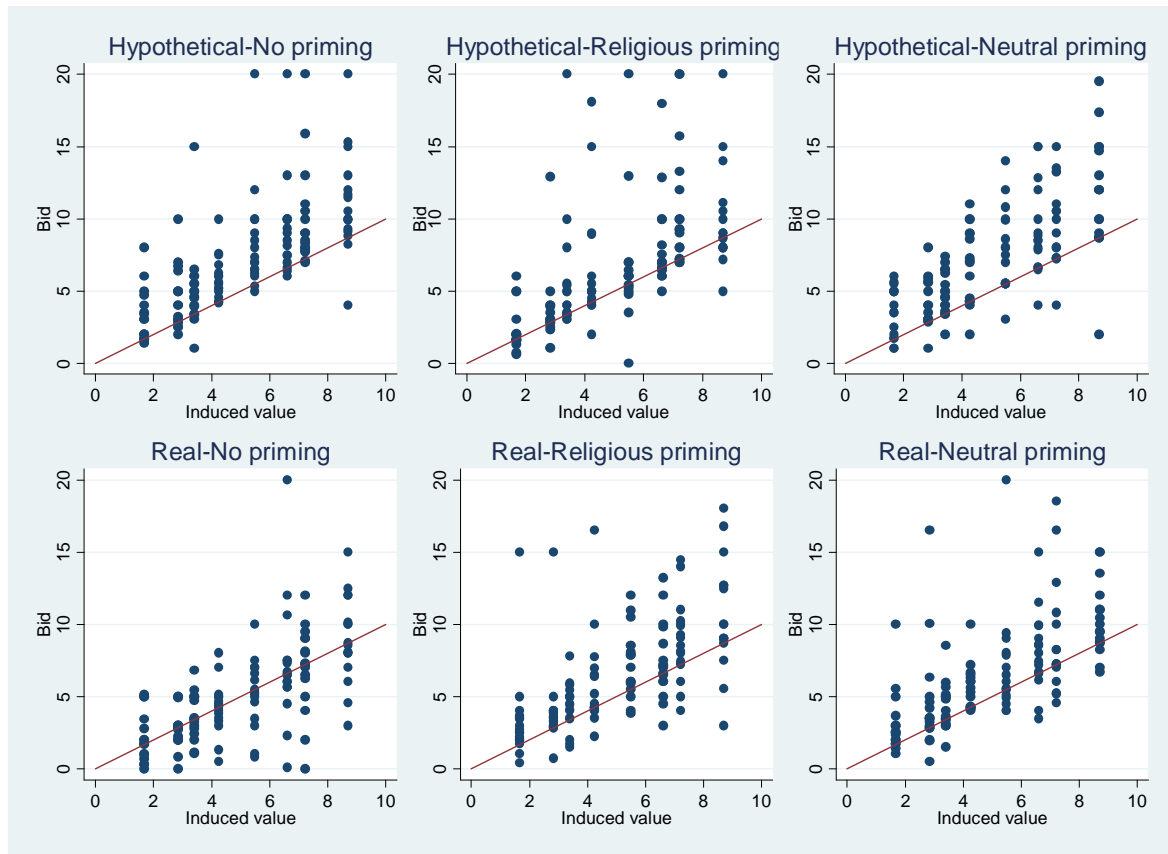
Treatments		Wald test (p-value)	Result
Hypothetical	No primes	35.93 (0.000)	H <sub>0</sub> rejected
	Religious primes	14.56 (0.104)	H <sub>0</sub> accepted
	Neutral primes	28.80 (0.001)	H <sub>0</sub> rejected
Real	No primes	2.49 (0.981)	H <sub>0</sub> accepted
	Religious primes	10.46 (0.314)	H <sub>0</sub> accepted
	Neutral primes	14.72 (0.098)	H <sub>0</sub> rejected

**Table 7.** Random effects regression of differences in bids and induced values

<b>Variables</b>	<b>Coef. (Std. Error)</b>
<i>Constant</i>	1.588 (1.661)
<i>Hyp</i>	0.595 (0.675)
<i>ReligPr</i>	-0.288 (2.272)
<i>Hyp</i> $\times$ <i>ReligPr</i>	-0.281 (0.983)
<i>NeutPr</i>	0.184 (0.676)
<i>Hyp</i> $\times$ <i>NeutPr</i>	0.046 (0.949)
<i>Religiousness</i>	0.038 (0.074)
<i>Religiousness</i> $\times$ <i>ReligPr</i>	-0.053 (0.123)
<i>Christian</i>	-1.182 (1.423)
<i>Christian</i> $\times$ <i>ReligPr</i>	2.054 (1.885)
<i>Atheist</i>	-0.367 (1.483)
<i>Atheist</i> $\times$ <i>ReligPr</i>	-0.637 (2.036)
<i>Donation</i>	0.486 (0.460)
Round dummies	YES
$\sigma_v$	1.753 (0.158)
$\sigma_e$	1.955 (0.054)



**Figure 1.** Ratio of revealed demand over induced demand in %



**Figure 2.** Distribution of bids by treatment

## Appendix A: Priming manipulation task

Please complete the following verbal fluency task. Do your best to complete every item

### 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

### Religious Primes

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 \_\_\_\_\_

### Control Primes

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 \_\_\_\_\_

Note: Subjects did not see the words in bold but in normal font

## Appendix B:

**Table B1.** Random effects regressions

Variables	Hypothetical			Real		
	No primes	Religious primes	Neutral primes	No primes	Religious primes	Neutral primes
Constant	-5.398 (4.478)	-8.804 (11.537)	7.627 (3.589)	-10.866 (7.435)	5.558 (5.405)	-16.514 (4.987)
$IV_{it}$	1.191 (0.075)	1.159 (0.115)	1.196 (0.066)	0.990 (0.061)	1.033 (0.087)	1.038 (0.080)
<i>Age</i>	0.108 (0.153)	0.141 (0.289)	-0.188 (0.111)	0.378 (0.312)	-0.251 (0.180)	0.785 (0.173)
<i>Gender</i>	-0.158 (1.266)	1.137 (1.473)	1.299 (1.210)	-0.825 (1.437)	1.498 (1.386)	0.049 (0.792)
<i>Income</i> <sub>2</sub>	1.731 (1.610)	2.436 (2.774)	-2.581 (1.736)	0.468 (2.072)	-0.176 (1.688)	1.265 (0.897)
<i>Income</i> <sub>3</sub>	1.549 (2.225)	1.386 (2.928)	-1.959 (1.777)	1.088 (1.805)	-0.072 (1.761)	0.431 (1.150)
<i>Hsize</i>	0.592 (0.480)	0.902 (1.402)	-0.313 (0.471)	0.507 (0.468)	0.002 (0.540)	-0.335 (0.446)
Round dummies	YES	YES	YES	YES	YES	YES
$\sigma_u$	2.294 (0.557)	2.690 (0.691)	2.048 (0.496)	2.184 (0.519)	2.009 (0.518)	1.066 (0.349)
$\sigma_\varepsilon$	1.898 (0.132)	2.927 (0.203)	1.674 (0.116)	1.548 (0.107)	2.219 (0.154)	2.038 (0.141)
Log-likelihood	-272.840	-318.725	-258.942	-250.158	-287.639	-273.050

Notes: Round effects are controlled but omitted. Each column indicates a separate regression. Sample size is 128 for each treatment. Standard errors in parenthesis. *Gender* is a dummy for males. *Income*<sub>2</sub> (*Income*<sub>3</sub>) is a dummy indicating household's income position is average (above average or better). *Hsize* indicates household size.

**Table B2.** Wald tests

Treatments		Wald test (p-value)	Result
Hypothetical	No primes	38.250 (0.001)	H <sub>0</sub> rejected
	Religious primes	14.550 (0.410)	H <sub>0</sub> accepted
	Neutral primes	37.180 (0.001)	H <sub>0</sub> rejected
Real	No primes	6.580 (0.950)	H <sub>0</sub> accepted
	Religious primes	15.360 (0.354)	H <sub>0</sub> accepted
	Neutral primes	62.570 (0.000)	H <sub>0</sub> rejected