

# Moral Incentives in Credit Card Debt Repayment: Evidence from a Field Experiment

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We study the role of morality in debt repayment, using an experiment with the credit card customers of a large Islamic bank in Indonesia. In our main treatment, clients receive a text message stating that “non-repayment of debts by someone who is able to repay is an injustice.” This moral appeal decreases delinquency by 4.4 percentage points from a baseline of 66 percent and reduces default among customers with the highest ex ante credit risk. Additional treatments help benchmark the effects against direct financial incentives and rule out competing explanations, such as reminder effects, priming religion, and provision of new information.

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## I. Introduction

The ability to collect debts is one of the main pillars of any financial system. While economists have extensively examined the importance of screening, monitoring, and reputational considerations, little attention has been paid to the role of morality in establishing a norm of debt repayment. This is quite surprising, given that throughout history—from ancient philosophy to contemporary debates—questions of debt and debt repayment have often been closely associated with issues of morality. In Plato's *Republic*, for example, Socrates defines the meaning of justice as “telling the truth and repaying one's debts.”<sup>1</sup> More recently, a debate about the morality of defaulting on one's mortgage or student loan in times of economic distress has featured prominently in the news media.<sup>2</sup> Issues of morality have also played a role in the context of sovereign debt, for example, in debates about defaults and debt forgiveness in countries such as Argentina or Greece.

In this paper, we study the role of moral considerations in debt repayment, using a field experiment with the credit card customers of a large bank in Indonesia. Our experiment is set in the context of Islamic banking, which is a large and rapidly growing industry in Indonesia and around the world, with currently more than 300 banks in over 75 countries and approximately US\$1.5 trillion in assets (World Bank 2014) offering Sharia-compliant financial products. While Islamic banks typically offer the same range of consumer financial products that are also available at conventional banks, they often emphasize the ethical dimension of their business model, thus providing an environment in which communications with both financial and moral content are natural.<sup>3</sup>

We use this setting to conduct a series of experiments in which late-paying credit card customers receive messages alerting them to the moral

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<sup>1</sup> There are also numerous references to the morality of debt and debt repayment in religious texts. An example from the Bible is Romans 13:7–8: “Give to everyone what you owe them . . . and let no debt remain outstanding.” An example from Islam is *Shahih al-Bukhari* 3:575: “The best among you are those who repay their debts handsomely.” Many languages, including German and Hebrew, share the same word for “debt” and “guilt.” Nietzsche (1887) offers a detailed account of this association and its influence on the development of moral norms.

<sup>2</sup> See, e.g., Lee Siegel, “Why I Defaulted on My Student Loans,” *New York Times*, June 6, 2015; “Times Op-Ed Goes All In on Student Debt Silliness,” *Forbes*, June 8, 2015.

<sup>3</sup> Not all clients of Islamic banks are motivated by religious considerations. In fact, 10 percent of credit card clients at our partner bank are non-Muslims. This is roughly the same as the share of non-Muslims in the Indonesian population.

consequences of nonrepayment. The design of our experiment takes advantage of the fact that our partner bank had already introduced a mobile phone text messaging system that sends reminders to customers who have not made the required minimum payment 1 day after the due date. Working with the bank, we developed a set of additional text messages, which included basic reminders, placebo messages, messages containing a moral appeal, and messages highlighting the credit reputation consequences of delinquency. These messages were randomly assigned at the individual customer level and sent to customers who had missed the repayment due date and had still made no payment 2 days before the end of a 10-day grace period granted by the bank. If no payment is received by the end of this grace period, the customer is considered delinquent, the credit card is blocked, the account is charged a late payment fee, and the customer is reported to the Indonesian credit registry, which generally precludes borrowing from any formal sector lender for at least 24 months—the time period for which the negative entry remains on record—even if the debt is eventually repaid. The main outcome of interest in our experiments is therefore the discrete choice of either repaying before the end of the grace period or becoming delinquent.

In the main treatment condition of our experiment, late-paying customers receive a text message that highlights that not repaying a debt when one is able to repay violates a moral norm. The message refers to the Islamic doctrine on nonrepayment of debts using a quote from the *Shahih-al-Bukhari*, one of the main religious texts of Sunni Islam, which serves as an important source for the interpretation of Islamic law and is widely known and respected among Indonesian Muslims:<sup>4</sup> “The Prophet (Peace and blessings be upon Him) says: ‘non-repayment of debts by someone who is able to repay is an injustice’ (Imam al-Bukhari). Please repay your credit card balance at your earliest convenience. Call [customer service number].”

The design of our experiment has several important features that help us identify the effect of moral appeals on debt repayment. First, debt repayment is a common and consequential financial decision, which we are able to study using a real-stakes field experiment integrated into the credit card repayment cycle of a large bank. Second, the messages in our experiment are sent through the bank’s automated text messaging system, which allows us to address the moral appeal to delinquent customers directly. Third, the bank routinely uses text messages to communicate with its customers, and messages with religious or moral content are not uncommon in this context. Therefore, the channel of communication and the content of the messages in our experiment are credible and natural in our setting. Finally, using a number of placebo messages and follow-up surveys,

<sup>4</sup> The *Shahih-al-Bukhari* is one of the six major *hadith* collections of Sunni Islam (*Kuttub al-Sittah*). It reports on the sayings, deeds, and teachings of the Prophet and is widely used in the application of Islamic law.

we are able to examine the mechanism through which moral appeals affect behavior.

We find that moral appeals increase debt repayment. In our baseline specification, receiving the moral message decreases the share of delinquent customers by 4.4 percentage points compared to a baseline share of 66 percent in the control group. To assess the economic magnitude of this effect, we benchmark the impact of moral incentives against that of direct and indirect financial incentives. Our first benchmark is a cash rebate treatment in which past-due customers received a message from the bank that offered them a repayment rebate in the form of principal reduction equal to 50 percent of their current minimum payment conditional on making a payment before the end of the grace period. Point estimates indicate that moral incentives are more effective than this substantial financial incentive, and we can bound the effect of the cash rebate to be no more than 1.2 times the impact of the moral message. This implies that the bank would have to offer customers a principal reduction equal to at least 6 percent of median monthly income to generate the same increase in repayment rates observed in the moral incentives group. In a second benchmarking treatment, past-due customers received a text message that highlighted the consequences of delinquency on the future ability to obtain credit. This message induces the strongest (9.8 percentage point) reduction in delinquency rates among all of the messages sent as part of our experiment. While we cannot directly assess the extent to which this treatment provides new information as opposed to bringing customers' attention to something they already knew, the message highlights that the financial stakes of the repayment decision are meaningful—especially through the effect of a negative credit registry entry on the ability to obtain credit—and the results show that customers respond strongly when they are reminded of these stakes.

We then use a series of interventions to rule out alternative mechanisms that are unrelated to the moral appeal but could trigger repayment in response to receiving the moral message. First, could the impact of the moral message be due to a simple reminder effect? To test this possibility, a group of customers were sent a simple reminder message that did not contain a moral appeal. This message had no significant effect on repayment, which rules out this channel. Second, does the moral appeal work because it primes customers on religion or evokes a religious frame of mind? We examine this possibility, using a placebo message, which included a quote from the Prophet that is taken from the same religious text as the moral message but makes no reference to the Islamic doctrine on debt repayment, while still reminding the customer to repay her debt. We find that this message has no impact on repayment, which rules out an explanation of our main result based on priming religion. Third, does the moral appeal work because receiving a strongly worded message signals that the

bank is particularly committed to debt collection? To test this possibility, we surveyed customers who had received either no message, the basic reminder, or one of several different versions of the moral message, 1 day after the final repayment deadline. In the survey, customers were asked “How committed do you think [bank name] is to collect debt from delinquent customers?” We find no statistically significant difference in responses to this question between customers assigned to different treatments. Finally, it is worth noting that several of our text messages, including the simple reminder and the religious placebo message, were specifically designed for the experiment and had never before been received by the bank’s customers. The fact that these messages do not affect repayment also allows us to rule out the possibility that the moral appeal is effective only because it comes in the form of a particularly novel or attention-grabbing message.

Having ruled out these alternative channels, we investigate the mechanism through which moral incentives affect debt repayment. The original moral incentive message explicitly quoted the Prophet, cited the text from which the quote was taken, and employed a word of Arabic origin for “injustice” that is often associated with religion. To examine whether the moral appeal works because of its explicitly religious connotation, we implemented two variations of the moral message, which successively removed its religious components. The first message omitted the reference to the Prophet and the religious text from which the quote was taken, thus allowing us to test whether invoking a credible religious source increases the effectiveness of the moral appeal. The second message additionally replaced the Arabic-origin word for “injustice” in the original message with a less formal Indonesian word, which has the same meaning but no religious connotation. This allows us to test whether the moral appeal is effective when it is entirely unrelated to religion.

We find that all variations of the moral appeal have very similar effects, which indicates either that the pure moral statement is sufficient to trigger repayment or that customers associate even the nonreligious versions of the moral appeal with religion. To disentangle these two explanations, we conducted a follow-up survey in which customers in the treatment and control groups were read the moral appeal and asked whether they associated it with religion. The results show that this is not the case, suggesting that the effect of the moral message is driven by the moral statement it contains rather than the religious context of the message (although part of the effect may be coming from the way respondents’ religiosity interacts with the moral content of the message).

We then test whether the message continues to affect repayment when it is sent repeatedly and find that the effect of receiving the moral message a second time is very similar to that of receiving it for the first time. This indicates that the message does not work because of its novelty or

because it provides new information. Instead, our results are most consistent with the interpretation that the message temporarily draws attention to the moral dimension of the repayment decision and triggers repayment by highlighting that not repaying one's debts violates a moral norm.

Finally, we test whether, in addition to reducing delinquency, the moral message also affects default, defined by the bank as remaining more than 90 days past due. Since the card is a revolving line of credit, full default is extremely costly to the borrower and therefore is a very rare event. With baseline default rates below 0.5 percent (5 percent in our sample of late-paying customers), it is thus not too surprising that our treatments do not significantly reduce default in the full sample. There is, however, substantial variation in the ex ante credit risk of customers, and we show that the moral message significantly reduces default among customers with high predicted credit risk.<sup>5</sup>

Overall, our findings suggest that when making important financial decisions, people experience a utility cost from consciously violating a moral norm, so that moral appeals can affect behavior, even when neither the moral appeal nor the response to it is observed by others and when they do not mention a moral authority, threat of punishment, or adverse financial consequences.

This paper relates to several strands of the literature. First, our work is related to a large literature on nonmonetary incentives (Frey 1997; Akerlof and Kranton 2000; Bénabou and Tirole 2003, 2006; Gneezy 2005). In particular, we shed light on how moral appeals affect an important economic decision: the decision to repay one's debts. Moral appeals are among the most common strategies of persuasion, and many companies, for example, advertise their support for fair trade or charitable causes to influence consumer choices.<sup>6</sup> There is a body of evidence both in the lab (see Dal Bó and Dal Bó 2014) and in the field studying different types of normative appeals and their impact on a wide range of behaviors, from evasion of television license fees (Fellner, Sausgruber, and Traxler 2013) to tax compliance (Hallsworth et al. 2015, 2017), paying for newspapers (Pruckner and Sausgruber 2013), and energy conservation (Ito, Ida, and

<sup>5</sup> When we split the sample on the basis of customers' predicted ex ante credit risk, we find that the moral message reduces default by 10.5 percentage points (from a baseline rate of 13 percent) among the 10 percent of customers with the highest credit risk, by 4.2 percent (from a baseline rate of 11 percent) among the 25 percent of customers with the highest credit risk, and by 2.1 percentage points (from a baseline rate of 8 percent) for customers with above-median credit risk.

<sup>6</sup> Most closely related to our setting, a number of banks have used television commercials with moral content to get delinquent borrowers to repay their debt. For example, Indian banks have aired television and radio commercials with moral appeals made by children in an effort to persuade defaulting borrowers to repay their loans. See "Banks Make Emotional Appeals to Get Borrowers to Repay Loans," *Live Mint*, October 2016.

Tanaka 2018). However, to our knowledge, this paper is the first to provide field evidence that purely moral appeals can affect an important economic decision even in the absence of confounding factors, such as reminder effects, social effects, or changes in the perceived material cost of noncompliance. Moreover, we contribute to the literature by providing evidence of why these appeals work and of how effective they are relative to financial incentives.

Second, our work contributes to a literature that examines debt accumulation and repayment (see Agarwal et al. 2009; Agarwal, Skiba, and Tobacman 2009; Bertrand and Morse 2011; Zinman 2015). In particular, Guiso, Sapienza, and Zingales (2013) use survey data to study how moral considerations may play a role in strategic default in the mortgage market. They find that 82 percent of respondents believe that it is morally wrong to engage in strategic default and that those expressing this opinion are about 10 percentage points less likely to default strategically on their mortgages. By exploring how messages that emphasize different aspects of the repayment decision affect behavior, our results also relate to empirical work on attention and household finance (Stango and Zinman 2014; Alan et al. 2018).

Beyond helping to understand the role of moral considerations in an important economic decision, our work is also related to a literature on religion and economic behavior (see Iannaccone 1998; Barro and McCleary 2006; Clingingsmith, Khwaja, and Kremer 2009; Bénabou, Ticchi, and Vindigni 2015; Campante and Yanagizawa-Drott 2015; Cantoni 2015; Benjamin, Choi, and Fisher 2016). Identifying the effect of moral appeals linked to religion is difficult because religious activities typically combine moral, instrumental, and social motivations. For example, people may go to church because they believe it is the “right thing to do,” but they may also do so for indirect material or social benefits, such as socializing or signaling one’s beliefs or shared values. We add to this literature by showing that moral appeals can meaningfully affect behavior, even when they make no reference to a religious or moral authority, and in an environment in which the social interactions usually associated with religion are absent.

The paper proceeds as follows. In Section II, we describe the setting and experimental design. Section III presents the results. Section IV interprets our findings, and Section V concludes the paper.

## II. Experimental Design

### A. *The Credit Card*

We design a natural field experiment with the universe of late-paying customers of Indonesia’s most popular Islamic credit card. The credit card is issued by one of the country’s leading Islamic banks, which offers credit



cards as part of its portfolio of Islamic consumer finance products. Originally introduced in 2009, the card had approximately 200,000 customers at the time of our experiment.

The credit card is designed to comply with the principles of Islamic law, which, among other prescriptions, prohibits charging interest and investing in activities considered contrary to the principles of Islam. In order to be fully consistent with Islamic law, the features of the card are based on a *fatwa* (legal decree) issued in 2006 by the Indonesian Council of Islamic Scholars, which lays out the guidelines under which banks can offer Sharia-compliant credit cards. Following these rules, the credit card is structured as an *Ijarah* (service fee) contract, which means that customers pay a fee for the transaction services provided by the card instead of a variable interest rate. Customers are charged fixed annual fees of Rp 120,000 (US\$10) for a basic card, Rp 240,000 (US\$20) for a gold card, and Rp 600,000 (US\$45) for a platinum card, plus a monthly membership fee of 2.75 percent of the customer's credit limit. This monthly fee can be partially or fully refunded through a "cash rebate," which is proportional to the customer's available credit line and can range from zero to the total monthly fee.<sup>7</sup> The fee is waived entirely if there is no outstanding debt.

There is a monthly billing cycle, with a billing date on the eighteenth day of each month. The minimum monthly payment, equal to either 10 percent of the customer's total outstanding balance or Rp 50,000 (whichever amount is higher) plus possible arrears and overdrafts, is due on the eighth day of the following month. Customers who do not make the minimum payment by the due date receive a text message reminder from the bank the following day. The bank grants customers who miss the due date a repayment grace period of 10 days, which ends on the eighteenth day of each month (we refer to this date as the "repayment deadline"). Customers who fail to make the minimum payment by this date are considered delinquent and are reported to the Indonesian credit registry, the Sistem Informasi Debitur, which all banks consult before issuing credit. Even if the debt is eventually repaid, a negative entry remains on record for 24 months and generally precludes borrowing from any formal sector lender for that period of time. Additionally, delinquent customers are charged a nominal late payment fee and their card is automatically blocked.<sup>8</sup> Once

<sup>7</sup> The cash rebate is calculated as follows: cash rebate =  $2.75\% \times (\text{credit limit} - \text{amount outstanding})$ . The net monthly fee is the monthly membership fee minus the cash rebate, i.e.,  $2.75\% \times \text{amount outstanding}$ .

<sup>8</sup> Late payment fees range from Rp 15,000 to Rp 35,000 and increase over time. For example, customers who are more than 30 days late are charged additional fees ranging from Rp 20,000 to Rp 50,000. However, to be compliant with Islamic law, the bank is allowed to charge late fees only to compensate for the costs of debt collection, including follow-up and legal costs.



the customer makes the required minimum payment, the card is immediately unblocked. If a customer's payment remains outstanding for more than 90 days after the due date, the customer is considered in default, the card is permanently blocked, and the account is closed. Accounts that remain more than 120 days overdue are sent to the bank's collections department and eventually referred to an outside collections agency.

### *B. Sample Population and Random Assignment*

The population for our experiment comprises the 14,429 credit card customers who were more than 1 week past due on their minimum payment at least once during one of the six months between February 2015 and April 2016 in which the experiment was carried out.<sup>9</sup> Because some customers were late more than once during this period, there are 23,520 observations in our sample frame.<sup>10</sup>

The experiment was conducted in six waves, coinciding with the monthly credit card repayment cycle.<sup>11</sup> Each month, the bank shared with us the list of customers who had not made the minimum required payment by the sixteenth day of the month (2 days before the final repayment deadline at the end of the 10-day grace period) but had previously been current on their payment schedule (i.e., they had made the previous month's minimum payment on time). In the main experiment, we excluded from this list all customers who had previously received a text message treatment. Customers assigned to the control group in a previous month remained in the sample and could either be assigned to one of the treatments

<sup>9</sup> The experiment was conducted in February, March, May, and June 2015 and February and April 2016. We originally planned to have one treatment group that would receive restructuring offers in April 2015. However, the partner bank was not able to immediately operationalize this. Upon agreement with the bank, we then decided to pause our main intervention in April 2015 and to resume in May 2015. In May 2015, the bank attempted to implement the restructuring offers with a sample of 200 customers but faced problems with the implementation and customer response to this treatment. We exclude these 200 observations from our analysis. We also ran a small pilot with 250 customers in January 2015, whose results were similar to those of our main intervention.

<sup>10</sup> Among the universe of 14,429 customers, 8,691 were late only once, while the remainder appeared in our sample more than once: 3,052 customers were late twice, 1,414 were late three times, 579 four times, 191 five times, and 52 in all six months.

<sup>11</sup> The first three waves of the experiment were conducted in February, March, and May 2015. The last three waves were conducted in June 2015 and February and April 2016. As part of a parallel experiment for a second paper, we had two other treatment groups with customers receiving multiple text messages on the same day. We excluded those 2,000 observations from our analysis. Results are unaffected when we include these observations and are reported in the online appendix. In the note to table A.4 we also discuss some design and implementation issues that affect the interpretation of results from these additional treatments.

or form part of the control group again.<sup>12</sup> For example, in March 2015, 4,803 customers were more than 1 week late. Out of these, 1,018 had previously received a treatment message and were thus excluded from the sample; the remaining 3,785 customers were assigned to one of the treatment conditions or the control group. Following this process, we obtain a data set that includes 13,428 observations, representing 12,104 unique credit card customers.<sup>13</sup>

Eligible customers were randomly assigned to one of several treatment conditions or to a control group. As part of the bank's standard communications policy, all customers received a neutral text message reminder 1 day after they had missed the due date (i.e., when they were 1 day past due). The 4,120 customers assigned to the control group received no other text from the bank, while the 9,308 customers assigned to one of the treatment conditions received additional information through a text message sent 2 days before the repayment deadline (i.e., when they were 7 days past due). All treatments were randomly assigned at the individual customer level and delivered through text messages using the bank's existing customer notification system.<sup>14</sup> Figure 1 summarizes the credit card billing cycle and the time line of our intervention.

In February and April 2016, we conducted a separate follow-up experiment with the 898 customers who reappeared on the list of late payers and had previously received the moral message as part of the main experiment. The experiment was designed to test whether the moral message works only the first time it is sent—for example, because it is novel or conveys new information—or whether sending the message repeatedly could still affect repayment. Following the same procedure and timing as

<sup>12</sup> When looking at a long-term outcome, such as default, we exclude customers who were in the control group in a given month, reappeared in the sample in a following month, and were randomized to receive a treatment message before their long-term behavior was observed. In fact, long-term outcomes in the control condition are not observable for these customers. The outcomes are observed, however, for similar customers who were rerandomized into the control condition. To maintain representativeness, when looking at default we reweight the sample by giving more weight to these latter customers who appeared in the control group more than once.

<sup>13</sup> Of these 13,428 observations, 10,903 customers appear on the list of late payers only once, 1,088 appear twice (the first time in the control group), 104 appear three times (the first two times in the control group), six appear four times (the first three times in the control group), and one customer appears five times (the first four times in the control group). Although this approach does not affect the internal validity of our analysis, it could potentially reduce the representativeness of our sample, since in a given month, customers who previously received a treatment message could have ended up on the list of late payers if they had been assigned to the control group instead. However, given that the effect of our treatments is very similar for subjects appearing in the sample for the first time and those previously assigned to the control group, reweighting the sample to correct for the probability of being excluded does not affect our results.

<sup>14</sup> All messages were in Bahasa Indonesia, the official language of Indonesia, which is also the standard language used by the bank in all of its customer communications. Appendix fig. A.2 summarizes the experimental design.

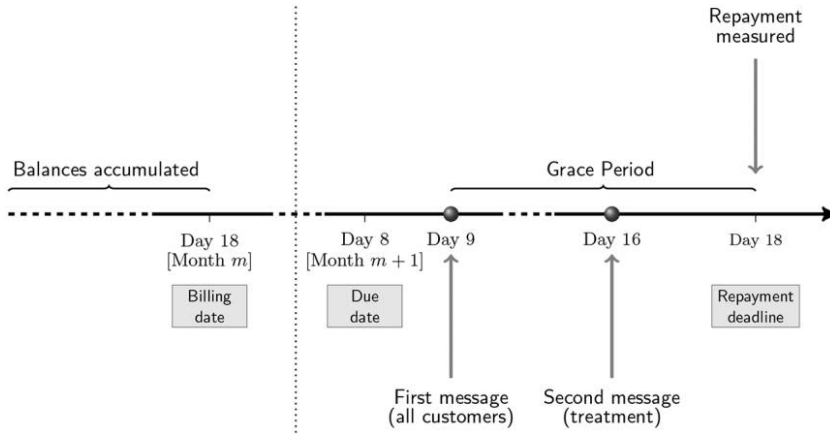


FIG. 1.—The figure shows the credit card billing cycle and timing of the intervention. Customers receive their monthly statement on the eighteenth day of each month. The due date is on the eighth day of the following month. One day later, the bank sends a simple reminder message to all late-paying customers. The repayment deadline is on the eighteenth day of the following month, at the end of a 10-day grace period. On the sixteenth day of the month (2 days before the repayment deadline), randomly assigned messages are sent to customers assigned to one of the treatment groups. Repayment status is observed at the time of the final repayment deadline, which is midnight on the eighteenth day of the following month (1 month after the billing date).

above, recurrent late payers were randomly assigned either to a control group or to a repeated message treatment group.<sup>15</sup> The 450 customers assigned to the control group again received only a neutral reminder 1 day after they missed the required minimum payment. The 448 customers assigned to the repeated moral message treatment group received a moral message identical to the one they had previously received. As in the main experiment, this message was sent 2 days before the repayment deadline at the end of the 10-day grace period granted by the bank.

### C. Experimental Treatments

#### 1. Control Group

A total of 4,120 customers were assigned to the *control* group, which forms the basis of comparison throughout the experiment. Customers in this group received a single reminder 1 day after they had missed the required minimum monthly payment: “Your [name of the card] has reached the due date. Please make a payment at your earliest convenience. If you have

<sup>15</sup> We stratify on how recently the customer had received the first moral message: 364 customers were treated 2 months before reappearing in the late-payer list, while the other 534 customers were treated for the first time between 8 and 14 months before.

already paid, ignore this text. Call [customer service number].” While all other customers received an additional message from the bank 2 days before the repayment deadline, customers in the control group received only this initial reminder.

## 2. Moral Incentives

To test the impact of moral appeals, we assigned 2,244 participants to the *moral incentive* treatment condition. In addition to the basic reminder sent to all customers who missed the due date, these customers received an additional message drawing attention to the moral implications of not repaying one’s debts. The message quotes from the *Shahih al-Bukhari*, one of the main religious texts of Sunni Islam, which reports of the teachings, deeds, and sayings of the Prophet Muhammad and serves as one of the main sources for the interpretation of Islamic law. The quote highlights the religious doctrine on repayment of debts and asks the customer to repay her outstanding balance: “The Prophet (Peace and blessings be upon Him) says: ‘non-repayment of debts by someone who is able to repay is an injustice’ (Imam al-Bukhari). Please repay your credit card balance at your earliest convenience. Call [customer service number].”

To better understand the mechanisms underlying the impact of moral appeals, we implemented two additional variations of this treatment, which varied the degree of its religious content. The first variation of the message (the *implicit moral incentive* treatment condition) removed the reference to the Prophet and the text from which the quote was taken. This message, assigned to 1,180 customers, reads, “Non-repayment of debts by someone who is able to repay is an injustice. Please repay your credit card balance at your earliest convenience. Call [customer service number].”

The second variation of the message (the *nonreligious moral incentive* treatment condition), which was assigned to 1,186 customers, not only omitted the reference to the Prophet and the source of the quote but also replaced the Arabic-origin term for “injustice” (*kezaliman*) with the standard Indonesian word (*ketidakadilan*), which is more colloquial and has no religious connotation.

The first variation of the moral message allows us to test whether a moral appeal is strengthened by invoking a credible religious source. The second message tests whether receiving a moral statement without any explicit religious connotation can affect the repayment decision.

## 3. Direct Financial Incentives: Cash Rebate

To benchmark the effect of moral appeals against direct financial incentives, we implemented a treatment consisting of a direct one-time mon-

etary incentive in the form of a large cash rebate. In this *cash rebate* treatment condition, the bank sent the standard reminder on the due date and an additional message 2 days before the repayment deadline, in which customers were offered a rebate equal to 50 percent of their currently outstanding minimum payment, conditional on making the required minimum payment by the deadline.<sup>16</sup> The rebate would then be credited against expenditures in the next billing cycle starting 3 days after the offer is made, so that the reward is available to customers practically right after they make a payment. This treatment was assigned to 336 participants, using a message which reads as follows: “This month, make your credit card payment to get a cash rebate equal to 50% of your minimum payment on your next statement. Please repay your credit card balance at your earliest convenience. Call [customer service number].”

For this treatment to serve as a useful benchmark, we need to be sure that customers do not misinterpret the offer and understand that the size of the rebate is independent of their current or future behavior. We took several steps to verify that this was the case. First, the rebate offer was designed in close cooperation with the bank, and we ensured that the wording was clear and similar to the bank’s usual customer communications.<sup>17</sup> Second, we closely monitored the treatment implementation and found no indication that customers were confused about the offer or contacted the bank with requests for clarification. Since the rebate is credited in the next billing cycle (which starts 3 days after a customer receives the financial incentive offer), one could also be concerned that customers might erroneously believe that the incentive is proportional to the payment due in the following cycle, as opposed to the current amount due, which comprises expenditures for the previous billing cycle, which ended before the rebate was offered. If customers erroneously consider the size of the rebate to be under their control, one would expect them to reduce current repayment and increase spending to increase the rebate amount. We test for this and find that neither of these patterns is present in the data. There were also no instances in which a customer disputed the rebate amount he or she received. Finally, we conduct a customer survey to measure respondents’ preferences for deposits on their checking account versus statement credit, which verified that customers value a cash rebate in the form of statement credit nearly as much as immediate cash. We can thus reliably use this treatment to benchmark the effect of moral incentives and express it in

<sup>16</sup> The current minimum payment is based on spending in the previous billing cycle and therefore is unaffected by the borrower’s current or future spending.

<sup>17</sup> The bank frequently uses rebates and discount offers in its marketing activities, so that customers in our sample were familiar with this type of offer. Moreover, as described above, a cash rebate similar to our treatment is an inherent feature of the card’s pricing structure.

terms of the size of conditional principal reductions the bank would have offer to achieve the same increase in repayment rates as the moral message.

#### 4. Indirect Financial Incentives: Credit Reputation

To test the effect of indirect financial incentives, we implemented another benchmarking treatment, which consisted of a message highlighting the consequences of nonrepayment for customers' credit record and their ability to obtain credit in the future. In this *credit reputation* treatment, customers received the standard reminder on the due date and an additional message 2 days before the repayment deadline. The message stated that nonrepayment will result in the customer being reported to the Indonesian credit registry, the Sistem Informasi Debitur, which will diminish access to credit in the future. This message was assigned to 2,000 customers and reads as follows:<sup>18</sup> "Late payments are reported monthly to Bank Indonesia Sistem Informasi Debitur (SID), which all banks consult. This will diminish your ability to get credit in the future. Please repay your credit card balance at your earliest convenience. Call [customer service number]."

#### 5. Placebo: Simple Reminder

We assigned 1,362 customers to the *simple reminder placebo* treatment. Customers in this treatment condition received the standard reminder on the due date and an additional neutral reminder 2 days before the repayment deadline.<sup>19</sup> This second reminder is similar to the standard message sent to all customers who miss the due date and makes no reference to the moral or financial implications of nonrepayment: "The due date of your [name of the card] bill was on [due date] and your payment has not been received yet. Please repay your credit card balance at your earliest convenience. Call [customer service number]."

We use this treatment to test how receiving a second reminder affects repayment through channels such as limited attention and memory. Comparing its effect to that of moral incentives allows us to distinguish the impact of moral appeals from the effect of receiving additional reminders.

<sup>18</sup> We designed two variations of this text message and randomly assigned 1,000 customers to each of two subgroups. The first subgroup received the message in the main text. The second group received a text that says "Late payments are reported monthly to Bank Indonesia Sistem Informasi Debitur (SID), which all banks can consult. Please repay your card balance at your earliest convenience. Call [customer service number]." We pool these two treatments in our analysis, since their effect on repayment is not statistically different.

<sup>19</sup> A number of customers were additionally assigned to this treatment condition in the last wave of the experiment to compare the effect of the moral incentive to that of a simple reminder on outcomes measured in a phone survey. The survey asked whether customers would like to receive the same text message again and how committed they thought the bank is to collecting debt. The survey instrument is available in the appendix.

## 6. Placebo: Religious Message

Finally, we assigned 1,000 customers to a *religious placebo* treatment condition. This treatment is designed to address the possibility that borrower behavior could be affected by priming religion.<sup>20</sup> Customers in this treatment group received the standard message on the due date and an additional message with a quote from the Prophet taken from the same source used in the moral incentive treatment condition, 2 days before the repayment deadline. However, in contrast to the moral incentive message, this quote made no reference to financial matters or debt repayment: “The Prophet (Peace and blessings be upon Him) says: ‘When Allah wishes good for someone, He bestows upon him the understanding of the Book’ (Imam al-Bukhari). Please repay your credit card balance at your earliest convenience. Call [customer service number].”

This treatment allows us to test whether moral appeals work because they highlight the moral implications of a specific action—the nonrepayment of debts—or simply because they remind recipients of the religious nature of their contract with the bank or evoke a religious frame of mind.

### D. Data and Summary Statistics

The data set we use in our analysis combines the results from the experiment with administrative data from our partner bank and information from a number of follow-up phone surveys administered to the bank’s customers.

#### 1. Administrative Data

We first obtained bank data on customer account characteristics (age, gender, religion, province, monthly income, and credit limit) for the universe of past-due customers in our sample. Table 1 reports summary statistics and presents a test of random assignment.<sup>21</sup> The median credit card customer in our sample is male, is 41 years old, has a monthly income of Rp 5 million (US\$375), has a credit limit of Rp 10 million (US\$750), and has Rp 7,739,015 (US\$580) of credit card debt.<sup>22</sup> As expected given

<sup>20</sup> Laboratory experiments have shown that religious primes can induce prosocial behavior, increasing the amount shared in dictator games (Shariff and Norenzayan 2007), reducing cheating (Randolph-Seng and Nielsen 2007; Mazar, Amir, and Ariely 2008), and increasing charitable donations (Pichon, Boccato, and Saroglou 2007). Priming religion also increases punishment of unfair behavior, but only among religiously committed subjects (McKay et al. 2011; Laurin et al. 2012).

<sup>21</sup> See app. table A.2 for summary statistics and a test of random assignment for the follow-up experiment.

<sup>22</sup> For comparison, Indonesia’s per capita income was US\$3,491 (approximately US\$291 per month) at the time of the experiment (World Bank 2014).



TABLE 1  
BALANCE AND TREATMENT CELL SIZE

	TREATMENT						p-VALUE (7)
	FULL SAMPLE (1)	Moral Incentive (2)	Simple Reminder (3)	Religious Placebo (4)	Credit Reputation (5)	Control Group (6)	
A1. Waves I, II, and III—Balance of Covariates							
Age	42.03 [9.071]	42.36 [9.317]	42.10 [8.776]	41.73 [8.717]	41.99 [9.092]	42.03 [9.195]	.631
Female	.40 [.489]	.40 [.490]	.41 [.491]	.41 [.491]	.39 [.488]	.40 [.489]	.914
Muslim	.92 [.273]	.92 [.271]	.91 [.286]	.91 [.289]	.92 [.271]	.92 [.264]	.427
Annual income (Rp million)	151.67 [836.968]	135.51 [175.295]	185.73 [1242.218]	134.86 [187.644]	177.65 [1369.992]	132.85 [201.640]	.418
Credit limit (Rp million)	13.55 [9.338]	13.93 [9.708]	13.28 [8.652]	13.77 [9.444]	13.38 [9.272]	13.55 [9.448]	.438
A2. Waves I, II, and III—Treatment Cell Size							
Wave I	2,871	400	400	400	800	871	
Wave II	2,985	400	400	400	800	985	
Wave III	1,965	200	200	200	400	965	
Total	7,821	1,000	1,000	1,000	2,000	2,821	

FULL SAMPLE (1)	TREATMENT					p-VALUE (7)	
	Moral Incentive		Nonreligious (4)	Cash Rebate (5)	Control Group (6)		
	Religious (2)	Implicit (3)					
B1. Wave IV—Balance of Covariates							
Age	42.24 [9.491]	41.82 [9.170]	42.70 [9.415]	41.98 [9.137]	42.31 [9.196]	42.38 [10.477]	.764
Female	.39 [.488]	.42 [.494]	.38 [.486]	.38 [.487]	.37 [.482]	.40 [.490]	.703
Muslim	.92 [.271]	.93 [.253]	.91 [.281]	.90 [.302]	.93 [.253]	.92 [.265]	.517
Annual income (Rp million)	134.64 [189.589]	121.99 [154.065]	132.46 [187.183]	138.35 [192.350]	152.25 [233.037]	128.27 [172.253]	.345
Credit limit (Rp million)	13.56 [9.834]	13.15 [10.587]	13.13 [9.525]	14.20 [10.587]	13.87 [9.867]	13.44 [9.803]	.569
B2. Wave IV—Treatment Cell Size							
Wave IV	1,687	336	336	336	336	343	
Total	1,687	336	336	336	336	343	

TABLE 1 (Continued)

	TREATMENT						$\beta$ -VALUE (7)
	FULL SAMPLE (1)	Religious (2)	Moral Incentive Implicit (3)	Nonreligious (4)	Simple Reminder (5)	Control Group (6)	
	C1. Waves V and VI—Balance of Covariates						
Age	41.61 [9.722]	41.73 [10.093]	41.80 [9.481]	41.36 [9.639]	40.95 [9.954]	41.79 [9.562]	.557
Female	.39 [.488]	.41 [.492]	.37 [.483]	.40 [.489]	.43 [.496]	.36 [.481]	.087
Muslim	.90 [.306]	.88 [.326]	.90 [.295]	.92 [.279]	.88 [.321]	.89 [.314]	.087
Annual income (Rp million)	158.51 [966.064]	141.85 [219.339]	205.76 [1942.643]	159.15 [556.385]	160.08 [609.755]	131.45 [184.891]	.379
Credit limit (Rp million)	13.87 [10.257]	13.68 [10.037]	14.00 [10.530]	13.73 [10.143]	13.59 [9.967]	14.17 [10.441]	.786
	C2. Waves V and VI—Treatment Cell Size						
Wave V	2,106	546	482	488	0	590	
Wave VI	1,814	362	362	362	362	366	
Total	3,920	908	844	850	362	956	

NOTE.—Panel A1 reports summary statistics for the sample and presents a test of random assignment for waves I, II, and III. Col. 1 reports the mean level of each variable, with standard deviations in brackets, for the full sample. Cols. 2–6 report the mean level of each variable, with standard deviations in brackets, for all experimental treatment conditions. Col. 7 reports the  $\beta$ -value of a test for equality of means in all experimental conditions. Panel A2 reports treatment cell sizes by month. Panels B1 and B2 and C1 and C2 report the same statistics for waves IV and waves V and VI, respectively.

random assignment, the sample is well balanced across all baseline characteristics.<sup>23</sup>

In a second step, the bank shared data on credit card repayment for customers in our sample after each wave of the experiment, as well as historical repayment data covering the 12 months prior to our intervention. In the monthly repayment data, we observe a customer's delinquency status (whether the customer made the required monthly minimum payment by the end of the grace period), which is the main outcome of interest for our analysis. The bank also provided further financial data, including information on credit card usage and savings accounts for a subset of customers. In particular, we collected data on savings account balances for all customers in the first four waves of the main experiment who have an account with our partner bank.<sup>24</sup> The bank also provided us with data on credit card default, defined as failing to make the required minimum payment within 90 days of the due date.<sup>25</sup>

## 2. Survey Data

We combine data from the experiment with information from a number of phone surveys administered to the bank's credit card customers.<sup>26</sup> The main survey, conducted in June and July 2015, asked respondents about their level of religiosity and their familiarity with the quote used in the three variations of the moral incentive treatment condition. The same survey was also administered to a randomly drawn sample of the bank's credit card customers all over Indonesia who were not late on their payments during the sample period. We use the results from this survey to construct a measure of local religiosity for the regions in which credit card customers reside.

An additional survey was administered 1 day after the repayment deadline in April 2016 to a random sample of credit card customers who had participated in the experiment that month. The purpose of this survey was to test whether the moral appeal signals that the bank is particularly

<sup>23</sup> Our sample is also very similar to the universe of the bank's credit card customers along most observable dimensions. Late payers are only marginally more likely to be female (40 percent vs. 37 percent) and, on average, have a slightly lower credit limit (Rp 13.6 million vs. Rp 14.7 million).

<sup>24</sup> The bank's customers are not required to have a checking or savings account with the bank in order to obtain a credit card. The most common deposit account at our bank is a liquid savings (*tabungan*) account. At the time of the experiment, 30 customers had a checking account and 1,088 customers had a savings account at the bank.

<sup>25</sup> Data on savings accounts and longer-term repayment were available only up to August 2015. At the time the data were collected, default was thus realized only for customers in the first three waves of the main experiment, the third wave being implemented in May 2015.

<sup>26</sup> The survey instruments are available in the appendix.

committed to debt collection, whether receiving it causes any disutility to customers, and to measure whether the credit reputation message increases knowledge about the credit reporting system. Respondents in the survey sample had previously received either no treatment message, the basic reminder, or one of the versions of the moral message. The survey first asked these customers how committed they thought the bank was to collecting debts. Second, it asked whether they wished to receive text messages in the future similar to the one they had received a few days earlier. Third, customers were randomized in two groups: those in a treatment group were read the content of the reputational incentive message, while those in a control group were not given any information. All customers were then asked questions about the Indonesian credit registry and their beliefs about the consequences of nonrepayment.

In April 2017 we selected a random sample of credit card customers for a final survey. These were customers who had participated in the experiment in June 2015—the month in which the cash rebate treatment was conducted—but had not been offered the rebate. The purpose of this survey was to elicit customer preferences for an immediate deposit into their bank account relative to a delayed cash rebate on their next credit card statement (identical to how the cash rebate treatment was implemented) using a nonincentivized multiple price list procedure.<sup>27</sup>

### 3. Main Outcome of Interest

Our main outcome of interest is credit card delinquency. The bank considers a customer to be delinquent if she fails to make the required minimum payment by the end of the 10-day grace period, which occurs on the eighteenth day of the month. Accordingly, we measure delinquency as a dummy variable equal to one if a customer fails to make the required payment by the end of the grace period and zero otherwise. When customers become delinquent, the bank reports them to the Indonesian credit registry, their card is automatically blocked, their account is charged a late payment fee, and they may receive phone calls from the bank's collection department.

<sup>27</sup> The survey conducted in June and July 2015 was administered to 2,274 participants of our experiment and to 567 other randomly selected customers. The survey conducted in April 2016 was administered to 93 randomly selected participants of the experiment that month, stratified by treatment group. Finally, the survey conducted in April 2017 was administered to 98 customers who are similar along observables to the 336 customers who received the cash rebate. Response rates and initial sample sizes for these surveys are 43 percent and 5,233, 41 percent and 1,399, 20 percent and 460, and 25 percent and 400, respectively. In some of the surveys, response rates are correlated with observables (e.g., in the first survey women are less responsive than men). Response rates are, however, never correlated with treatment assignment.

### E. Estimation

Since treatment status was randomly assigned, our identification strategy is straightforward. We identify experimental treatment effects using regressions of the form

$$Y_i = \alpha + \sum_c \beta_c I_{c,i} + \gamma' \mathbf{X}_i + \epsilon_i, \quad (1)$$

where  $Y_i$  is an outcome of interest, such as an indicator for customer  $i$  being delinquent. The variables  $I_{c,i}$  are indicators for customer  $i$ , assigned to treatment condition  $c$ . In some specifications, we additionally include a vector of control variables,  $\mathbf{X}_i$ , which contains either month fixed effects only or month fixed effects as well as a set of customer and account characteristics. In all regressions, the omitted category is the control group, which received only a basic reminder on the due date but no second text message 2 days prior to the repayment deadline.<sup>28</sup>

The results reported in the regression tables are based on sampling-based inference. In the text, we also report the results of randomization-based inference, where we calculate Fisher exact  $p$ -values for the sharp null hypothesis of no effect. As sample statistics, we use the difference in means by treatment status. Given the large sample size, calculation of the sample statistic for all possible realizations of the treatment assignment mechanism is computationally not feasible. For this reason,  $p$ -values for our permutation tests are based on 10,000 iterations using random sampling with replacement from the universe of possible treatment assignments, while holding the probability of being treated constant. To compare the effect of different treatments to that of moral incentives, the text also reports 95 percent confidence intervals for the ratio between the effect of each treatment and moral incentives.

## III. Results

### A. Main Result: Moral Incentives

We first examine the effect of the moral message on delinquency. Table 2 shows treatment effect estimates for the moral incentive message across all waves of the experiment. In column 1, we begin by presenting results from a regression without controls, which represents raw delinquency rates. Compared to the control group, the share of delinquent customers decreases by 4.4 percentage points under the moral incentive treatment

<sup>28</sup> Since we do not observe whether customers open the messages they receive, all of our results should be interpreted as intent-to-treat estimates. Note, however, that all messages are sent from the bank, so that there is no reason to believe that customers are more or less likely to open messages associated with a specific treatment.

TABLE 2  
TREATMENT EFFECTS: MORAL INCENTIVES

	DUMMY FOR DELINQUENCY		
	(1)	(2)	(3)
Moral incentive	-.044*** [.013]	-.052*** [.013]	-.051*** [.013]
Delinquency rate control group		.66	
Month fixed effects	No	Yes	Yes
Controls	No	No	Yes
Sample	Full sample	Full sample	Full sample
Observations	6,364	13,428	13,428
R <sup>2</sup>	.002	.011	.057

NOTE.—Col. 1 restricts the sample to customers assigned to the moral incentive or control groups. Cols. 2 and 3 use the entire sample. Col. 1 reports results from an ordinary least squares (OLS) regression of a delinquency dummy on treatment indicators. The omitted category is the control group, for which we report the mean delinquency rate. Col. 2 adds month fixed effects. Col. 3 adds individual covariates (age, gender dummy, Muslim dummy, province dummy, income, a dummy for having been in the sample in a previous month, and a dummy for having been delinquent at least once in the previous 12 months). Robust standard errors are in brackets.

\* Significant at the 10 percent level.

\*\* Significant at the 5 percent level.

\*\*\* Significant at the 1 percent level.

condition. The difference in delinquency rates is significant at the 1 percent level ( $p$ -value  $< .001$ ). Using randomization-based inference, we also reject the null hypothesis that the moral incentive treatment had no effect (Fisher exact  $p$ -value  $< .001$ ). We add month fixed effects in column 2 and customer-level covariates in column 3. The results remain very similar across all specifications, indicating that the randomization was successful. Treatment effects range from  $-4.4$  percentage points to  $-5.2$  percentage points relative to a baseline delinquency rate of 66 percent in the control group. The treatment effect is similar for men and women and also does not differ by age, religion, or whether a customer has appeared on the list of late payers at least once in the 12 months before our intervention.<sup>29</sup> The effect is stronger for customers with a lower debt-to-income ratio, which suggests that the treatment response is moderated by financial constraints. We find that the same pattern also holds for the credit reputation treatment.

<sup>29</sup> Fewer than 10 percent of customers in our sample are non-Muslim, so that it is not possible to estimate this effect precisely. We discuss heterogeneity by local religiosity in Sec. IV.A.1. Heterogeneous treatment effects are reported in table A.3. Importantly, the fact that the effect is the same for customers who were delinquent in the 12 months prior to the intervention and customers who were not implies that customers who generally pay late and customers who generally pay on time are equally responsive to moral appeals and suggests that our estimates could potentially generalize to the population of customers who typically repay their card debt on time in the absence of any intervention.



*B. Benchmarking the Moral Incentive Effect*

## 1. Direct Financial Incentives: Cash Rebate

To assess the economic magnitude of the moral incentive effect, we conduct two benchmarking exercises, in which we compare the impact of the moral message against that of direct and indirect financial incentives. In our first benchmarking treatment, the bank sent text messages to a randomly chosen subset of customers and offered them a cash rebate in the form of a principal reduction equal to 50 percent of their outstanding minimum payment, conditional on making the required payment before the deadline. Customers assigned to this treatment were informed that this rebate would be credited to their account in the next billing cycle. The median rebate offered was Rp 380,000 (equal to 8 percent of monthly earnings for the median customer in our sample), and the average rebate offered was Rp 500,000. This treatment allows us to measure the impact of moral incentives in monetary terms and identify the amount of financial incentives the bank would need to provide to generate the same reduction in delinquency rates as the moral incentive message.

The results of the financial incentive treatment are reported in table 3. We do not find a statistically significant effect of financial incentives on

TABLE 3  
BENCHMARKING MORAL INCENTIVES: CASH REBATE

	DUMMY FOR DELINQUENCY		
	(1)	(2)	(3)
Moral incentive	-.054 [.036]	-.052*** [.013]	-.051*** [.013]
Cash rebate	-.021 [.035]	-.014 [.030]	-.003 [.029]
Delinquency rate control group	.70	.66	
Moral incentive – cash rebate	-.033 [.036]	-.038 [.030]	-.047 [.029]
Month fixed effects	No	Yes	Yes
Controls	No	No	Yes
Sample	Only wave IV	Full sample	Full sample
Observations	1,015	13,428	13,428
R <sup>2</sup>	.002	.011	.057

NOTE.—Col. 1 restricts the sample to customers late in June 2015 and assigned to either the moral incentive, financial incentive (run only in wave IV), or control groups. Cols. 2 and 3 use the entire sample. Col. 1 reports results from an OLS regression of a delinquency dummy on treatment indicators. The omitted category is the control group, for which we report the mean delinquency rate. Col. 2 adds month fixed effects. Col. 3 adds individual covariates (age, gender dummy, Muslim dummy, province dummy, income, a dummy for having been in the sample in a previous month, and a dummy for having been delinquent at least once in the previous 12 months). Moral incentive – cash rebate is the difference between the moral incentive and cash rebate coefficients. Robust standard errors are in brackets.

\* Significant at the 10 percent level.

\*\* Significant at the 5 percent level.

\*\*\* Significant at the 1 percent level.

repayment nor a significant difference between the effect of moral and financial incentives.<sup>30</sup> This is mainly due to the limited sample size, which was the result of the partner bank not wanting to incur the comparatively high cost of the rebate (Rp 540,000 for the average person who took up the offer, compared to practically no cost for the moral message). We can nonetheless use the results to obtain a conservative benchmark for the size of the moral incentive effect. While point estimates indicate moral incentives being more effective than financial incentives, we can use the confidence interval to conservatively bound the effect of moral incentives to be at least 84 percent of the effect of financial incentives. This suggests that moral incentives are at least as effective as a statement credit reward of Rp 317,726 or approximately 6 percent of the median monthly income of customers in our sample (1/1.196, or 84 percent of the median rebate offered).<sup>31</sup>

## 2. Indirect Financial Incentives: Credit Reputation

In a second benchmarking exercise, we compare the effect of the moral message to that of reputational incentives—another type of material incentive that has been shown to be important in similar contexts.<sup>32</sup> Our test uses a treatment in which the bank sent text messages that informed late-paying customers of the existence of the Indonesian credit registry and the consequences of being reported for nonrepayment. The message stated that all banks in Indonesia consult the credit registry before

<sup>30</sup> In the month when both treatments were run concurrently, point estimates indicate that the cash rebate treatment had an effect of  $-2.1$  percentage points compared to  $-5.4$  percentage points for moral incentives. The 95 percent confidence interval for the ratio between the coefficient of financial incentives and the coefficient of moral incentives is  $[-1.069; 1.196]$ .

<sup>31</sup> Because the cash rebate treatment is a conditional offer, credited to a customer's account in the next month, we conduct several exercises to rule out potential confounding factors. First, we ensure that borrowers are not confused about the content of the offer and timing of the rebate. We closely monitored the implementation and found no instances in which customers asked questions indicating that they had not understood the offer or contacted the bank with further questions or complaints about the offer. Second, we conduct an elicitation exercise (using a nonincentivized phone survey with past-due clients) to show that customers do not strongly discount statement credit relative to immediate cash deposits. This exercise indicates that, on average, customers value Rp 100 in statement credit next month the same as an immediate Rp 92 cash deposit into their account. Importantly, 79 percent of customers value statement credit exactly the same as immediate cash, and even the lowest decile values statement credit as much as an immediate Rp 90 cash deposit. At the same time, 6 percent of customers do not give any value to a principal reduction. This is consistent with these customers planning to default on their debt and completely discounting statement credit: default rates are indeed about 5 percent in our sample of late payers (below 0.5 percent in the entire customer population).

<sup>32</sup> See, e.g., Pagano and Jappelli (1993) and Brown and Zehnder (2007) for evidence on credit reporting and loan repayment and Liberman (2016) for evidence on the willingness to pay for a good credit reputation among credit card borrowers in an emerging market.

TABLE 4  
BENCHMARKING MORAL INCENTIVES: CREDIT REPUTATION

	DUMMY FOR DELINQUENCY		
	(1)	(2)	(3)
Moral incentive	-.060*** [.018]	-.052*** [.013]	-.051*** [.013]
Credit reputation	-.098*** [.014]	-.102*** [.014]	-.104*** [.013]
Delinquency rate control group		.66	
Moral incentive – credit reputation	.038** [.019]	.051** [.016]	.053*** [.016]
Month fixed effects	No	Yes	Yes
Controls	No	No	Yes
Sample	Waves I, II, and III	Full sample	Full sample
Observations	5,821	13,428	13,428
R <sup>2</sup>	.008	.011	.057

NOTE.—Col. 1 restricts the sample to customers late in February, March, or May 2015 and assigned to either the moral incentive, reputational incentive (this treatment was not run in waves IV, V, and VI), or control group. Cols. 2 and 3 use the whole sample. Col. 1 reports results from an OLS regression of a delinquency dummy on treatment indicators. The omitted category is the control group, for which we report the mean delinquency rate. Col. 2 adds month fixed effects. Col. 3 adds individual covariates (age, gender dummy, Muslim dummy, province dummy, income, a dummy for having been in the sample in a previous month, and a dummy for having been delinquent at least once in the previous 12 months). Moral incentive – credit reputation is the difference between the moral incentive and credit reputation coefficients. Robust standard errors are in brackets.

\* Significant at the 10 percent level.

\*\* Significant at the 5 percent level.

\*\*\* Significant at the 1 percent level.

issuing credit, so that a registry entry will prevent a customer from accessing formal credit in the future.<sup>33</sup> In addition to serving as a benchmark for the moral incentive effect, we can also use this treatment to examine the possibility that customers of an Islamic bank might be especially unresponsive to material incentives.

Table 4 reports the results. In terms of raw delinquency rates, reported in column 1, the credit reputation message decreases the probability of becoming delinquent by 9.8 percentage points, as compared to 6.0 percentage points for the moral incentive message over the same time period. We can use randomization-based inference and reject the sharp null hypotheses that reputational incentives had no effect (Fisher exact  $p$ -value  $< .001$ ) or that they had the same effect as the moral incentive message (Fisher exact  $p$ -value = .049). The results remain very similar in columns 2 and 3, where we add month fixed effects and covariates.<sup>34</sup> These results

<sup>33</sup> Survey evidence indicates that this treatment does not necessarily increase customers' knowledge of how the registry functions, but instead simply makes customers think that the consequences of being reported to the credit registry are severe.

<sup>34</sup> The 95 percent confidence interval for the ratio between the credit reputation and moral incentive coefficients is [1.041; 3.059].

indicate that customers in our sample do respond to the material incentives linked to their credit reputation. As above, we can calculate the 95 percent confidence interval for the ratio between the cash rebate coefficient and the credit reputation coefficient and use it to obtain a conservative benchmark for the size of the reputational incentive treatment effect. In this case, the confidence interval is  $[-0.524; 0.586]$ , implying the effect of reputational incentives to be at least 171 percent of the effect of financial incentives. This suggests that the bank would have to offer a statement credit reward of Rp 648,464, or approximately 13 percent of median monthly income, to obtain the same effect as the credit reputation message. If we assume that the credit reputation treatment moves beliefs about the probability of the existence of the credit registry from zero to one, we can interpret this number as the willingness to pay for a clean credit record.<sup>35</sup> Liberman (2016) shows that credit card borrowers in Chile are willing to pay 11 percent of their median monthly income to maintain a clean credit record, so that our results provide suggestive evidence that the customers in our setting are no less responsive to reputational incentives than customers of a regular bank in an emerging economy where debt repayment decisions have no religious association.<sup>36</sup>

<sup>35</sup> The assumption that probabilities shift from zero to one gives the most conservative estimate of the willingness to pay for a clean credit record; any other intermediate shift in beliefs would result in a higher estimate of the willingness to pay.

<sup>36</sup> Note that it is difficult to use the credit reputation treatment as a direct benchmark, since the treatment combines the effect of learning about the existence of the credit registry with the effect of being made aware of the consequences of nonrepayment. To gain a better understanding of how the credit reputation treatment affects customers' decisions, we conducted a small follow-up survey. In this survey, customers were randomized into two groups: customers in a treatment group were read the content of the reputational incentive message, while customers in a control group were not given any information. All participants were then asked some questions about the Indonesian credit registry. The results from the survey suggest that late-paying customers are poorly informed about how the credit registry works and that the reputational incentive message does not increase their knowledge of how the registry functions. Instead, the message seems to make customers believe that the consequences of being reported to the credit registry are more severe.

In an additional benchmarking exercise, we express the impact of the moral incentive message in terms of persuasion rates, as suggested by DellaVigna and Gentzkow (2010), which makes it possible to compare the impact of moral incentives to effect sizes from other studies that have used nonmonetary incentives. The persuasion rate of an intervention is defined as the change in behavior generated, scaled by exposure to the treatment and the population share left to be persuaded. Formally, this can be expressed as

$$f = 100 \times \frac{y_T - y_C}{e_T - e_C} \frac{1}{1 - y_0},$$

where  $e_i$  is the share of group  $i$  receiving the message,  $y_i$  is the share of group  $i$  adopting the behavior of interest, and  $y_0$  is the counterfactual share that would change behavior if there were no message. Using this approach, we show that the magnitude of the moral incentive effect (persuasion rate of approximately 7 percent) is comparable to the impact of other types of nonmonetary incentives documented in the literature. The full results are available in the appendix.

### C. *Ruling Out Other Mechanisms*

The results so far establish that receiving the moral message substantially decreases credit card delinquency. However, there are several mechanisms other than responsiveness to the moral appeal that could explain this effect. In this section, we present a number of tests to evaluate alternative channels and show which of these potential explanations can be ruled out.

#### 1. Reminding Customers

First, receiving a text message might increase repayment rates simply because it acts as a reminder, irrespective of whether the message contains a moral appeal or not (see, e.g., Karlan, Morten, and Zinman 2016). To address this possibility, we compare repayment in the moral incentive treatment group to repayment among customers assigned to the simple reminder placebo treatment, which consisted of a basic nonreligious reminder that made no reference to morality or religion and was sent at the same time as the moral message. The results, reported in table 5, show that receiving the simple reminder has no effect on repayment. The raw delinquency rate is 65 percent in the group receiving the basic reminder, compared to 66 percent in the control group. The  $p$ -value of the difference between the simple reminder and the control is .714 (Fisher exact  $p$ -value = .729), and the  $p$ -value of the difference between the simple reminder and the moral message is .013 (Fisher exact  $p$ -value = .015). We can therefore rule out that the moral message works simply because it reminds customers to repay their debt.<sup>37</sup>

#### 2. Priming Religion

Second, receiving a text message with religious content could affect the repayment decision through priming effects, which are also unrelated to moral suasion. The moral message might, for example, remind recipients of the religious connotation of the credit contract or evoke a religious frame of mind more generally. To rule out this possibility, we compare repayment in the moral incentive treatment group to repayment among customers who received the religious placebo message. The religious placebo message contains a quote from the Prophet that is taken from the same religious text as the quote used in the moral message but makes no reference to the Islamic doctrine on debt repayment while still reminding customers to repay their credit card debt. The results, reported in table 5, show that the religious placebo message has no effect on the repayment

<sup>37</sup> See table A.5 for results in which the simple reminder is used as the main comparison group.

TABLE 5  
RULING OUT OTHER CHANNELS

	DUMMY FOR DELINQUENCY		
	(1)	(2)	(3)
Moral incentive	-.060*** [.018]	-.052*** [.013]	-.051*** [.013]
Simple reminder	-.006 [.018]	-.023 [.015]	-.022 [.015]
Religious placebo	-.002 [.018]	-.006 [.017]	-.010 [.017]
Delinquency rate control group		.66	
Moral incentive – simple reminder	-.054** [.022]	-.029* [.017]	-.028* [.017]
Moral incentive – religious placebo	-.058*** [.022]	-.045** [.019]	-.041** [.019]
Month fixed effects	No	Yes	Yes
Controls	No	No	Yes
Sample	Waves I, II, and III	Full sample	Full sample
Observations	5,821	13,428	13,428
R <sup>2</sup>	.002	.011	.057

NOTE.—Col. 1 excludes customers late in June 2015, February 2016, and April 2016 and restricts the sample to customers assigned to the moral incentive, simple repayment reminder, religious placebo (not run simultaneously in waves IV, V, and VI), or control groups. Cols. 2 and 3 use the entire sample. Col. 1 reports results from an OLS regression of a delinquency dummy on treatment indicators. The omitted category is the control group, for which we report the mean delinquency rate. Col. 2 adds month fixed effects. Col. 3 adds individual covariates (age, gender dummy, Muslim dummy, province dummy, income, a dummy for having been in the sample in a previous month, and a dummy for having been delinquent at least once in the previous 12 months). Moral incentive – simple reminder is the difference between the moral incentive and simple reminder coefficients. Moral incentive – religious placebo is the difference between the moral incentive and religious placebo coefficients. Robust standard errors are in brackets.

\* Significant at the 10 percent level.

\*\* Significant at the 5 percent level.

\*\*\* Significant at the 1 percent level.

rate. The raw delinquency rate is 65 percent in the group receiving the religious reminder and thus is nearly identical to the delinquency rate in the control group. The  $p$ -value of the difference between the religious placebo and the control is .889 (Fisher exact  $p$ -value = .904), and the  $p$ -value of the difference between the religious placebo and the moral message is .007 (same as the Fisher exact  $p$ -value), indicating that the effect of the moral message is also not driven by priming religion.

### 3. Novelty of the Message

Third, customers may respond to the message not because of its moral content, but because it is novel or attention-grabbing. To test for this possibility, we consider delinquency rates under different text message treat-

ments that use new content. Note that several of the messages that were sent to credit card customers as part of the experiment—including the simple reminder, religious placebo, and financial reminder messages—were specifically designed for the study and had never previously been received by the bank’s customers. The fact that none of these messages had a statistically significant effect on repayment allows us to rule out that the effect of the moral message is explained by the novelty of the message. We can also rule out the possibility that receiving a message with religious content and a quote from the Prophet is particularly attention-grabbing, using the religious placebo treatment condition. The messages sent in this treatment use a quote from the Prophet that is taken from the same religious text. However, as we show above, this message has no effect on debt repayment.

#### 4. Signaling the Bank’s Commitment to Debt Collection

Finally, since customers had previously received a text message at the time of the due date, receiving a second message might be perceived as a signal that the bank is particularly committed to debt collection and could affect delinquency rates independent of the moral appeal. To address this possibility, the bank sent the placebo reminder message and one of the three variations of the moral message described above to customers that had not been previously treated. Another group of customers was randomly assigned to a control group and received no message. We conducted a phone survey with customers in both groups the day after the payment deadline and asked “How committed do you think [bank name] is to collect debt from delinquent customers on a scale from 1 to 5 (where 1 is not very committed, and 5 is very committed)?” The percentage of respondents answering 4 or 5 is 76 percent in the control group, 67 percent in the basic reminder group, and only 59 percent among customers that received a moral message (the  $p$ -value for the test of equality of all three coefficients is .302, and the  $p$ -value of the test of equality between respondents in the control group and the treatment group is .124). Hence, there is no evidence to suggest that receiving the moral message prompts repayment because it is perceived as a signal that the bank is particularly committed to enforcing outstanding debts.<sup>38</sup>

<sup>38</sup> We also obtained the repayment history of all clients in our sample from the partner bank and use this information to test whether the response to the moral incentive treatment differs, depending on whether a customer appears on the list of late payers for the first time or has been delinquent before. We find no evidence that this is the case. These findings also relate to those in Hallsworth et al. (2015), where reframing debt nonrepayment from an error of “omission” to an error of “commission” increased the repayment of tax debts. The paper also finds that the act of commission is associated with greater beliefs about punishment for



#### D. *Disutility from Receiving the Message*

While our results show that moral incentives are effective at getting customers to repay their credit card debt, it is unclear whether this comes at a utility cost to customers. To examine this possibility, the bank called back a subset of customers who had received either the simple reminder or one of the versions of the moral incentive message as part of a follow-up survey 1 business day after the messages were sent (messages were sent on Friday and surveys were conducted on the next Monday). These customers were asked the question “[Bank name] is sending reminder messages to its customers to help them make their payment on time. You received one of these messages last week. Would you like to receive the same message again in the future?” The share of customers responding that they would like to receive the message again was 80 percent, both for those who had received a placebo reminder and for those who had received one of the variations of the moral message. The fact that a large majority of customers would like to receive similar messages in the future suggests that receiving a moral appeal does not impose a disutility on the recipients. Moreover, receiving a message containing a moral appeal does not seem to create a differential disutility, compared to a simple reminder.

As an additional test, we examine whether sending a moral appeal negatively affects the bank by reducing card usage or transaction volumes (e.g., customers might be dissatisfied with the bank after receiving the message or may want to avoid receiving a similar message in the future). We find that this is not the case. In the 30-day window after the intervention, the average amount spent is Rp 1,130,299 for customers that received the moral message and Rp 1,186,966 for customers in the control group ( $p$ -value = .535). The probabilities of card usage during this time period are .434 and .450, respectively ( $p$ -value = .265).<sup>39</sup>

### IV. Interpreting the Results

#### A. *What Drives the Moral Appeal?*

The evidence in the previous section rules out several mechanisms that are unrelated to moral suasion but could generate higher repayment rates in response to the moral message. We next explore competing hypotheses that might explain why the moral appeal is effective and present tests to distinguish between these alternative explanations.

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nonrepayment, which is the authors’ preferred interpretation for the results. In our setting, since there are no changes in beliefs about punishment from the bank, the findings indicate an association of commission with greater moral costs. This suggests that the mechanism of moral penalties might also be at play, in addition to the main channel proposed in that paper.

<sup>39</sup> These results also hold for different time windows after receiving the message.

### 1. Religious Connotation of the Message?

The first possibility is that the moral message reduces delinquency only because it is delivered in a religious context. This seems plausible, since the original moral incentive message explicitly quotes the Prophet and cites an important religious text as its source. Moreover, the original moral incentive message used a word for “injustice” that is of Arabic origin and often used in a religious context, so that the moral appeal could be associated with religion even if explicit references to its source are removed.

In order to distinguish the effect of the religious connotation of the moral message from that of the moral appeal, the bank sent two additional variations of the moral message to a randomly chosen subset of credit card customers. The first message was identical to the main treatment but omitted the reference to the Prophet and the source of the quote. The second variation of the moral message omitted the reference to the Prophet and the source of the quote and additionally replaced the Arabic-origin word for “injustice” with the standard Indonesian word, which has no religious connotation. Hence, the first message tests whether adding a credible religious source adds power to the impact of a moral appeal. The second message tests whether receiving a simple moral appeal without any religious connotation can affect the repayment decision.

The results are reported in table 6. The three versions of the moral message had nearly identical effects on raw delinquency in the months in which they were sent (4.1 percentage points for the religious moral message and 3.9 percentage points for the other two versions). Fisher exact *p*-values for the null hypothesis of no effect against the religious moral message are .938 for the nonreligious moral message and .967 for the implicit moral message (while the Fisher exact *p*-value against the control group is .039 in both cases).<sup>40</sup> This could indicate that either customers already associated the moral appeal contained in the message with religion or were able to identify it as a saying of the Prophet or that the pure moral statement was sufficient to increase repayment. To disentangle these competing hypotheses, we conducted a follow-up phone survey with a random sample of credit card customers. In this phone survey, the message with the standard Indonesian word for “injustice” and without reference to the Prophet was read to participants of the experiment, who were then asked to indicate its source.<sup>41</sup> The vast majority of clients were not immediately aware of the religious origin of the message. When asked “Who do you think might have said this phrase?” out of five given options, 76 percent

<sup>40</sup> The 95 percent confidence intervals for the ratio between the coefficient of the non-religious moral incentive and the implicit moral incentive to the effect of the religious moral message are [0.109; 1.400] and [0.111; 1.435], respectively.

<sup>41</sup> None of the customers in this sample had previously received any of the moral incentive text messages.

TABLE 6  
WHAT DRIVES THE MORAL APPEAL? RELIGIOUS CONNOTATION

	DUMMY FOR DELINQUENCY		
	(1)	(2)	(3)
Moral incentive	-.041** [.019]	-.051*** [.013]	-.051*** [.013]
Implicit moral incentive	-.039** [.019]	-.041** [.018]	-.039** [.018]
Nonreligious moral incentive	-.039** [.019]	-.040** [.018]	-.038** [.017]
Delinquency rate control group	.68		.66
Moral incentive – implicit moral incentive	-.001 [.019]	-.011 [.018]	-.011 [.018]
Moral incentive – nonreligious moral incentive	-.002 [.020]	-.011 [.018]	-.012 [.018]
Month fixed effects	No	Yes	Yes
Controls	No	No	Yes
Sample	Waves IV, V, and VI	Full sample	Full sample
Observations	4,909	13,428	13,428
R <sup>2</sup>	.001	.011	.057

NOTE.—Col. 1 restricts the sample to customers late in June 2015, February 2016, or April 2016 and assigned to the moral incentive, implicit moral incentive, nonreligious moral incentive (the last two treatments were run only in waves IV, V, and VI), or control groups. Cols. 2 and 3 use the whole sample. Col. 1 reports results from an OLS regression of a delinquency dummy on treatment indicators. The omitted category is the control group, for which we report the mean delinquency rate. Col. 2 adds month fixed effects. Col. 3 adds individual covariates (age, gender dummy, Muslim dummy, province dummy, income, a dummy for having been in the sample in a previous month, and a dummy for having been delinquent at least once in the previous 12 months). Moral incentive – implicit moral incentive is the difference between the moral incentive and implicit moral incentive coefficients. Moral incentive – nonreligious moral incentive is the difference between the moral incentive and nonreligious moral incentive coefficients. Robust standard errors are in brackets.

\* Significant at the 10 percent level.

\*\* Significant at the 5 percent level.

\*\*\* Significant at the 1 percent level.

chose “I don’t know,” whereas only 20 percent associated the phrase with religious figures or institutions (including the bank itself). These findings suggest that the reduction in delinquency is not the result of an implicit association of the statement contained in the moral message with religion. These results also corroborate the view that our sample is relatively secular: most clients did not recognize the statement as the Islamic doctrine on nonrepayment of debts.

The follow-up survey also helps us further clarify the role of religiosity in explaining our results. In the survey, respondents were asked about the importance of religion and the rules of Islamic law in their life, using a 1–5 Likert scale. Additionally, the survey asked customers to rank the relative importance of family, work, friends, and religion. Because of the relatively small sample size of the survey, we cannot directly use this measure

to assess the individual-level heterogeneity of treatment effects.<sup>42</sup> Instead, we use the data to construct a province-level measure of religiosity. To do so, we split the sample according to the share of respondents who identified as very religious in each province and compare treatment effects for customers in locations classified as more or less religious according to this measure.<sup>43</sup> For provinces below the median in terms of religiosity, receiving some version of the moral message reduced delinquency by 4.6 percentage points ( $p$ -value = .001). For provinces in the top half in terms of religiosity, receiving the moral message lowered the likelihood of becoming delinquent by an additional 1.1 percentage point, but the effect is not significantly higher than in less religious provinces. The  $p$ -value of the interaction between the moral message and a dummy for local religiosity above the median is .596. Taken together, these findings indicate that our main effects are driven by the response to the moral appeal rather than the religious nature of the message, although it is of course possible that the religious context of our experiment enhances the responsiveness of clients to these moral appeals.

## 2. Provision of New Information? The Impact of Repeated Messages

We next explore whether the moral message works only the first time it is sent—for example, because it conveys new information—or if it continues to work when the message is sent to customers who have received it before. To address this question, we conducted a follow-up experiment with a sample of customers who had already received the moral message once and reappeared on the list of late payers. In February and April 2016, customers in this group were sent either the same version of the moral message that they had previously received for a second time, with a lag of 2 months or approximately 1 year, or were assigned to a control group that received no additional message.

Table 7 reports the results, pooling across the different versions of the moral message. We find suggestive evidence that repeated moral messages still affect repayment and that the size of the effect is not lower among customers who receive the moral message for a second time. In the specification without individual covariates and month fixed effects, reported in

<sup>42</sup> This survey was administered to 2,841 customers: among them, 2,274 participants of our experiment and 567 randomly selected customers of the bank that did not participate in the experiment.

<sup>43</sup> We identify customers as very religious if they answered “extremely important” to survey questions that asked them about the importance of religion and the rules of Islamic law in their life and if they ranked religion as the most important aspect of their life among all choices given.

TABLE 7  
WHAT DRIVES THE MORAL APPEAL? REPEATED MORAL MESSAGES

	DUMMY FOR DELINQUENCY			
	(1)	(2)	(3)	(4)
Repeated moral incentive	-.041 [.030]	-.041 [.030]	-.036 [.031]	-.043* [.025]
First moral incentive				-.045*** [.011]
Delinquency rate control group		.72		.67
Repeated moral incentive – first moral incentive				.001 [.026]
Month fixed effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Sample		Waves V and VI		Full sample
Observations	898	898	898	14,326
R <sup>2</sup>	.002	.006	.071	.056

NOTE.—Cols. 1–3 restrict the sample to customers that were part of the follow-up experiment. These customers had been late in February 2016 or April 2016 and had received a moral message in a previous wave of the experiment. Col. 4 uses the entire sample, consisting of the main and follow-up experiments. Col. 1 reports results from an OLS regression of a delinquency dummy on treatment indicators. The omitted category is the control group, for which we report the mean delinquency rate. Col. 2 adds month fixed effects. Col. 3 adds individual covariates (age, gender dummy, Muslim dummy, province dummy, income, a dummy for having been in the sample in a previous month, and a dummy for having been delinquent at least once in the previous 12 months). Col. 4 adds a dummy for having received one of the three versions of the moral message for the first time and additional treatment group dummies. Repeated moral incentive – first moral incentive is the difference between the repeated moral incentive and first moral incentive coefficients. Robust standard errors are in brackets.

\* Significant at the 10 percent level.

\*\* Significant at the 5 percent level.

\*\*\* Significant at the 1 percent level.

table 7, column 1, the effect of the repeated moral message is 4.1 percentage points ( $p$ -value = .175, Fisher exact  $p$ -value = .163).<sup>44</sup> We then compare the effects of the first and the second moral messages. In order to do so, we pool the sample from the repeated message experiment with the data from the main experiment. This requires some caution, since there are likely to be selection issues. In particular, customers who show up on the list of late payers for a second time are likely to differ from those who appear on the list for the first time. Indeed, we find that while the two samples are well balanced on demographics, customers in the repeated message sample have lower income and credit limits and are more likely

<sup>44</sup> There is suggestive evidence that the effects do not vary depending on the time lag between the first and the repeated message. Sending the moral message to customers who received the same message 1 year earlier increases repayment by 4.0 percentage points compared to sending no message ( $p$ -value = .323). Sending a moral message to customers who received the same message 2 months before increases repayment by 4.4 percentage points compared to sending no message ( $p$ -value = .346). However, the sample sizes are too small to estimate effects separately by time since the first message.

to have been delinquent at least once in the previous year.<sup>45</sup> We therefore include individual covariates to address this potential selection problem.<sup>46</sup> The results are reported in table 7, column 4. The point estimate of receiving the moral message for the first time is 4.5 percentage points. With a point estimate of 4.3 percentage points, the effect of receiving the moral message for a second time is nearly identical, and both effects are statistically significant (at the 1 percent and 10 percent level, respectively). The *p*-value of a test of equality of the two effects is .955, and the 95 percent confidence interval of the ratio between the effect of receiving the message for the second time and the effect of receiving the message for the first time is  $[-0.156; 2.092]$ .<sup>47</sup>

The result that the moral message affects repayment even when it is sent repeatedly rules out the possibility that the message affects repayment by conveying new information. In addition, the finding that even a moral message with no reference to religion affects repayments indicates that the effect is not driven by the recipients learning about a religious teaching that they were not previously aware of. Similarly, the effect cannot be explained by the customer learning that nonrepayment of debts can be considered immoral. In both cases, the message would affect repayment only when this information is conveyed for the first time. While we cannot test for this mechanism directly, our results are consistent with the interpretation that customers care about the morality of repaying their debt and that the moral message temporarily draws attention to the moral aspect of the repayment decision.

## *B. Additional Results and Extensions*

### 1. Impact on Credit Card Default

In this section, we examine the effect of the moral message on default, defined as failing to make a payment within 90 days from the due date. Since the financial product we consider is a revolving line of credit, cus-

<sup>45</sup> See table A.6 for details.

<sup>46</sup> Another possible concern is the presence of differential selection due to the treatment. However, we do not find any evidence of this type of selection: the proportion of customers showing up on the list of late payers a second time in 2016 after having appeared in the sample of our main experiment in 2015 is 0.237 among those receiving a moral message and 0.239 among controls (*p*-value of the difference .865). So the fact of being late again after a few months from our intervention is likely due to some negative shock independent of treatment status (possibly a negative income or liquidity shock, which is in line with these customers having a lower income and credit limit and being more likely to have been more than 30 days past due in the past).

<sup>47</sup> Intuitively, this exercise compares the effect size of a message sent to customers who have never seen the message before and are late for a first time to the effect size in the selected sample of customers who have seen the message before and are late for a second time. While both estimates can be interpreted causally, we cannot causally evaluate the effect of repeated messages on the nonselected sample since no further messages are sent to customers who are not late a second time.

tomers face strong repayment incentives. It is therefore not surprising that outright default is a rare event and is much less common than delinquency: in our sample of late payers, only 5 percent of customers eventually end up defaulting on their credit card debt.<sup>48</sup> Columns 7 and 8 of table 8 show that the moral incentive message does not reduce this already very low default rate when we consider the entire population of experimental participants. However, there is substantial variation in the ex ante credit risk of customers in this sample, and we find that the moral message is extremely effective at reducing default among the customers with the highest ex ante credit risk. To calculate a customer's ex ante credit risk, we estimate a linear model of default probabilities for customers in the control group. More specifically, we run a linear regression of a dummy variable for whether a customer defaulted on month fixed effects and a set of individual-level covariates. We then use the model to predict the credit risk for each customer and split the sample into groups according to the predicted probability of default. Columns 9 and 10 of table 8 restrict the sample to the 10 percent of customers with the highest predicted credit risk.<sup>49</sup> In column 9 we look at raw default rates and find that the moral incentive message decreases the probability of default by 10.5 percentage points from a baseline default rate of 13 percent (Fisher exact  $p$ -value = .008). Reputational incentives are also effective in reducing default among high-risk customers: informing customers about the credit registry decreases the probability of default by 7.7 percentage points (Fisher exact  $p$ -value = .003). The results remain similar in column 10, where we add month fixed effects and covariates.<sup>50</sup>

<sup>48</sup> In the overall population of credit card customers, default rates are below 0.5 percent. Information about default is available only for customers in the first three waves of our experiment.

<sup>49</sup> Note that this sample split was decided on ex post rather than as part of the original research design. To corroborate our findings, we provide results for alternative credit risk thresholds in the appendix.

<sup>50</sup> We find that moral and reputational incentives also decrease delinquency by 13.7 and 18.6 percentage points, respectively, in this high-risk group (see cols. 3 and 4 of table 8): this implies that the messages reduce default by increasing immediate repayment by the end of the grace period, i.e., during the time period when we have full experimental control, and not by changing behavior later, when the treatments could potentially interact with external factors. Table 8 reports additional results on both delinquency and default across all treatments available, including for customers with low credit risk. In table A.7, we report results for different credit risk cutoffs and show that the moral message leads to economically meaningful and statistically significant reductions for various alternative subsamples of customers with above-median credit risk. When we expand the sample to the 25 percent of customers with the highest ex ante credit risk, we find that the moral message reduces default by 4.3 percentage points (from a baseline default rate of 11 percent). In the sample of customers with above-median ex ante credit risk, we still find a marginally significant reduction in defaults of 2.2 percentage points (from a baseline default rate of 8 percent). To further verify these results, we also predict the ex ante credit risk of customers in our sample using two different machine learning algorithms. The results of these exercises are reported in tables A.8 and A.9.



TABLE 8  
EFFECTS ON CREDIT CARD DEFAULT

	DUMMY FOR DELINQUENCY				DUMMY FOR DEFAULT							
	Full Sample	High Credit Risk	Low Credit Risk		Full Sample	High Credit Risk	Low Credit Risk					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Moral incentive	-.080*** [.020]	-.087*** [.020]	-.137** [.061]	-.167*** [.061]	-.074*** [.022]	-.080*** [.021]	-.004 [.008]	-.003 [.008]	-.105*** [.027]	-.106*** [.029]	.008 [.009]	.008 [.009]
Credit reputation	-.118*** [.017]	-.127*** [.017]	-.186*** [.052]	-.192*** [.052]	-.111*** [.018]	-.121*** [.018]	-.002 [.007]	-.002 [.007]	-.077*** [.028]	-.087*** [.029]	.006 [.007]	.006 [.007]
Simple reminder	-.026 [.020]	-.034* [.020]	-.081 [.061]	-.082 [.060]	-.020 [.021]	-.029 [.021]	.010 [.009]	.011 [.009]	-.011 [.040]	-.017 [.041]	.013 [.009]	.013 [.009]
Religious placebo	-.022 [.020]	-.032 [.020]	-.010 [.057]	-.019 [.055]	-.025 [.021]	-.035* [.021]	-.013 [.009]	-.013 [.009]	-.005 [.040]	-.003 [.042]	.013 [.009]	.013 [.009]
Delinquency rate control group				.74		.67						
Default rate control group							.05					.05
Month fixed effects	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Sample	6,979	6,979	717	717	6,262	6,262	6,979	6,979	717	717	6,262	6,262
R <sup>2</sup>	.010	.080	.028	.121	.009	.078	.001	.016	.021	.057	.001	.014

NOTE.—Cols. 1–2 and 7–8 restrict the sample to customers late in February, March, and May 2015, which is the sample of customers for whom information on default is available. Using customers in the control group, we estimate the probability of default based on an OLS estimation that regresses a dummy for credit card default on month fixed effects and individual covariates (age, gender dummy, Muslim dummy, province dummy, income, a dummy for having been in the sample in a previous month, and a dummy for having been delinquent at least once in the previous 12 months). We use this model to predict the probability of default for each customer and split the sample into two groups: the 10 percent of customers with the highest predicted probability of default (high credit risk) and the remaining 90 percent of customers (low credit risk). Cols. 3–4 and 9–10 restrict the sample to high-credit risk customers, while cols. 5–6 and 11–12 restrict the sample to low-credit risk customers. Cols. 1, 3, and 5 report results from OLS regressions of a delinquency dummy on treatment indicators. Cols. 2, 4, and 6 add month fixed effects and individual covariates. Cols. 7, 9, and 11 report results from OLS regressions of a dummy for credit card default on treatment group indicators. Cols. 8, 10, and 12 add month fixed effects and individual covariates. The omitted category in all regressions is the control group, for which we report mean delinquency and default rates. Robust standard errors are in brackets.

\* Significant at the 10 percent level.

\*\* Significant at the 5 percent level.

\*\*\* Significant at the 1 percent level.

## 2. Impact on the Intensive Margin of Repayment

We can further unpack the mechanism through which moral incentives affect behavior by examining the intensive margin of repayment, that is, the amount repaid conditional on making the minimum payment. Since each of our treatments may induce a different subset of consumers to repay, looking at the intensive margin of repayment in isolation induces selection problems. Specifically, because customers with a lower average willingness to repay might make a payment if they were included in one of the moral incentive treatment groups, a simple comparison between treatment and control groups would most likely understate the intensive margin effect.

To avoid this selection problem, we impute zeros for all customers who did not make a payment and analyze the combined effect of our treatments on the intensive and extensive margins. These are unconditional means and therefore are not subject to selection. Following this approach, we find that the average amount repaid in the control group is Rp 637,819. The average repayment in the moral incentive group is slightly higher than in the reputational incentive group at Rp 745,352 versus Rp 713,437 ( $p$ -value = .654).<sup>51</sup> Moreover, the share of customers that repay substantially more than (more than twice) the amount required to avoid being reported to the credit registry is significantly higher in the moral incentive group than in the reputational incentive group (23 percent vs. 19 percent,  $p$ -value = .096). This result suggests that customers in the reputational incentive treatment act more strategically in response to the message and are more likely to repay only the required 10 percent of their outstanding balance. In contrast, customers receiving the moral message tend to repay more than the amount needed to avoid being reported to the credit registry.

## 3. Impact on Savings Account Balances

To better understand how customers make payments in response to the experimental treatments, we also examine the effect of repayment on savings account balances. For this purpose, we obtained detailed data on savings account balances for participants of our experiment from our partner bank. We have access to customers' daily balances on their *tabungan* (Indonesian for "savings") accounts. These are the most common types of deposit accounts among clients of our partner bank and have all characteristics of a standard liquid savings account. Since credit card custom-

<sup>51</sup> For these comparisons, we restrict the sample to customers late in February, March, and May 2015 since these are the only months when the reputational incentive message was sent. If we consider the whole sample, the average amount repaid in the control group is Rp 615,835 and in the moral incentive group is Rp 725,169.

ers are not required to also have another account with the bank, savings account balances are available for only 13 percent of customers in our sample, which may give rise to selection issues.

We find that making the minimum payment increases the likelihood of a savings account balance reduction, suggesting that customers are using their savings account balances to repay more expensive credit card debt. Among those who made the minimum payment in response to receiving one of our messages, 22 percent saw a reduction in their savings account balance between the sixteenth and eighteenth days of the month. Among those who did not repay, only 8 percent saw a reduction of their savings account balance over the same time period. The difference is significant at the 1 percent level ( $p$ -value  $< .001$ ). However, we do not have sufficient statistical power to detect differences in savings balances across the different treatment arms of our intervention, so that we consider this evidence as merely suggestive.

## V. Conclusion

In this paper, we provide evidence that nonpecuniary moral incentives affect debt repayment. In our setting, moral appeals are more effective than substantial monetary incentives as a means to encourage debt repayment at different time horizons. We find that the impact of our intervention on behavior is driven by responses to a moral appeal, rather than its religious connotation, and use a number of placebo treatments to rule out competing explanations, such as reminder effects, novelty of the message, priming religion, signaling the lender's commitment to debt collection, and the provision of new information.

An important feature of our experiment is that we are able to shed light on private individual motivations, as opposed to social image concerns, as drivers of moral behavior. While it is of course difficult to fully rule out the presence of social factors in the repayment decision, we provide evidence that individuals respond strongly to messages directed at their sense of morality, even in a setting in which a moral appeal is made in private, so that peer effects and threats of social shaming that are present in many similar environments (see, e.g., DellaVigna, Malmendier, and List 2012; Perez-Truglia and Troiano 2018) are largely absent. We show that it is possible to activate this individual sense of morality in economic transactions without threats of punishment or references to the negative consequences of nonrepayment and that this has economically large effects on debt repayment. In addition, we show that the effect of moral appeals in our experiment is not reliant on an explicit association with religion or another moral authority: we find that moral appeals are effective even when any religious connotation is removed and the message simply states that nonrepayment of debts violates a moral norm.

While our experiment is set in the context of Islamic banking to obtain an environment in which moral appeals are natural and credible, we believe we can derive more general lessons from our findings. Indonesia is a large emerging market economy, in which the product we study is marketed to a relatively secular customer population and widely used across many segments of society. This is reflected in the characteristics of our sample: a customer survey indicates that more than half of the customers at our partner bank have at least one other credit card from a non-Islamic bank, the vast majority of respondents were not aware of the religious origin of the quote used in our messages, and the share of non-Muslims in our sample is very similar to that in the Indonesian population. Moreover, we show that moral appeals are also effective in less religious regions and that their effect does not rely on an explicit reference to religion. When we restrict the sample to customers in Jakarta, who are much less likely to consider themselves religious, we find effects of a magnitude similar to that of the rest of the sample. We also test the responsiveness of customers in our sample to monetary incentives using the credit reputation message and find that the magnitude of their response to this type of incentive is very much in line with findings from credit card customers in other emerging economies (see Liberman 2016).

Overall, our findings are consistent with the interpretation that people experience a utility cost from consciously violating a moral norm, even if the act of noncompliance is not observable to others. While this perceived cost may be higher among religious respondents, our results suggest that moral considerations in economic transactions are a more general phenomenon that is likely to extrapolate to populations outside our setting. This provides a partial rationale for the widespread commercial use of moral appeals that highlight a moral norm but make no reference to the negative consequences of noncompliance in many nonreligious settings, such as energy conservation, recycling, and loan repayment.

The presence of moral considerations in economic transactions also has important implications for market efficiency, as we show in the appendix.<sup>52</sup> Introducing a moral disutility from not repaying one's debt into models of credit provision with adverse selection alleviates the lemons problem, since borrowers with a low ability to repay are more likely to experience a disutility from nonrepayment. Moral considerations may additionally alleviate moral hazard in credit markets by making debtors less willing to default. In fact, a theoretical literature on general equilibrium models of default assumes that individuals experience a disutility from default to obtain the existence of a competitive equilibrium with trade

<sup>52</sup> In other contexts, moral considerations can determine the actual existence of markets in which transactions are considered repugnant even if the parties directly involved benefit from that trade (see Elias, Lacetera, and Macis 2016).

(see, e.g., Dubey, Geanakoplos, and Shubik 2005). In these models, efficiency is typically the highest for intermediate costs of debt nonrepayment.

The relative importance of monetary and nonmonetary considerations in economic decisions is of course context dependent. Studying how moral incentives operate in other settings is therefore an important avenue for future research.

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