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# Charitable Giving by the Poor A Field Experiment in Kyrgyzstan

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# Charitable giving by the poor
# A field experiment in Kyrgyzstan

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Abstract

In a large-scale natural field experiment, we partnered with a micro-lending company in Kyrgyzstan that asked over 180,000 of its clients for donations to social projects as a form of corporate philanthropy. In a 2x2 design, we explored two main (pre-registered) hypotheses about giving by the poor. First, based on a conjecture that poor are more price sensitive than the rich and in contrast to previous studies, we hypothesize that matching incentives induce crowding in of out-of-pocket donations. Second, we hypothesize that our population cares about their proximity to the charitable project. We find evidence in favor of the former hypothesis but not the latter.

Previous studies of charitable giving focus on middle- or high-income earners in Western countries, neglecting the poor, although the lowest income groups are often shown to contribute substantial shares of their income to charitable causes. Our results challenge the evidence in the extant literature but are in line with our theoretical model. The implications for fundraising managers are that the optimal design of fundraising campaigns crucially depends on the targeted groups, and that donation matching is successful in stimulating participation in poorer populations.

JEL classifications: C93, D64, D12

Keywords: Charitable giving, field experiments, matching donations

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# 1. Introduction

Most studies on charitable giving focus on middle-class individuals in Western countries (see, e.g., DellaVigna, List, and Malmendier 2012; List and Lucking-Reiley 2002; Andreoni, Rao, and Trachtman 2017; Landry et al. 2010; Altmann et al. 2018). By contrast, giving by poorer segments of the population and in developing countries has not been studied extensively. Yet, studying giving by the poor in more detail is important for at least three reasons: (i) Giving by the poor provides a natural test arena to examine the robustness of some of the common fundamental findings on giving behavior that have been established in the literature. (ii) Giving by the poor matters economically because they tend to donate substantial fractions of their income to charity (Andreoni 2006a). (iii) Participation of recipients in the financing and/or production of donor-provided goods is recognized as an important tool to (re-)build social capital (Grieco, Braga, and Bripi 2016), to screen recipients with the highest marginal return (Sadoff and Samek 2019), and has been increasingly and successfully used in the developing world (Mansuri and Rao 2012). Giving by the poor might be different from giving from the rich due to many factors, including higher price elasticity, differing preferences regarding local public goods, and others. These differences can imply the suboptimality of naively imported policies that do not take into account that in developing countries charitable behavior might profoundly differ from giving behavior in WEIRD countries,

In this paper, we focus on two hypotheses about giving behavior among the poor. First, we test the robustness of the one of the key common fundamental findings that have been established in the charitable giving literature based on middle-income or rich samples: that matching of donation leads to lower revenues due to the crowding out of large donations (see, e.g., Huck, Rasul, and Shephard 2015). In contrast to the literature, we hypothesize that a treatment with matching incentives will increase out-of-pocket donations, because we expect the poor to have a higher price elasticity for the charitable good. In many samples studied so far, donors demand more of a charitable good when prices fall, but they spend less on it, in line with a price elasticity above -1. We illustrate how this pattern is linked to properties of donors' preferences and income. Specifically, we show in a simple yet general model that, on the extensive margin, crowding in of additional small donations merely requires that for very low levels of own consumption, the marginal utility of own consumption is large enough relative to the marginal utility of the charitable good. This requirement generates corner solutions in the absence of matching where the agent does not donate. When the price of giving falls sufficiently, interior solutions arise and we observe crowding in. On the other hand, on the intensive margin, crowding out of large donations requires a sufficient degree of complementarity between own consumption and the charitable good. We argue such complementarity is more likely to be prevalent for relatively high levels of own consumption that are only obtained if agents are fairly rich. (The happier I am with my own consumption, the more joy I might obtain on the margin if others' consumption increases.) Hence, we hypothesize that when we introduce matching in a relatively poor population of donors, we should find an overall positive effect on giving—with no crowding out on the intensive margin and crowding in of small donations on the extensive margin consistent with a higher price elasticity of the charitable good among the poor.[1] We study this hypothesis by comparing a treatment with matching incentives to a treatment with an unconditional lead gift chosen to generate a similar positive signal about the quality of the project (Vesterlund 2003)—in line with the recent literature.

Our second hypothesis deals with the role of distance between the donors and the beneficiaries of the charitable good. The experimental literature documents that social distance affects generosity (see, e.g., the seminal study on dictator games by Hoffman et al. 1994), however results of field studies are inconclusive.[2] We hypothesized that, in our population, we would observe a preference for geographically nearer projects for at least two reasons: First, in line with findings in Whillans, Caruso, and Dunn's (2017), community might matter more for the poor. Second, our donors may be more likely to also benefit from the charitable projects if they are geographically near.[3]

To test our two hypotheses, we conducted a large-scale field experiment in Kyrgyzstan with over 180,000 customers of a microfinance company. We pre-registered our study, including the two main hypotheses regarding the effects of matching and of local benefits. The customers of the microfinance company represent the poorer segment of the population of Kyrgyzstan, because the middle class has access to banks offering cheaper loans. The campaign lasted for two months and collected donations for infrastructure projects relating to water supply, health, and education in nine different localities in Kyrgyzstan. All projects were implemented by a single, newly

[1] This is in line with the literature that finds higher price elasticity among the poor with respect to, for example, health and education (Holla and Kremer 2009; Spears 2014).

[2] Some studies in the charitable giving and fundraising literature find an effect of geographic proximity on charitable giving for middle-income donors (Genç, Knowles, and Sullivan 2021; Grimson, Knowles, and Stahlmann-Brown 2020); others find donors are largely unaffected by geographic proximity (Brown, Meer, and Williams 2017; Meer 2014; Adena and Harke 2021).

[3] Note that if such benefits exist, the fundraising mechanism that we study could assume the flavor of a public good game. However, it would not change any part of our analysis.

established Kyrgyz charity. We implemented a 2x2 design. In one dimension, we either had a lead donor who pledged an unconditional lead gift or a lead donor who offered one-to-one matching of donations. The latter was capped at an amount equal to the unconditional lead gift, thereby holding the signaling value of the lead gift constant and only varying the price of giving as in Huck and Rasul (2010). In the second dimension, we varied the impact of the current donations on the location of future projects by announcing the next project of the charity would be implemented in the region with the highest donation per client during the current campaign.[4]

We find that, compared with the simple announcement of a lead gift, matching does increase the return from our campaign by 39%. This increase is driven by a substantial effect on the extensive margin in the order of 42% and the absence of any crowding out for larger donations. All in all, we provide strong evidence in favor of our first hypothesis. Although we cannot directly measure parameters of donor utility functions, we can estimate the average price elasticity of demand for the charitable good and compare it with previous studies. We find a substantially higher price elasticity of giving than previous studies based on Western and richer samples. Thus, we find that simple linear matching improves the effectiveness of fundraising relative to the mere presence of a lead donor who pledges an unconditional gift. As discussed above, this finding is in contrast to fundraising among donors in middle- and higher-income countries. Thus, the central insight of our study is that matching-induced crowding out is not universal. Although we cannot claim a causal relationship between income and crowding out, our leading explanation for this difference is that the presence of crowding out depends on the income distribution of potential donors. We provide further corroboration for this explanation by exploring heterogenous treatment effects within our sample where we find additional support for the explanation. By contrast, crowding in on the extensive margin does appear to be a universal phenomenon.[5]

In contrast to the affirmative results for our first hypothesis, we find no strong support for our second hypothesis. The treatment in which donations increase the probability of a future project being implemented locally has no significant effect on total giving in our dataset.[6] We also find no correlation between donations and the spatial distance between donors and the current projects. We

[4] Note our second variation includes a competitive component. However, this competitive component should not affect charitable behavior in the absence of preferences for local charitable output.

[5] The exception might be an extreme case, in which, in the baseline, all participants donate.

[6] We observe a significant effect of the local treatment on the amount given, conditional on donation, in one of the specifications, but restrain from interpreting this result as strong support for our hypothesis, especially given the lack of a significant overall effect on the sum of the donations.

conclude that no rough-and-ready rule exists for the effect of spatial distance between the donor and charitable output.

We proceed as follows. After a brief literature review in section 2, we give some background information in section 3. Section 4 explains the experimental design, section 5 presents a simple theory of matching incentives, and section 6 summarizes our hypotheses. Section 7 presents the results, and section 8 concludes.

## 2. Literature

*Charitable giving and public-good contributions might differ by wealth and income.* Several existing studies have uncovered differences in generosity by wealth or income. A U-shaped relationship has been documented, for example, by Andreoni (2006a) in observational data and Duquette and Hargaden (2021) in a within-subjects laboratory experiment. Moreover, in international rankings of generosity, both, high- and low-income countries are ranked at the very top (Blanco and Dalton 2019). Other studies, however, find either a positive relationship or no relationship with income or wealth (James and Sharpe 2007). Further studies concerned with differences in pro-sociality between the rich and the poor include Andreoni, Nikiforakis, and Stoop (2021), who find the rich of a Dutch city returned a misdelivered letter with a visible banknote inside more often than the poor. In contrast, Blanco and Dalton (2019) conclude the rich and poor from Bogota are equally generous and driven by similar motives. From a series of field experiments, Whillans, Caruso, and Dunn (2017) conclude the rich and the poor have different self-concepts: whereas the poor respond more to charity appeals that emphasize community, the rich do so when the charity appeal emphasizes agency. Other field experiments include de Oliveira, Croson, and Eckel (2011), de Oliveira, Eckel, and Croson (2012), and Li, de Oliveira, and Eckel (2017), who demonstrate that giving behavior among low-income people exhibits both persistence and context dependence. For example, experiences with crime increase the likelihood of donations. Bennett (2012 and 2018) shows that the working-poor exhibit giving patterns that are more similar to those of middle-income people than to those of the non-working poor. This research suggests that the patterns of giving might differ by social status, income, and wealth, which implies that fundraising strategies honed on data from WEIRD countries might not be optimal for poorer populations in the developing countries. Unfortunately, the number of studies on charitable behavior in developing countries is very limited. Exceptions include a nonexperimental study by Mahmud and Wahhaj, 2018, on voluntary contributions made by credit borrowers to their nonprofit microfinance institute in Pakistan. Other exceptions include Jack and Recalde, 2015, , Auriol et al., 2020, and Condra, Isaqzadeh, and Linardi, 2019, who study, however, issues that are largely unrelated to our investigation.

*Matching donations.* Starting with Eckel and Grossman (2003), Davis, Millner, and Reilly (2005), and Karlan and List (2007), a number of laboratory and field experiments analyzed matching incentives for charitable giving (see Epperson and Reif, 2019, for a review of the literature). Matching has been shown to increase the likelihood of giving but to lower the average donation given (also called the checkbook amount or out-of-pocket donation). Several studies find that, compared with an unconditional lead gift, matching leads to crowding out of larger donations, which can harm the overall success of a fundraising drive despite creating additional small gifts (Huck and Rasul 2011; Rondeau and List 2008; Huck, Rasul, and Shephard 2015).[7] Based on this literature, we expect that matching incentives will indeed increase the likelihood of giving in a poor population. However, in contrast to the above literature, we hypothesize that, in the poor population, we will not observe a reduction of out-of-pocket giving with matching.

*Local benefits*. Anecdotal evidence suggests donors prefer local charities. Using data from an online giving platform in the US, Meer (2014) finds some evidence in favor of local versus national preferences. Gallier et al. (2019) document that donors choose higher donations to a foodbank that is closer to their location. In an online experiment in England, Adena and Harke (2021) find that, on average, participants directed 55% of their donation to a UK project and the remainder to developing countries. In a hypothetical survey experiment with participants from New Zealand, Genç, Knowles, and Sullivan (2021) find donors prefer to support a charity that is active in New Zealand rather than charities in other countries. Similarly, Grimson, Knowles, and Stahlmann-Brown (2020) report that land owners in New Zealand choose to donate to charities that are located closer to them. In low-income Mexican villages, Candelo, Eckel, and Johnson (2018) find higher giving toward family members than toward community members or strangers, with no difference

[7] Some studies propose alternative matching schemes that might be reducing or avoiding crowding out: for example, matching where the match money goes to another, ideally complementary, project (Adena and Huck 2017b) or personalized threshold matching, where a fixed match kicks in if donors give at least as much as an individually set threshold (Adena and Huck 2019). Other innovative matching schemes analyzed include nonconvex matching (Castillo and Petrie 2021; Huck, Rasul, and Shephard 2015), matching conditional on a minimum number of donors in a group (Gee and Schreck 2018), matching for donations above the median (Charness and Holder 2019), or conditional on giving fixed amounts to two funds (Meier 2007).

between the last two groups.[8] By contrast, in a laboratory experiment with giving to real nonprofits, Brown, Meer, and Williams (2017) find no obvious preferences for local versus national charities. Based on this literature and on the hypothesis that local public goods matter more for poor individuals, we expect that our treatment highlighting the local benefits will achieve higher giving.

# 3. Background information

We partnered with a recently established charity called "Apake" in Kyrgyzstan. The charity implements projects to improve local life in different areas of Kyrgyzstan that are chosen from proposals submitted by citizens. After concentrating on corporate donors in the first phase of its existence, the charity launched its first large-scale campaign directed at small donors. The charity selected nine projects related to water supply, local infrastructure, hospitals, or school reconstructions, one in each of the administrative regions of Kyrgyzstan. The expected cost of all projects was 2 million KGS (approx. 28,600 USD).[9] One of the charity's corporate partners, a microfinance company, agreed to participate in the campaign by advertising the projects and collecting donations from its clients, and they were the only individuals targeted by the fundraising campaign in the period under study. Each office received a transparent donation box to be placed close to the cash desk, treatment-specific posters, treatment-specific flyers, and flyers with general information about the charity and the nine projects. Appendix A4 provides detailed content of posters and flyers.

Credit specialists were incentivized to inform as many clients as possible about the campaign. Every two weeks after the start of the experiment, credit specialists were ranked based on the percentage of clients who were aware of the fundraising campaign. These rankings did not have any direct monetary consequences for credit specialists but were part of the company's established ranking. Approximately every two months, the best performers received prizes, such as certificates, books, tickets for events, and so on. No incentives were in place for specialists relating to the amounts of donations collected.[10]

[8] Studies based on laboratory experiments with dictator games confirm that giving increases when social distance is reduced; see, for example, Hoffman et al. (1994).

[9] Realized costs for implementing the projects were 1,930,036 KGS. Data from the annual audit report are available on https://apake.kg/en/reports/. For USD/KGS, throughout the paper, we use average exchange rates for the experiment period.

[10] Note that clients do not have a motive to donate in order to get their loan approved. The reason is that active clients cannot receive an additional loan. In the case of the end of the term, they are almost automatically qualified for a new

Clients come to the office regularly to make a repayment for an active loan (see Figure B1d in the Appendix B1 for a distribution of repayments in the sample and period under study) or to acquire a loan. Once they put their donation into the donation box, they were asked to write down a telephone number and the amount donated. After the campaign was over, the charity made every donation verifiable on its website for the donor, by posting the first five and the last two digits of the cellphone number and the amount given. Two digits were replaced with a star, making identification of the donor difficult for an outsider. This approach was essential for transparency and accountability, especially because the charity was a new one without a reputation the clients could rely on. Thus, all donors could verify whether their donations had reached the fund. The fact that the donations were made public only after the end of campaign prevented any unintended dynamics, and the semi-anonymization reduced social pressure, though, if any existed, it was equal between treatments. For us, this approach was a convenient way to match the donors with the client database and avoid spillovers between clients-cum-donors.[11] Appendix A1 provides additional details of the campaign.

The population under study consisted mostly of people who are self-employed and, on average, owe a debt equal to an average monthly income. Average/median self-reported monthly income in our data was 21,304/18,633 KGS (approx. 306/268 USD), which compares to a GDP per household of approximately 530 USD monthly.[12] Note the income data in our sample were self-reported because no formal proof is available in most cases. The company does not rely (much) on income declaration when deciding about loans. Thus, our data on income are likely to be inflated, and the population under study is likely to be poorer than these numbers suggest. Note also that the population with a formally verifiable income or collateral would also have access to less expensive loans from banks (provided geographic access). Additionally, those who are self-employed and have businesses with a verifiable regular income qualify for business loans by banks, which are much cheaper. Thus, by focusing on clients of the microfinance institution we study a more

loan, if they successfully repaid previous loans. This scenario is very different from the environment studied in Mahmud and Wahhaj (2018).

[11] Clients could donate to the fund in another way—through cash-in machines that are typically placed in big shops or banks and are typically used to refill the prepaid cell phones. This method was mentioned on the posters placed in the microfinance offices. Only a few donations were made through the terminals, and all could be matched to the customers. Therefore, we count them with the other donations without explicitly distinguishing them.

[12] This number is based on the annual GDP per capita (current USD) of 1,220.47 USD (2017) (api.worldbank.org/v2/en/country/KGZ?downloadformat=excel, viewed 04.06.2019) and an average size of a household of 5.21; see Table A2a in the Appendix A2.

vulnerable segment of the population, also relative to other people in Kyrgyzstan. More details on the population under study can be found in Appendix A2.

The loan sums range from around 70 USD to 2,850 USD. The interest rate in our period is between 11% and 50%,[13] with an average of around 35% per year. All clients have to repay loans monthly, on a pre-specified date without delay, but they are also free to repay more, or more often. More details on the loan conditions and the company's way of working can be found in Appendix A3.

# 4. Experimental Design

We implemented a 2x2 design. The first experimental dimension relates to donation matching: clients of one half of the offices were informed that a lead donor had already contributed half a million KGS (around 7,000 USD). Clients of the other half were informed that a large donor would match their donations one by one up to a threshold of half a million KGS. In both cases, the information was true, with the microfinance company acting as a lead donor and the experimenters matching donations. Given that the final collected amount was close to half a million KGS, the signaling value of both treatments should be equivalent even if potential donors did not take the upper threshold level at its face value but formed rational expectations.[14] The exact source of the money was not mentioned to clients.

The second experimental dimension varied local benefits of donations given. Clients of one half of the offices did not get any additional information, while clients of the other half were informed that "If clients of [name of the company] from your region donate the highest amount per active client, the next project that will be funded from the charity will aim to help your region!" This was implemented later.[15] In the local-benefits treatment, we thus raise the utility of the donation for those who have stronger preferences for local charitable output, while keeping the charitable organization fixed.[16] Note we have no reason to assume ex ante that one region or another has a

[13] For the Islamic type of loans, we converted the fee to the equivalent interest rate. The sample also contained 740 loans with interest in the range of 0%–5%. These preferential loans can be issued as financial help for long-term clients for emergency situations.
[14] With expectations being not rational and very low, the signaling value could be lower in the matching treatment. This scenario, however, would lead to an even harder test for the matching treatment to outperform a lead-donor treatment with a higher signaling value.
[15] Contributions from offices without the local treatment also count toward the average per region.
[16] Even if the treatment introduces an element of competition, we do not expect the competition solely to affect behavior. Rather like in Augenblick and Cunha (2015), we expect our treatment to shift attention toward/switch on the parameter on local charity in the utility function, and thus expect a positive effect if and only if preference for local charity is present.

higher chance of donating the highest amount per client. Despite some differences in the regions, the population in focus is quite homogeneous, because the average loan sums and interest rates are the same across regions.

Prior to the implementation, we performed blocked randomization in order to reduce the risk that the treatment groups differed on some important dimensions. For this reason, we used the blockTools package in R (Moore and Schnakenberg 2016), taking into account a rich set of individual-, specialist-, and office-level variables. The results of the randomization process make us confident that no major differences exist between the treatment groups. Given the large number of levels and variables, some differences cannot be avoided, but we make sure to control for any imbalances by adding control variables in regressions, and we cluster errors at the office level in our specifications. We describe the randomization procedure and the results of the balancing tests in detail in Appendix A5.

Five offices were closed, and a few new offices opened in the period between the randomization (January 2018) and the end of the campaign (Mai 2018). Management included in the experiment one office that was not part of the randomization but opened before the start of the experiment. Another office was merged with another nearby office. Thus, the final sample available for analysis includes 99 offices and 185,845 clients.[17] Consistent with the goal of keeping the original randomization balances, we also replicate our analyses for what we call the conservative sample, which excludes the offices from incomplete randomization blocks and the office that was not part of the original randomization.[18] This procedure leaves us with 80 offices and 152,319 clients.

[17] These clients are those with an active loan at the time of the experiment, all of which are included in the subsequent analysis, reflecting an intention to treat (ITT) approach. Indeed, the vast majority are likely to have received some form of treatment. Among the clients with an active loan at the time of the experiment, more than 92% made at least one repayment during the campaign, and the repayments were made predominantly in the office where posters, flyers, and the donation box were very visible. But even those who did not visit the office during the campaign might have received an information call from their credit specialists (in the survey, non-visitors reported knowledge of the campaign with a probability of around half the size of that of visitors).

[18] The randomization procedure created blocks of four offices that are most similar on observables. In each block, the offices were randomly distributed into four treatments, with one office per treatment. In the conservative sample, if one of the offices from the block was no longer part of the sample (e.g., the office was closed), the other three offices belonging to the same block were also excluded from the analysis. Therefore, apart from five closed offices and one merged office, a further 18 offices were excluded, altogether 24 offices and six blocks from the original randomization sample of 104. Additionally, we excluded the office that opened before the start of the experiment but was not part of the original randomization. This approach preserves the balance of the sample and leaves us with 80 offices (conservative sample).

To measure the spread of information from credit specialists to the clients, for each credit specialist, the firm's internal call center made survey calls to a random subsample of his/her clients and recorded whether the client was aware of the specific fundraising campaign.

# 5. Theoretical effects of a matching treatment

This section aims to illustrate under which theoretical conditions matching leads to crowding in of additional donations on the extensive margin and crowding out of larger donations on the intensive margin—the relationships that are typically found in the literature so far—and under which conditions the opposite applies.[19]

Let $u(y, x)$ be the agent's least concave utility function (Kannai 1980) over private consumption, $y$, and achieved donation, $x$, that is the out-of-pocket donation plus the applicable match if one exists. We assume $u$ to be strictly increasing in both arguments and strictly concave. The agent's income is denoted by $I$. The price of the charitable good (manipulated through matching) is denoted by $p$. In the absence of matching, we have $p = 1$, and in case of 1:1 matching, we have $p = 0.5$.[20] The agent's maximization problem is

$$max\ u(y, x) \text{ subject to the budget constraint } I = y + px. \quad (1)$$

The solution to (1) gives the agent's demand function for the charitable good, $x(I, p)$, and we obtain the slope of the indifference curves as $\frac{dy}{dx} = -\frac{u_x}{u_y}$ and the price elasticity of the demand for donations as

$$e_{x,p} = \frac{\partial x(I,p)/\partial p}{x(I,p)/p}. \qquad (2)$$

## 5.1. Conditions for crowding in on the extensive margin

For a falling price to generate additional donations, the slope of the indifference curve must be bigger than –1 for zero donations. This requirement ensures a corner solution where the agent does not donate. At the same time, the indifference curves must not be fully flat; otherwise, corner

[19] Note that hypotheses concerning the effect of matching donations in our pre-registration, described in section 6, were based on our intuition of the channels formalized in this section. For transparency, we acknowledge that we formalized our intuition only after conducting the experiments.
[20] Note that in the theory part, we only analyze the effect of a price change, keeping the utility and the charitable good fixed. Therefore, when implemented in practice, the control treatment must have the same signaling value for the donor as the matching treatment. Thus, the appropriate baseline is a lead-donor treatment.

solutions would obtain for any level of matching. Specifically, when matching induces a price $p$ smaller than 1, crowding in obtains if

$$-p > -\frac{u_x}{u_y} > -1 \text{ at } (\mathrm{y}, x) = (I, 0). \qquad (3)$$

Thus, crowding in should always result from matching if the price falls enough and as long as some donors in the sample do not donate if the price is equal to 1.

## 5.2. Conditions for crowding out on the intensive margin

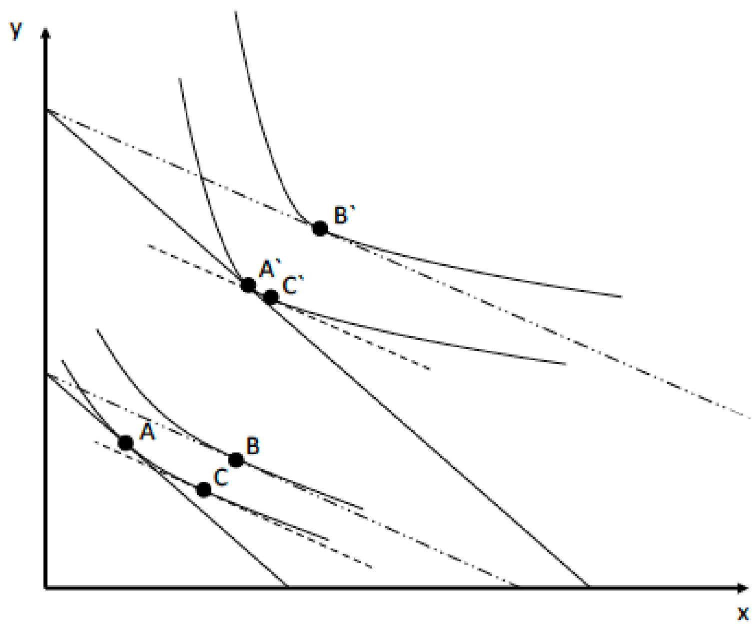
Crowding out occurs whenever the local elasticity of demand for the charitable good at the optimally chosen bundle for $p$ = 1 is greater than –1, that is, when demand is not too price elastic. In Appendix A8, we show that for crowding out to occur, we need $0 > \frac{u_y}{x(u_{xx}-u_{xy})} > -1$. The first part of this inequality is satisfied whenever $u_{xx} < u_{xy}$, which holds as long as $x$ and $y$ are not perfect substitutes, and for the second part, one obtains

$$u_{xy} > \frac{u_y}{x} + u_{xx}. \qquad (4)$$

Hence, for crowding out to occur, complementarity between private consumption and the charitable good must be sufficiently strong at the optimal bundle (or, rather, the degree of substitutability must not be too large). How does this condition relate to income? Crowding out for a rich agent and no crowding or crowding in for a poor agent requires increasing complementarity of the charitable good and own consumption. In terms of the agent's indifference curves, this translates to increasing convexity for higher utility levels. Now note that increasing convexity reduces the scope for substitution effects in the presence of price changes. Hence, for richer agents, the adjustment to a lower price of the charitable good will be predominantly determined by the income effect, such that they will increase their own consumption and crowding out will occur. On the other hand, poorer agents feel a stronger trade-off between the charitable good and own consumption, generating larger substitution effects that may dominate the income effect. Hence, own consumption may stay constant or even fall, as illustrated in Figure 1.

We find such a pattern plausible: the poor feel a stronger tradeoff between own consumption and the charitable good; for the rich, the tension of substitution is reduced or even turned into complementarity, whereby, when consuming more, they might feel ever greater joy when others' consumption increases as well.

Figure 1: Crowding out and crowding in of donations on the intensive margin




Notes: x – charitable donation; y – private consumption; AC – large substitution effect for a poor individual, resulting in crowding in of donations; A'C' small substitution effect for a rich individual resulting in crowding out of donations.

## 6. Hypotheses

We pre-registered a set of hypotheses at AEA RCT Registry (AEARCTR-0002693, March 5, 2018). Our central substantive hypotheses are as follows:

*M (Matching)*[21]

**M1 The likelihood of giving is higher in the matching than in the control treatment with an unconditional lead gift.**

**M2 The amount given (conditional on giving) does not differ between the matching and the unconditional lead gift treatment.**

**M3 The combined effect (that is, the return from the campaign) is higher in the matching than in the unconditional lead gift treatment.**

[21] H3 in the pre-registration.

Motivation: Based on previous research (e.g., Huck and Rasul 2011; Huck, Rasul, and Shephard 2015; Adena and Huck 2017), we could expect matching to crowd in small donations and crowd out large ones. Because our sample consists of low-income individuals, we expect them to have a substantially higher price elasticity for the charitable good than previously studied middle- and high-income individuals. Consequently, we expect only the crowding-in effect to hold, inducing a larger number of gifts, with all donation values being small. This intuition is formalized in the model above. We assume most of the experiment participants do not reach the consumption levels such that condition (4) is fulfilled, which implies the hypotheses above.

*L (Local benefits)*[22]

**L1 The likelihood of giving does not differ between treatments with or without local benefits.**

**L2 The amount given, conditional on giving, is higher in the treatment with local benefits than in the treatment without.**

**L3 The combined effect (return) is positive in the local-benefits treatment.**

Motivation: In light of the reasoning by Whillans, Caruso, and Dunn (2017), who stress the importance of community for giving by the poor, and supported by the idea that the poor are more likely to benefit personally from projects that are in their vicinity, we expect a preference for local projects and thus higher giving in the treatment with local benefits.[23]

Note M/L1–3 are not independent hypotheses but M/L3 linearly depend on M/L1 and M/L2. The total number of independent tests is thus six, with M/L1 and M/L2 being our main hypotheses. We opt against multiplicity hypotheses testing (MHT) corrections, which we explain in detail in Appendix A6. Note, however, that we take a conservative approach by clustering errors at the office level. Note also that we do not derive any hypotheses for the interactions for two major reasons: lack of power (which is indirectly related to MHT and further discussed in Appendix A6) and because we find no obvious prior to be derived from theory or the previous literature. Note as well that, in practice, charities often use different incentives and framings in combination, such that no natural baseline exists. In our later analysis, the average effects of the matching (or local)

[22] H4 in the pre-registration.

[23] Concerning both margins, which margin (or both) the effect should go through is unclear. Our hypothesis that it solely goes through the intensive margin is necessarily speculative. In fact, we were agnostic here and mainly wanted to pre-register the fact that we want to explore both margins.

treatment are a weighted average of the average treatment effect of each version on the other dimension.

Additionally, we formulated two supporting hypotheses regarding our specific implementation and the spread of the information from credit specialists to the clients, which are presented for completeness in Appendix A7 and analyzed in Appendix B6.

# 7. Results

First, we provide overall results of the campaign in subsection 7.1. Then, we provide the results for our main hypotheses regarding matching and local incentives in subsection 7.2. Subsection 7.3 contains more detailed analyses of the price elasticity, followed by the results regarding our supporting hypotheses for the behavior of credit specialists in section 7.4.

## 7.1. Campaign results

The total number of donations claimed was 7,027, generating a response rate of 3.8%. The average donation conditional on giving was 63 KGS (approx. 0.90 USD; see Figure B1a in Appendix B1 for a histogram of donations).[24] Among all claimed donations, 6,421 could be linked to a client of the microfinance company. The remaining 606 (8.6% of all claimed donations) could only be assigned to the office in which the donation was made. In Table 1, we test and confirm that no differences exist between treatments in the share of unidentified donation claims.[25]

Table 1: Probability of an unidentified donation

| Dependent variable: dummy unidentified donation | | |
|---|---|---|
| Treatment matching | -0.001 | -0.002 |
| | (0.007) | (0.008) |
| Treatment local | 0.006 | -0.001 |
| | (0.007) | (0.008) |
| Observations | 7027 | 6282 |
| $R^2$ | 0.000 | 0.000 |
| Offices included | all (99) | conservative (80) |

Notes: OLS; standard errors in parentheses; sample of positive donations; conservative sample excludes incomplete blocks of four from the randomization stage and new offices. * p < 0.10, ** p < 0.05, *** p < 0.01

[24] Collected donations plus match money (excluding the lead donation in treatments without matching) amounted to approximately 38% of the total project costs.

[25] Note that in Table 1, we present standard errors instead of robust or clustered ones, because this choice is more conservative given that we want to confirm a zero effect.

Table 2: Deviations between actual and claimed donations by treatment

| Dependent variable: deviations in donations | | |
|---|---|---|
| treatment local | 74.778 | 118.821 |
| | (224.262) | (233.614) |
| treatment matching | 284.066 | 195.224 |
| | (224.354) | (238.433) |
| Observations | 99 | 99 |
| $R^2$ | 0.018 | 0.079 |
| controls | - | yes |

Notes: OLS; averages by office; standard errors in parentheses; sample of offices; controls include number of clients and region dummies. * p < 0.10, ** p < 0.05, *** p < 0.01

Table 3: Summary statistics by treatments

| | Lead donor | Matching | Non local | Local |
|---|---|---|---|---|
| Percent of clients who donated | 3.1% | 4.4% | 3.6% | 4.0% |
| Average positive donation, KGS | 64.20 | 62.45 | 61.49 | 64.78 |
| Average donation per client, KGS | 2.00 | 2.74 | 2.22 | 2.58 |
| Average donation per office, KGS | 3,508.60 | 5,516.29 | 4,296.96 | 4,670.88 |
| Share of unidentified donations | 8.6% | 8.6% | 8.3% | 8.9% |
| Number of clients | 89,253 | 96,592 | 96,987 | 88,858 |
| Number of offices | 51 | 48 | 50 | 49 |

Notes: Full sample. Average donation per office is based on the total sum of donations in the donation boxes and includes unidentified donations. Table B3a in Appendix B3 presents summary statistics by four treatment groups separately.

We identify differences between claimed donations and the content of the donation boxes, with, on average, an additional KGS 409 in the donation boxes (see Figure B1b in Appendix B1 for the distribution of differences by office). These differences may have resulted from some donors refusing to write down their telephone number, claiming they had donated less than they actually had, or the cashier overlooking their donation. Again, in Table 2, we test and confirm no significant differences existed between treatments.

Table 3 provides summary statistics of relevant outcomes by treatment. The next section is informative about the significance of the differences, where we necessarily correct for inter-office correlations.

## 7.2. Treatment effects on clients

First, we study the likelihood of giving by treatment. In Table 4, we regress the donation dummy on treatment dummies in a linear probability framework.[26] Column I presents the results for the

[26] Probit or logit regressions lead to similar results. Here, we prefer OLS for convergence and multicollinearity reasons (given a large number of dummy control variables in some regressions) as well as because logit analysis is suboptimal in finite samples of rare-events data (King and Zeng 2001).

full sample, including unidentified donations and without controls. Column II excludes unidentified donations. In column III, the sample is restricted to the conservative sample. Column IV includes controls in the conservative sample to increase precision and is our preferred specification. Independent of the sample restrictions and the presence of controls, the coefficients of the matching treatment are positive and significant.[27] The effect is estimated to be 1.2 percentage points. Given the average response rate of 3.1% in the lead-donor treatment, this effect amounts to a 39% increase in the likelihood of giving. The coefficient on the local treatment is much smaller and never significant. Thus, the results support hypotheses M1 and L1.

The increase in the likelihood of giving is in line with previous findings on matching. However, the primary motivation of the paper is to understand whether the previously found crowding-out effect is reduced in a poorer population due to higher price elasticity. A first impression can be gained from Table 5, which presents the results of OLS estimations of the log of the positive donation amount on treatment dummies. Columns I–IV follow the same sample restrictions and specifications as those in Table 4. In all our models, the coefficients on treatments are not significant. Therefore, we confirm hypothesis M2—we find no evidence of a difference in the amount given between the matching and lead-gift treatment.

Table 4: Treatment effects on the likelihood of giving

Dependent variable: donation dummy

| | I | II | III | IV |
|---|---|---|---|---|
| treatment matching | 0.013** | 0.012** | 0.013* | 0.012*** |
| | (0.006) | (0.005) | (0.006) | (0.004) |
| treatment local | 0.003 | 0.003 | 0.004 | -0.003 |
| | (0.006) | (0.006) | (0.007) | (0.005) |
| Observations | 185845 | 185239 | 152319 | 149969 |
| R2 | 0.001 | 0.001 | 0.001 | 0.018 |
| Controls | - | - | - | yes |
| Sample | incl. unidentified don. | excl. unidentified don. | conservative + excl. unidentified don. | conservative + excl. unidentified don. |

Notes: OLS; robust errors clustered at the office level; conservative sample excludes incomplete blocks of four from the randomization stage and new offices; the full sample with controls is identical to the one excluding unidentified donors because no controls are available for those observations; controls include dummies for the randomization-level block, client-level controls including gender of the client, age of the client, the number of previous loans taken in the company, education-level dummies, marital-status dummies, occupation-type dummies, dummies for taking up and closing the loan in the experiment period, self-reported income, interest rate of the loan, the sum of due repayment delayed for more than 30 days, and the term of the loan in months; office- and region-level controls including dummy for urban areas, region dummy, number of clients per office; specialist-level controls including client number, portfolio size, age, number of children, education dummies, experience in months, family size, female dummy, material status dummies, and nationality dummies. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[27] Note our hypotheses are directional (when assuming a difference), whereas the tests are not.

Table 5: Treatment effects on the intensive margin

Dependent variable: log of donation amount

| | I | II | III | IV |
|---|---|---|---|---|
| treatment matching | -0.050 | -0.060 | -0.061 | -0.023 |
| | (0.093) | (0.092) | (0.105) | (0.058) |
| treatment local | 0.031 | 0.032 | 0.016 | 0.118** |
| | (0.105) | (0.105) | (0.118) | (0.058) |
| Observations | 7027 | 6421 | 5482 | 5148 |
| R2 | 0.001 | 0.001 | 0.001 | 0.135 |
| controls | - | - | - | yes |
| sample | incl. unidentified don. | excl. unidentified don. | conservative + excl. unidentified don. | conservative + excl. unidentified don. |

Notes: See notes to Table 4.

The absence of significant effects of the matching treatment is in line with our hypothesis. However, the coefficients do have a negative sign, which does not allow us yet to reject crowding out. Given that one of our central questions of interest is the absence (or at least reduction) of crowding out in a poorer population, we take a closer look at this issue in the section 7.3.

Regarding the local-benefits treatment, column IV, which is our preferred specification, provides support for Hypothesis L2. Given that the coefficient is only significant in column IV, we provide some robustness checks and further discussion in Appendix B2, while being careful not to overinterpret this result.

Table 6. Treatment effects on total donations

Dependent variable: donation amount plus one, logged

| | I | II | III | IV |
|---|---|---|---|---|
| treatment matching | 0.046** | 0.042** | 0.046* | 0.048*** |
| | (0.021) | (0.020) | (0.024) | (0.016) |
| treatment local | 0.014 | 0.012 | 0.017 | -0.007 |
| | (0.022) | (0.020) | (0.024) | (0.018) |
| Observations | 185845 | 185239 | 152319 | 149969 |
| R2 | 0.001 | 0.001 | 0.001 | 0.016 |
| controls | - | - | - | yes |
| sample | incl. unidentified don. | excl. unidentified don. | conservative + excl. unidentified don. | conservative + excl. unidentified don. |

Notes: See notes to Table 4.

Finally, we study the overall effects of the treatments on the returns from the campaign. Table 6 presents the results of OLS regressions with the dependent variable being the log of donations plus

1. This approach is standard in the literature because it better accounts for the skewed distribution of donation amounts. In Table B3b in Appendix B3, we also report the effects on the non-logged donation values. Columns I–IV apply the same sample restrictions and specifications as those in Tables 4 and 5.

In all columns, the coefficients of the matching treatment are positive and significant, suggesting a positive increase in returns from the campaign. This result is in contrast to some previous findings documenting adverse overall effects of matching. Regarding the local-benefits treatment, the overall effect is not significant, against our hypothesis.

## 7.3. Is there really no crowding out?

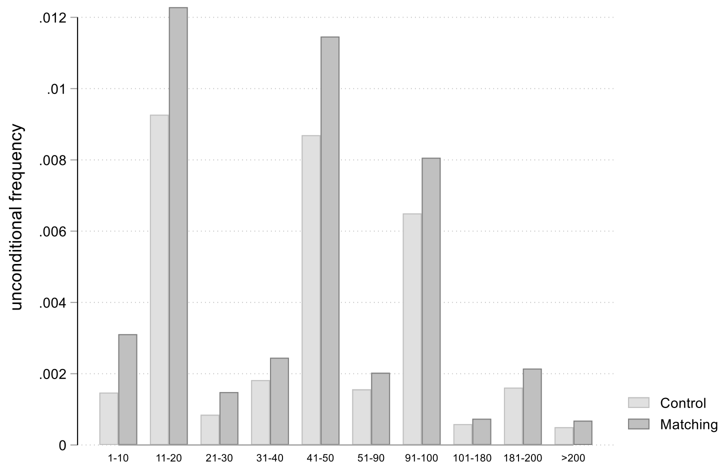
The question of whether we can really rule out crowding out in our data remains to be answered. In Table 5, in which the matching dummy is regressed onto log positive donations, we observe a small negative but statistically insignificant coefficient. This finding could result from large donors reducing their out-of-pocket donations in response to the matching (crowding out) or from additional donations attracted by the matching treatment being small (crowding in of small donations), thus reducing the average.[28] Whether the first or second effect is operative can be better assessed once we look more closely at the unconditional distributions of positive donations in both treatments.

Figure 2 shows the share of individuals donating an amount that falls into a particular monetary category (the share of non-givers, which is the remainder, is not shown). What can be inferred from the figure is that our matching treatment clearly *generates new giving in each category*, whereas the increases are somewhat more pronounced in lower categories. This observation strongly suggests no crowding out occurs in our sample.[29]

[28] Of course, we cannot exclude the possibility that all additional donors in the matching treatment would give high amounts, whereas the large donors give less than they would give in the control treatment. However, we deem this scenario unlikely.

[29] For comparison, Figure B1c in the Appendix B1 shows the equivalent exercise using data from Huck and Rasul (2011), who document meaningful crowding-out effects. In their sample of Munich opera attendees, in addition to crowding in small donations, the matching treatment clearly crowds out large donations—the frequency of donations in different categories above 150 EUR is always smaller in the matching treatment than in the lead-donor control.

Figure 2: Distribution of donations by categories in the matching versus control treatment




Notes: The horizontal axis presents the bins of the donation amounts in KGS. The vertical axis presents the percentage of the clients out of the total sample in respective treatments who donated amounts falling in the respective bins. In choosing the bins, we first used bins of 10 for donations up to 100 KGS, of 20 for donations up to 200 KGS, of 50 for donations up to 400 KGS, followed by 500, 1000, and 2000 KGS category. In all cases, the frequency is equal or higher in the matching treatment than in the unconditional lead gift. However, given the very low frequency in some bins, the above figure combines several bins into one.

A more formal assessment of the effectiveness of our matching treatment can be achieved by examining the price elasticity in detail. In Appendix B4, we calculate price elasticity for several other field experiments, all conducted in high-income countries, using a uniform method (arc elasticity) that requires a minimum amount of data. In almost all studies that have both a lead-donor and a 1:1 matching treatment, the price elasticity is above –1, whereas in our case, it is –1.393.

Finally, in our data, we have two sources of variation in the price for giving.[30] The first source results from our treatment manipulation and is purely exogenous. The second source results from the fact that the money donated cannot be used to repay the loan and costs the individual 1 plus the interest rate.[31] In Appendix B5, we present estimates of interest-induced price elasticity in our sample. The resulting estimates are –2.5 to –3.0, and very clearly below –1.

[30] We thank Kim Scharf for this point.
[31] The microfinance company allows for flexible repayments on top of the monthly rate. Indeed, we observe a non-negligible number of additional repayments above the required 2–3 times in the period under study; see Figure B1d in Appendix B1. Additionally, the repayment amounts vary.

Summing up this section, we find *no support for crowding out* in our sample. We observe that donors in our sample exhibit the most price-elastic demand for charitable goods compared with previous studies.

### 7.4 Is it about income?

So far, we attribute the higher elasticity and thus the absence of crowding out to the fact that our sample is poorer than the samples of previous studies. Although we cannot directly establish a causal relation between income and crowding out, we can analyze the observed within-sample heterogeneity of crowding out as a function of participants' income. Panel A of Table B1b in Appendix B1 presents the coefficients of the matching-treatment dummy in OLS regressions of the log positive donation on both treatment dummies, splitting the sample by income for a variety of income thresholds. For all income thresholds and subgroups, the coefficient for the matching treatment is not significant, confirming the global absence of crowding out in our sample. Note, however, that independent of the threshold, the higher-income group always has a lower coefficient, thus edging closer toward crowding out than the lower-income group. Panel B of Table B1b in Appendix B1 presents the coefficients of the matching-treatment dummy in an OLS regression of a donation dummy. Unlike the results for the log positive donation amount, the coefficients for the treatment dummy are essentially the same for all income groups. Both results are in line with our theoretical analysis. First, we can expect crowding in everywhere, because our sample predominantly consists of nondonors (especially at a price of the donation equal to 1) and all individuals in the sample are indebted. Second, we can expect crowding out in case of sufficiently high complementarity between charitable goods and private consumption, which is more likely to occur in a higher-income sample.

What about other possible differences between our sample and typical samples studied so far could explain the absence of the crowding-out result? Although we cannot control for all differences, we can investigate heterogeneous treatment effects, which we do for three features that make our sample distinct from previous samples: our sample has (i) a lower share of participants with post-school degrees, (ii) a higher share of people living in non-urban areas, and (iii) a higher share of those occupied in agriculture. Table 7 presents the results of OLS estimations of the log of the positive donation amount on treatment dummies, including an interaction of the matching treatment with indicators for high income (80th percentile of the sample), higher education, living in an urban area, and occupation in agriculture. Column I presents results without controls, and column II includes controls. Neither model shows a significant crowding-out effect on average, because the treatment dummy is not significant, in line with our findings above. However, the coefficient is significantly lower for 20% of the richest clients in our sample. By contrast, interactions of the treatment dummy with dummies for higher education, living in a city, or occupation in agriculture are not significant, suggesting the absence of crowding-out results in our sample is likely to be driven by income.

Table 7: Heterogeneous treatment effects on the intensive margin

| Dependent variable: log of donation amount | I | II |
|---|---|---|
| treatment matching | -0.141 | -0.035 |
| | (0.139) | (0.088) |
| treatment local | 0.019 | 0.088 |
| | (0.104) | (0.056) |
| treatment matching X high income | -0.105** | -0.223*** |
| | (0.052) | (0.046) |
| treatment matching X higher education | -0.004 | -0.025 |
| | (0.052) | (0.068) |
| treatment matching X urban | 0.180 | 0.108 |
| | (0.164) | (0.156) |
| treatment matching X agriculture | 0.023 | -0.033 |
| | (0.050) | (0.062) |
| Observations | 6194 | 5973 |
| R2 | 0.008 | 0.12 |
| controls | - | yes |
| sample | excl. unidentified don. | excl. unidentified don. |

Notes: OLS; robust errors clustered at the office level; variable "treatment matching X high income" is the interaction of the dummy for treatment matching and the dummy for the income being higher than 41,000 KGS (80th percentile of income in the sample). The variable "treatment matching X higher education" is the interaction of the dummy for treatment matching and the dummy for having higher education. The variable "treatment matching X urban" is the interaction of the dummy for treatment matching and the dummy for living in a city. The variable "treatment matching X agriculture" is the interaction of the dummy for treatment matching and the dummy for being occupied in agriculture. Controls are described in the notes of Table 4. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

# 8. Conclusions

We conducted a large-scale field experiment with a sample of individuals who are much poorer than the usual subjects in fundraising experiments. The relative poverty of the population we study led us to formulate two hypotheses. First, we hypothesized that matching should outperform the lead-donor treatment because we would observe more price-elastic demand for the charitable good. Second, we hypothesized that local benefits would increase giving because the poor care more about their community and are more likely to benefit from local public goods.

To study the first hypothesis, we compared a treatment with matching to a treatment without (making sure the commitment from a lead donor was constant in both environments). In contrast to previous findings from fundraising among the middle classes, we found that matching leads to a higher response *without* any crowding out, supporting our conjecture about more price-elastic demand for charitable goods among the poor. In line with our theoretical considerations, we do not believe our results contradict previous findings on matching. On the contrary, they illustrate remarkable consistency in the links between income and price elasticity. The implications for fundraising managers, of course, are drastic. Although matching generates adverse effects when fundraising among the rich, it unambiguously improves fundraising among the poor. Our findings suggest successful corporate philanthropy designs from the developed countries established by the headquarters need to be adjusted before being implemented in foreign branches with poor populations.

Our second treatment varied the probability of future charitable output produced in a donor's region, keeping the charity producing it constant. We found little effect of the treatment variation. Our population shows no strong preference for local charitable output. This result should be taken with a grain of salt, however, because the variations in distance from the charitable output that we implemented were relatively small, specifically in comparison to the difference between giving to a national or international cause.

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# Online Appendix for
# Charitable giving by the poor
# A field experiment in Kyrgyzstan

# Appendix A.

## A1 Details of the fundraising campaign

At the time of the fundraising campaign, the company had around 650 active credit specialists in over 100 offices, each of which has a manager. Credit specialists work for a specific office only and sell micro-loans to members of the local community.

Before the start of the drive, at the beginning of March, all managers came to the capital city for a retreat (this is typically an annual or semi-annual event). During the retreat, the micro-finance company's CEO announced the fundraising campaign (not treatment specific) and the fund. The director of the fund also gave a presentation about the nine projects. On March 27, the managers of each office received treatment-specific explanations as an audio message from the CEO and scripts for communications with the clients. On March 29, all credit specialists received promotional videos (not treatment specific) about the fundraising campaign and the fund on their mobile phones in three languages: Kyrgyz, Uzbek, and Russian. They also received detailed, treatment-specific instructions by email, which included the main idea and a short script for communication with clients. All managers were instructed to discuss the (treatment-specific) details of the experiment and publicly answered questions from credit specialists during weekly morning meetings. Credit specialists were advised to inform their clients about the charitable campaign. The fundraising call lasted around two months until the end of May 2018.

Every week, the manager of the office took a photo of all new donation receipts and sent it to the director of the fund. Due to logistical constraints, the official collection of the donations was conducted only once, after the end of the experiment by an accountant of the fund. The sum of donations inside the boxes was compared to the sum claimed on the receipts.

To sum up, there were three ways for clients to learn about the campaign: First, when they arrived at the office for regular repayments and saw the posters and the donation box; second, when they were contacted by the credit specialist to advertise the campaign; third, when they received the call from the survey call-center.

## A2 Population under study

In order to better understand how the population under study compares to the rest of the population in Kyrgyzstan, we draw on the Life in Kyrgyzstan (LiK) representative survey (2010-2013). Among the approximately 3,000 households surveyed, 7.4 percent indicated having obtained a loan/credit at a microfinance company in the last 12 months (12.3 percent: any loan/credit in the last 12 months). The average household income was similar, independent of the microcredit intake, at 18,500 soms (see more comparisons in Table A2a).[1] In LiK, 3.7 percent of households indicated having donated funds to poor and other vulnerable people while according to the World Giving Index 2017, 29 percent indicated having donated to a charity in a past month. Globally, according to the Focus Economics ranking of the countries for 2019 and 2020, Kyrgyzstan is ninth poorest country in the world.[2] In the Global Finance 2016 rank, Kyrgyzstan is number 148 out of 189.[3] Broader indices that include aspects such as education or rule of law rank Kyrgyzstan somewhat in the middle (see, for example, the Legatum Prosperity Index™ 2017).[4]

[1] Note that the data from the panel dated back five years, thus the nominal income is not directly comparable to the data from 2018.
[2] https://www.focus-economics.com/blog/the-poorest-countries-in-the-world, date accessed 03.12.2018
[3] https://www.gfmag.com/global-data/economic-data/worlds-richest-and-poorest-countries, date accessed 03.12.2018
[4] https://www.prosperity.com/rankings, date accessed 03.12.2018

Table A2a: Life in Kyrgyzstan survey—comparing individuals with and without microcredit

| | | hh has taken a loan from a microcredit agency in the last 12 months | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | no | | | yes | | | |
| Variable | Values and labels | mean | se | N | mean | se | N | t-test p-value |
| number of HH members | 1-16 | 5.21 | 0.05 | 2210 | 5.23 | 0.17 | 176 | 0.903 |
| dummy: HH member donated funds to poor and other vulnerable people | 1-yes, 0-no | 0.04 | 0.00 | 2190 | 0.05 | 0.02 | 173 | 0.374 |
| total hours all HH members spent donating funds to poor and other vulnerable people | 0-40 | 0.15 | 0.03 | 2190 | 0.13 | 0.05 | 173 | 0.739 |
| district code | 0-city, 1-village | 0.63 | 0.01 | 2210 | 0.61 | 0.04 | 176 | 0.568 |
| total HH income in soms | 0-230000 | 18473.13 | 382.77 | 2210 | 18384.53 | 1007.03 | 176 | 0.935 |
| total HH income in soms / equalized by square root scale | 0-91000 | 8359.63 | 164.48 | 2210 | 8508.12 | 513.19 | 176 | 0.783 |
| general satisfaction with life / average of all adult HH members | 0-extremely unsatisfied, 10-absolutely satisfied | 6.88 | 0.03 | 2203 | 6.93 | 0.12 | 176 | 0.648 |
| satisfaction with HH income / average of all adult HH members | 0-extremely unsatisfied, 10-absolutely satisfied | 6.40 | 0.04 | 2185 | 6.45 | 0.13 | 176 | 0.702 |
| satisfaction with standard of living / average of all adult HH members | 0-extremely unsatisfied, 10-absolutely satisfied | 6.54 | 0.04 | 2202 | 6.42 | 0.13 | 175 | 0.346 |
| satisfaction with income situation / average of all adult HH members | 0-extremely unsatisfied, 10-absolutely satisfied | 6.05 | 0.03 | 2207 | 6.23 | 0.12 | 176 | 0.146 |
| satisfaction with income situation compared to others from village / average of all adult HH members | 0-extremely unsatisfied, 10-absolutely satisfied | 6.04 | 0.03 | 2207 | 6.14 | 0.12 | 176 | 0.461 |
| dummy: general satisfaction with life | 1-dissatisfied, 0-neutral or satisfied | 0.05 | 0.00 | 2203 | 0.07 | 0.02 | 176 | 0.472 |
| dummy: satisfaction with HH income | 1-dissatisfied, 0-neutral or satisfied | 0.11 | 0.01 | 2185 | 0.11 | 0.02 | 176 | 0.960 |
| dummy: satisfaction with standard of living | 1-dissatisfied, 0-neutral or satisfied | 0.08 | 0.01 | 2202 | 0.10 | 0.02 | 175 | 0.599 |
| dummy: satisfaction with income situation | 1-dissatisfied, 0-neutral or satisfied | 0.14 | 0.01 | 2207 | 0.09 | 0.02 | 176 | 0.044 |
| dummy: satisfaction with income situation compared to others from village | 1-dissatisfied, 0-neutral or satisfied | 0.14 | 0.01 | 2207 | 0.11 | 0.02 | 176 | 0.264 |

Source: Life in Kyrgyzstan Study, 2013. IDSC of IZA. Version 1.0, https://datasets.iza.org//dataset/124/life-in-kyrgyzstan-panel-study-2013, doi:10.15185/izadp.7055

## A3 Details of loan terms

The interest and the maximum amount of loan depend on the client's loan history and whether the client is eligible for special conditions. The share of Islamic (Sharia compliant) loans is 20 percent. These loans are issued without interest but are based on a fee to be paid in monthly installments alongside the loan repayment. In these cases, their monthly sum due for future months is instantly recalculated, lowering the amount of interest still to be paid (except for Islamic loans with a fixed fee). The share of female clients is 55 percent and the share of group loans, that is loans in which the whole group of individuals is liable for the repayment, is 27 percent. Most of the loans are issued for micro business purposes, but they also include some consumer loans. The default rate of the loans is very low for the microfinance market, below 1 percent.

The main determinant of the discount on the interest is the number of previous loans that the client has received and repaid without any delay (see Table B1e in the Appendix B that shows empirically how the interest rate depends on individual characteristics).

The Islamic loans can be only issued for payments for particular goods or services. They are also not offered in cash; instead the money is transferred to the merchant directly, while the client receives the good and becomes responsible for repayment of the price (plus a fee) in installments to the loan-issuing company.

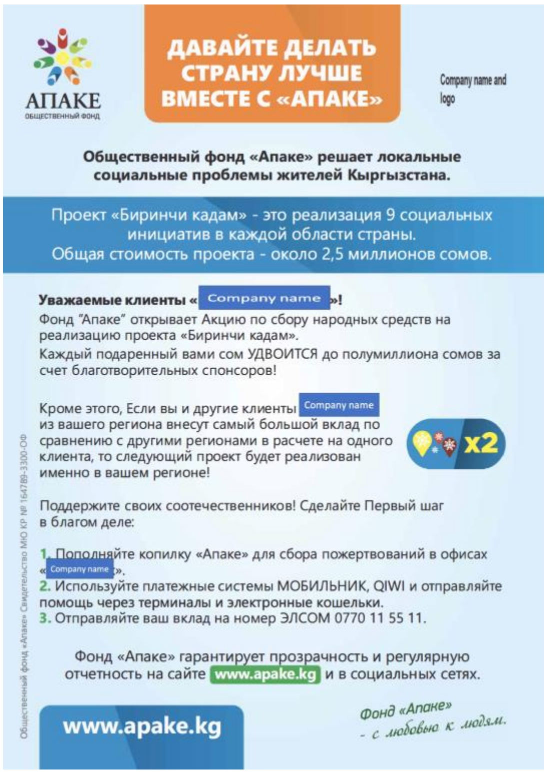
Typically, there are close relations between the credit specialists and the clients, as specialists decide whether to approve a loan, conditional on meeting formal requirements (like a clean loan history, Kyrgyz citizenship, availability of documents), and after an interview, visit at the workplace or at home, and potentially an interview with neighbors or colleagues of the client. Each credit specialist is free to reject the client or to acquire information over and above what is formally required. Specialists are motivated to give the loans to clients with a low risk of default, as the repayment rate is connected to the variable part of specialists' monthly salary.

## A4 Details of communication to clients

The communication to clients included flyers in Russian and Kyrgyz languages that were printed in A5 and were available to be taken from the office and posters in the office (Kyrgyz and Russian languages) placed on the cash desk and close to the donation box. Below, we present sample flyers and posters and explain treatment differences.

### A4.1 Flyers:

Figure A4a: Example of the flyer in Russian language for treatment matching and local.




In addition to the campaign slogans and the descriptions of means of payments, the flyer included treatment-specific descriptions:

"Birinchi Kadam" project involves the implementation of nine social initiatives in each region of the country. The total cost of the project is around 2.5 million KGS.[5]

"Apake fund" is starting a charitable campaign to collect donations to realize the project "Birinchi Kadam."

**Baseline (no matching, no local incentives):**

We already have a sponsor who donated half a million KGS for the project.

**Matching (no local incentives):**

Every som that you donate will be doubled thanks to a sponsor up to a total of half a million KGS.

**Local (no matching incentives):**

We already have a sponsor who donated half a million KGS for the project.

Moreover, if you and other clients of the "Company name" from your region will contribute the highest amount per client relative to the other regions, then the next project of the fund will be implemented in your region.

[5] Note that the realized costs we around 2 million KGS.

**Matching and local (example flyer on Figure A4a)**

Every som that you donate will be doubled thanks to a sponsor up to half a million KGS. Moreover, if you and other clients of the “Company name” from your region will contribute the highest amount per client relative to the other regions, then the next project of the fund will be implemented in your region.

A4.2 Posters:

In addition to the campaign slogans, the descriptions of means of payments, and the map of the projects, the poster included a treatment-specific description.

**Baseline**

A sponsor donated 500 000 KGS for this project.

**Matching**

An anonymous sponsor will double every som you donate* (*up to 500 000 KGS)

**Local**

A sponsor donated 500 000 KGS for this project.

If clients from your region donate the highest amount per client, the next project that will be funded from the charity will aim to help your region.

Figure A4b. Example of a poster for treatment matching and local.


**Matching plus local (example poster on Figure A4b)**

An anonymous sponsor will double every som you donate*. (*up to 500 000 KGS)

If clients from your region donate the highest amount per client, the next project that will be funded from the charity will aim to help your region.

## A5 Randomization

The randomization was conducted at the office level taking into account the following variables: number of credit specialists working for the office, average interest rate of all current loans, average current balance of all current loans, average cycle (number of loans issued to a current loan holder), average share of loan repayments delayed by 30 days, average experience of credit specialists in months, share of female credit specialists, average age of clients, share of female clients, share of clients married, share of clients of Kyrgyz nationality, region dummy 1–8, dummy equal to one if the current realized charitable project by the micro-lending company is in the same place as the office, share of clients of Uzbek nationality, and average number of children per client with the following weights: 10, 2, 2, 12, 3, 15, 2, 1, 1, 2, 1, 1, 4, 4, 4, 4, 4, 4, 4, 9, 4, 2. The choice of the variables and weights was motivated by the perceived importance of a particular variable, and in some cases, by the convergence properties of the algorithm. The client level data is as of 16.01.2018 but the specialists level data is as of the summer 2017. The sample has been divided block wise in 4 groups with earlier blocks being more homogenous than later ones. The total number of blocks is 26 (we dropped block 27 with only one office that was very different from others) making a total of 104 office level treatment units. We combined the groups 1–2 for the treatment without local benefits and 3–4 for the treatment with local benefits. Groups 1 and 3 were combined for the treatment with a lead donor and no matching and 2, 4 for the treatment with matching. Thus, group one was chosen to be a baseline, group two had the matching only, group 3 had the local benefits only, and group 4 had both matching and local benefits.

Office level data: In order to test the balance, we run a set of pairwise t-tests for comparisons between the treatments. Given that the blocked randomization was performed at the office level (104 offices), there is a good balance concerning all available variables as can be seen in Table A5a. In none of the tests $p<0.1$.

Credit specialist data: From a total of 492[6] we have individual level data on 370 credit specialists concerning their gender, region of origin, first language, age, experience in months etc. In what follows we check again balance of our treatment assignment based on the available characteristics using pairwise t-tests (see Table A5b). In 56 comparisons, we find some significant differences (two at $p<0.01$, two at $p<0.05$, and six at $p<0.1$), however, this approach is very conservative and might suffer from multiple testing problem. Therefore, in the next step, we run logit regressions with dependent variables being either the local treatment or the matching treatment and all available individual level variables as independent variables. Table A5c presents average marginal effects after logit. The robust standard errors are clustered at the office level. When looking at Table A5c, we can assess which individual characteristics of clients are correlated with the probability of being assigned to a particular treatment. There are no significant correlations at all. We conclude that we have achieved a reasonable balance at the specialists' level.

Individual level data: Given a large number of individuals (over 160,000), even small differences yield significant differences according to simple t-test comparisons. Therefore, in order to assess the balance at client`s level, we run logit regressions with dependent variables being either the local treatment or the matching treatment and all available individual level characteristics as independent variables. Table A5d presents average marginal effects after

[6] Excluding the dropped office.

logit. The robust standard errors are clustered at the office level. When looking at Table A5d, we can assess which individual characteristics of clients are correlated with the probability of being assigned to a particular treatment. We find one coefficient significant at p<0.01 and two coefficients significant at p<0.1 but the size of the marginal effects is rather small in all cases.

Table A5a: Balance at the office level

| Treatment | No local benefits | | Local benefits | | | Lead donor | | Matching | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | mean | standard error | mean | standard error | p-value | mean | standard error | mean | standard error | p-value |
| Number of specialists | 3.74 | 0.26 | 3.58 | 0.27 | 0.67 | 3.36 | 0.24 | 3.96 | 0.28 | 0.11 |
| Number of female specialists | 2.18 | 0.25 | 2.07 | 0.25 | 0.76 | 1.99 | 0.21 | 2.26 | 0.29 | 0.45 |
| Kyrgyz nationality dummy specialists | 0.88 | 0.04 | 0.89 | 0.04 | 0.85 | 0.87 | 0.04 | 0.91 | 0.04 | 0.48 |
| Uzbek nationality dummy specialists | 0.10 | 0.04 | 0.09 | 0.04 | 0.80 | 0.12 | 0.04 | 0.07 | 0.03 | 0.34 |
| Tadjik nationality dummy specialists | 0.01 | 0.01 | 0.01 | 0.01 | 0.75 | 0.00 | 0.00 | 0.02 | 0.01 | 0.17 |
| Other nationality dummy specialists | 0.01 | 0.01 | 0.01 | 0.01 | 0.92 | 0.01 | 0.01 | 0.00 | 0.00 | 0.45 |
| Speak Kyrgyz dummy specialists | 0.88 | 0.04 | 0.89 | 0.04 | 0.85 | 0.87 | 0.04 | 0.91 | 0.04 | 0.48 |
| Speak Uzbek dummy specialists | 0.10 | 0.04 | 0.09 | 0.04 | 0.80 | 0.12 | 0.04 | 0.07 | 0.03 | 0.34 |
| Speak Russian dummy specialists | 0.01 | 0.01 | 0.02 | 0.01 | 0.84 | 0.01 | 0.01 | 0.02 | 0.01 | 0.48 |
| Age of specialist | 30.63 | 0.56 | 31.04 | 0.70 | 0.66 | 30.74 | 0.60 | 30.92 | 0.67 | 0.84 |
| Experience in company in months | 38.46 | 2.58 | 35.96 | 2.66 | 0.50 | 35.66 | 2.28 | 38.76 | 2.92 | 0.40 |
| Number of clients per specialists | 359.08 | 11.36 | 352.49 | 11.62 | 0.69 | 353.44 | 11.28 | 358.37 | 11.71 | 0.76 |
| Portfolio at risk 30 days+ | 0.60 | 0.12 | 0.92 | 0.24 | 0.24 | 0.64 | 0.20 | 0.87 | 0.18 | 0.39 |
| Portfolio size KGS | 9561462.30 | 343694.31 | 9500509.93 | 318451.34 | 0.90 | 9453518.64 | 321921.28 | 9611972.26 | 342339.52 | 0.74 |
| Number of clients per office | 1696.12 | 151.04 | 1495.00 | 121.65 | 0.30 | 1459.24 | 124.68 | 1731.88 | 147.38 | 0.16 |
| Number of female clients | 980.40 | 87.49 | 868.16 | 76.34 | 0.34 | 829.92 | 74.66 | 1018.64 | 87.60 | 0.10 |
| Share of female clients | 0.57 | 0.01 | 0.58 | 0.01 | 0.74 | 0.57 | 0.01 | 0.58 | 0.01 | 0.71 |
| Dummy for marital status category: married | 0.70 | 0.01 | 0.69 | 0.02 | 0.51 | 0.69 | 0.02 | 0.69 | 0.01 | 0.99 |
| Dummy for marital status category: single | 0.13 | 0.01 | 0.13 | 0.01 | 0.84 | 0.13 | 0.01 | 0.13 | 0.01 | 0.77 |
| Interest | 31.05 | 0.26 | 31.30 | 0.33 | 0.54 | 31.42 | 0.30 | 30.93 | 0.28 | 0.24 |
| Kyrgyz nationality dummy clients | 0.79 | 0.04 | 0.83 | 0.04 | 0.45 | 0.78 | 0.04 | 0.84 | 0.03 | 0.32 |
| Uzbek nationality dummy clients | 0.17 | 0.04 | 0.13 | 0.03 | 0.44 | 0.17 | 0.04 | 0.12 | 0.03 | 0.37 |
| Tadjik nationality dummy clients | 0.01 | 0.00 | 0.02 | 0.01 | 0.47 | 0.01 | 0.01 | 0.01 | 0.01 | 0.79 |
| Russian nationality dummy clients | 0.01 | 0.00 | 0.01 | 0.00 | 0.58 | 0.01 | 0.00 | 0.01 | 0.00 | 0.49 |
| Other nationality dummy clients | 0.02 | 0.01 | 0.02 | 0.00 | 0.32 | 0.02 | 0.01 | 0.02 | 0.00 | 0.30 |
| Dummy for new clients (first loan in the company) | 0.38 | 0.01 | 0.37 | 0.01 | 0.68 | 0.37 | 0.01 | 0.37 | 0.01 | 0.91 |
| Age | 41.59 | 0.28 | 41.79 | 0.31 | 0.64 | 41.65 | 0.30 | 41.74 | 0.29 | 0.83 |
| Number of children | 1.61 | 0.04 | 1.67 | 0.05 | 0.34 | 1.63 | 0.04 | 1.65 | 0.05 | 0.75 |
| Family size | 4.38 | 0.06 | 4.31 | 0.07 | 0.47 | 4.36 | 0.06 | 4.32 | 0.06 | 0.68 |
| Current balance of the client's loan | 27077.33 | 481.98 | 27219.52 | 671.89 | 0.86 | 26803.92 | 652.24 | 27492.94 | 503.68 | 0.41 |
| Sum of loan when issued | 43301.47 | 777.96 | 43868.83 | 878.73 | 0.63 | 43430.35 | 801.53 | 43739.95 | 858.63 | 0.79 |
| Cycle | 2.87 | 0.09 | 2.92 | 0.08 | 0.70 | 2.82 | 0.07 | 2.98 | 0.09 | 0.17 |

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Share of delayed loans | 0.03 | 0.00 | 0.03 | 0.01 | 0.44 | 0.03 | 0.01 | 0.03 | 0.00 | 0.63 |
| Dummy for Bishkek region | 0.04 | 0.03 | 0.06 | 0.03 | 0.65 | 0.02 | 0.02 | 0.08 | 0.04 | 0.17 |
| Dummy for Osh city region | 0.04 | 0.03 | 0.04 | 0.03 | 0.94 | 0.04 | 0.03 | 0.04 | 0.03 | 0.94 |
| Dummy for Osh region | 0.26 | 0.06 | 0.22 | 0.06 | 0.63 | 0.26 | 0.06 | 0.22 | 0.06 | 0.68 |
| Dummy for Djalal-Abad region | 0.18 | 0.05 | 0.24 | 0.06 | 0.47 | 0.26 | 0.06 | 0.16 | 0.05 | 0.22 |
| Dummy for Chuy region | 0.12 | 0.05 | 0.06 | 0.03 | 0.30 | 0.10 | 0.04 | 0.08 | 0.04 | 0.73 |
| Dummy for Issyk-Kul region | 0.10 | 0.04 | 0.14 | 0.05 | 0.54 | 0.08 | 0.04 | 0.16 | 0.05 | 0.22 |
| Dummy for Batken region | 0.16 | 0.05 | 0.10 | 0.04 | 0.36 | 0.12 | 0.05 | 0.14 | 0.05 | 0.79 |
| Dummy for Naryn region | 0.06 | 0.03 | 0.08 | 0.04 | 0.70 | 0.06 | 0.03 | 0.08 | 0.04 | 0.70 |
| Dummy for Talas region | 0.04 | 0.03 | 0.06 | 0.03 | 0.65 | 0.06 | 0.03 | 0.04 | 0.03 | 0.65 |
| Share of female specialists | 0.56 | 0.05 | 0.55 | 0.05 | 0.91 | 0.58 | 0.05 | 0.54 | 0.05 | 0.56 |
| Dummy for project in the same locality | 0.10 | 0.04 | 0.08 | 0.04 | 0.72 | 0.10 | 0.04 | 0.08 | 0.04 | 0.73 |

Note: The base for all variables concerning credit specialist and clients are means at the office level

Table A5b: Balance at the credit specialists' level

| Treatment | No local benefits | | Local benefits | | | Lead donor | | Matching | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | mean | standard error | mean | standard error | p-value | mean | standard error | mean | standard error | p-value |
| Dummy for Bishkek region | 0.06 | 0.02 | 0.08 | 0.02 | 0.34 | 0.03 | 0.01 | 0.11 | 0.02 | 0.00 |
| Dummy for Osh city region | 0.05 | 0.02 | 0.03 | 0.01 | 0.38 | 0.04 | 0.02 | 0.04 | 0.01 | 0.67 |
| Dummy for Osh region | 0.29 | 0.03 | 0.22 | 0.03 | 0.13 | 0.29 | 0.03 | 0.23 | 0.03 | 0.17 |
| Dummy for Djalal-Abad region | 0.14 | 0.03 | 0.29 | 0.03 | 0.00 | 0.27 | 0.03 | 0.16 | 0.03 | 0.01 |
| Dummy for Chuy region | 0.10 | 0.02 | 0.06 | 0.02 | 0.11 | 0.08 | 0.02 | 0.08 | 0.02 | 0.88 |
| Dummy for Issyk-Kul region | 0.08 | 0.02 | 0.14 | 0.03 | 0.09 | 0.06 | 0.02 | 0.15 | 0.03 | 0.01 |
| Dummy for Batken region | 0.15 | 0.03 | 0.08 | 0.02 | 0.05 | 0.11 | 0.02 | 0.12 | 0.02 | 0.82 |
| Dummy for Naryn region | 0.09 | 0.02 | 0.05 | 0.02 | 0.19 | 0.07 | 0.02 | 0.07 | 0.02 | 0.82 |
| Dummy for Talas region | 0.05 | 0.02 | 0.05 | 0.02 | 0.89 | 0.05 | 0.02 | 0.05 | 0.01 | 0.94 |
| Kyrgyz nationality dummy specialist | 0.85 | 0.03 | 0.89 | 0.02 | 0.25 | 0.87 | 0.03 | 0.87 | 0.02 | 0.82 |
| Uzbek nationality dummy specialist | 0.13 | 0.02 | 0.09 | 0.02 | 0.21 | 0.11 | 0.02 | 0.11 | 0.02 | 0.99 |
| Tadjik nationality dummy specialist | 0.01 | 0.01 | 0.01 | 0.01 | 0.53 | 0.00 | 0.00 | 0.02 | 0.01 | 0.08 |
| Other nationality dummy specialist | 0.01 | 0.01 | 0.01 | 0.01 | 0.60 | 0.01 | 0.01 | 0.01 | 0.01 | 0.49 |
| Speak Kyrgyz dummy specialist | 0.85 | 0.03 | 0.89 | 0.02 | 0.25 | 0.87 | 0.03 | 0.87 | 0.02 | 0.82 |
| Speak Uzbek dummy specialist | 0.13 | 0.02 | 0.09 | 0.02 | 0.21 | 0.11 | 0.02 | 0.11 | 0.02 | 0.99 |
| Speak Russian dummy specialist | 0.02 | 0.01 | 0.02 | 0.01 | 0.93 | 0.01 | 0.01 | 0.02 | 0.01 | 0.52 |
| Female | 0.58 | 0.04 | 0.59 | 0.04 | 0.95 | 0.59 | 0.04 | 0.58 | 0.04 | 0.77 |
| Age | 31.51 | 0.50 | 31.14 | 0.59 | 0.63 | 30.78 | 0.53 | 31.80 | 0.55 | 0.18 |
| Experience in company in months | 41.90 | 2.09 | 38.85 | 2.24 | 0.32 | 37.25 | 2.01 | 43.17 | 2.24 | 0.05 |
| Number of clients | 364.43 | 13.21 | 350.53 | 13.17 | 0.46 | 355.99 | 13.89 | 359.23 | 12.61 | 0.86 |
| Portfolio at risk 30 days+ | 0.60 | 0.09 | 1.00 | 0.22 | 0.08 | 0.71 | 0.15 | 0.86 | 0.16 | 0.50 |
| Portfolio size KGS | 9757832 | 358254 | 9543717 | 362306 | 0.67 | 9584149 | 381037 | 9715301 | 342537 | 0.80 |
| Dummy for project in the same locality | 0.11 | 0.02 | 0.06 | 0.02 | 0.08 | 0.09 | 0.02 | 0.08 | 0.02 | 0.67 |

Table A5c: Credit specialist's characteristics and the probability of assignment to a treatment.

| Dependent variable | Dummy treatment local | Dummy treatment matching |
|---|---|---|
| Dummy for Bishkek region | 0.078 | 0.352 |
| | (0.340) | (0.332) |
| Dummy for Osh city region | -0.136 | -0.085 |
| | (0.363) | (0.333) |
| Dummy for Osh region | -0.036 | -0.082 |
| | (0.257) | (0.229) |
| Dummy for Djalal-Abad region | 0.184 | -0.131 |
| | (0.254) | (0.233) |
| Dummy for Chuy region | -0.146 | 0.052 |
| | (0.291) | (0.257) |
| Dummy for Issyk-Kul region | 0.135 | 0.219 |
| | (0.268) | (0.242) |
| Dummy for Batken region | -0.097 | 0.005 |
| | (0.271) | (0.256) |
| Dummy for Naryn region | -0.118 | 0.036 |
| | (0.316) | (0.298) |
| Kyrgyz nationality dummy specialist | -0.124 | -0.051 |
| | (0.237) | (0.252) |
| Uzbek nationality dummy specialist | -0.166 | 0.041 |
| | (0.256) | (0.270) |
| Female | 0.033 | -0.050 |
| | (0.060) | (0.061) |
| Age | 0.001 | 0.000 |
| | (0.004) | (0.004) |
| Experience in company in months | -0.002 | 0.001 |
| | (0.001) | (0.001) |
| Number of clients | -0.000 | 0.000 |
| | (0.001) | (0.001) |
| Portfolio at risk 30 days+ | 0.012 | 0.012 |
| | (0.016) | (0.016) |
| Portfolio size KGS | 0.000 | -0.000 |
| | (0.000) | (0.000) |
| Dummy for project in the same locality | -0.132 | -0.067 |
| | (0.165) | (0.163) |
| Observations | 365 | 365 |
| Pseudo $R^2$ | 0.062 | 0.062 |

Average marginal effects after logit, Robust standard errors clustered at office level in parentheses;
$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

Table A5d: Individual characteristics of clients and the probability of assignment to a particular treatment.

| Dependent variable | Dummy treatment local | Dummy treatment matching |
|---|---|---|
| Cycle | -0.001 | 0.012*** |
| | (0.005) | (0.005) |
| Issuing fee | 0.004 | -0.003 |
| | (0.006) | (0.006) |
| Interest rate | 0.000 | -0.002* |
| | (0.001) | (0.001) |
| Balance left to be paid | 0.000 | -0.000 |
| | (0.000) | (0.000) |
| Age | 0.000 | -0.000 |
| | (0.001) | (0.001) |
| Dummy for Kyrgyz nationality | 0.052 | 0.092 |
| | (0.082) | (0.085) |
| Dummy for Uzbek nationality | -0.020 | 0.056 |
| | (0.117) | (0.119) |
| Dummy for Tadjik nationality | 0.213 | 0.179 |
| | (0.210) | (0.216) |
| Dummy for Russian nationality | 0.004 | 0.057 |
| | (0.083) | (0.087) |
| Dummy for new client | -0.006 | 0.008 |
| | (0.019) | (0.020) |
| Number of children | 0.013 | 0.016 |
| | (0.011) | (0.012) |
| Family size | -0.004 | -0.009 |
| | (0.006) | (0.007) |
| Female dummy | -0.007 | 0.005 |
| | (0.009) | (0.009) |
| Dummy for marital status category: married | -0.036* | -0.017 |
| | (0.020) | (0.022) |
| Dummy for marital status category: single | -0.025 | -0.002 |
| | (0.029) | (0.032) |
| Dummy for project in the same locality | -0.141 | -0.080 |
| | (0.176) | (0.181) |
| Observations | 161759 | 161759 |
| Pseudo $R^2$ | 0.009 | 0.008 |

Notes: Average marginal effects after logit, Robust standard errors clustered at office level in parentheses; $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

# A6 Power calculations and multiplicity hypothesis testing

## POWER CALCULATIONS

We calculated power in our experiment using rdpower package for stata. Given our cluster randomization, we first need an estimate of intra cluster correlation (ICC). We are not aware of any study in a similar setting that could give us a valid estimate of ICC. Most studies on charitable giving rely on simple randomization and are conducted in western countries with middle-income subjects. In order to obtain best guess we computed ICC in our sample with respect to the current balance (current debt of a client) and total current loan issued per client. ICC based on current balance equals to 0.02 while ICC based on loan issued equals to 0.04. Assuming ICC=0.02, with 52 clusters and (over) 1500 individuals per cluster, we have enough power (>0.8) to detect a standardized effect size of at least 0.1. While assuming ICC=0.04, there is enough power to detect a standardized effect size of at least 0.12. Note however, that there is additional efficiency gain due to blocked randomization (see below) and potential inclusion of covariates when estimating the causal effect.

### NO MULTIPLICITY HYPOTHESIS TESTING CORRECTIONS FOR THE MAIN HYPOTHESES

There appears to be some disagreement among statisticians on whether and when corrections for MHT should be applied. While some call for uniform use of those, other criticize that they lead to overcorrection. We follow the more moderate view like in Schulz & Grimes (2005) and abstain from corrections in case of testing our main hypotheses. Here are the reasons:

(i) Our main hypotheses are guided by literature and theory. In other words, we are testing theory and not some random outcomes.

(ii) The number of tests is clearly limited by (i) and not large.

(iii) The corrections, like Bonferroni, lead to a redefinition of a hypothesis being tested to "all differences are zero versus at least one difference exists." This is not of interest to us.

(iv) Our three outcomes, response rate, positive contribution, and return depend linearly on each other (each one is a composite of two other), that is, the number of tests is less than it appears on first sight.

## A7 Additional hypotheses on specialist level

Given the specific implementation of the campaign, we formulated additional hypotheses on specialist level:

**S1 There are no treatment differences in shares of clients informed about the fundraising campaign.**

Motivation: Given the incentive structure provided to credit specialists to spread the information about the campaign, we expect no treatment effect on credit specialists' motivation to ask clients for donations, which we measure with the shares of clients informed measured by a survey.

**S2 Specialists with higher shares of informed clients raise more funds.**

Motivation: Since the shares of clients informed may serve as a proxy for specialist motivation, we want to see whether this measure is, at the same time, a good predictor for donations. While a direct link seems obvious, we will also perform an indirect test at the level of clients by regressing the rate of informed other clients of the same specialists on individual giving behavior.

## A8 Derivation of the crowding out condition

Crowding out occurs whenever the local elasticity of demand for the charitable good at the optimally chosen bundle for $p$ = 1 is greater than minus 1, that is, when demand is not too price elastic, $e_{x,p} > -1$.

Re-writing the first-order condition as $F(x,p) = -pu_y + u_x = 0$ we can employ the implicit function theorem to obtain

$$\frac{\partial x}{\partial p} = -\frac{\frac{\partial F}{\partial p}}{\frac{\partial F}{\partial x}} = -\frac{-u_y}{-pu_{xy}+u_{xx}} \ .$$

Inserting this into equation (2) in the main text from Section 5 we get the elasticity as

$$e_{x,p} = \frac{\frac{u_y}{u_{xx} - pu_{xy}}}{x/p}.$$

The goal is to find conditions for crowding out relative to a baseline where $p = 1$ so that we can simplify

$$e_{x,p} = \frac{\frac{u_y}{u_{xx} - u_{xy}}}{x} = \frac{u_y}{x(u_{xx} - u_{xy})}.$$

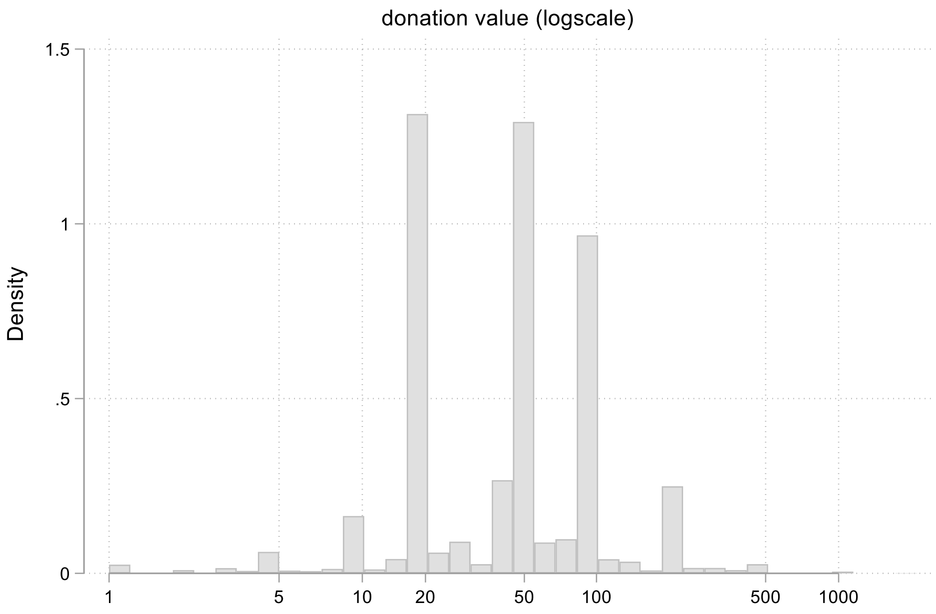
Hence, for crowding out to occur we need $0 > \frac{u_y}{x(u_{xx}-u_{xy})} > -1$. Now note that the first part of the inequality is satisfied whenever $u_{xx} < u_{xy}$ which holds as long as *x* and *y* are not perfect substitutes. For the right-hand part we obtain

$u_{xy} > \frac{u_y}{x} + u_{xx}$, equation (4) from Section 5.

# Appendix B.

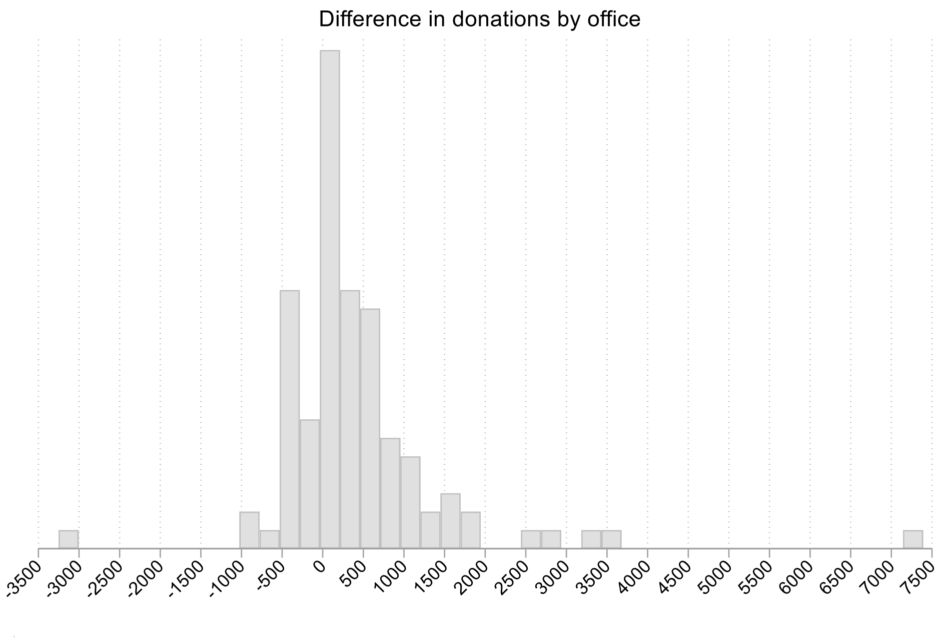
## B1 Additional figures and tables

Figure B1a: Histogram of donation values



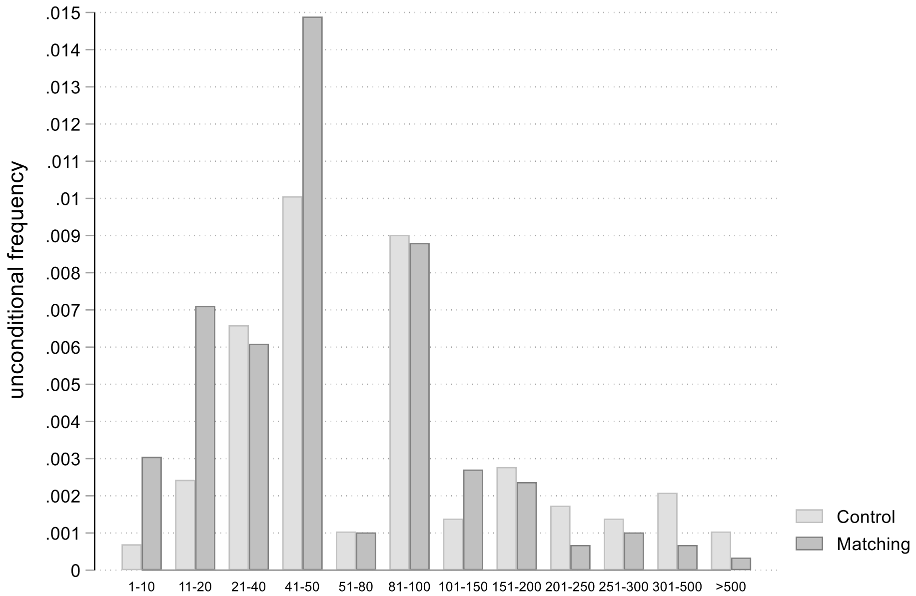

Notes: X-axis presents the bins of the donation sums in KGS. Y-axis presents density of the distribution.

Figure B1b: Differences in reported donation relative to donations in the box by office



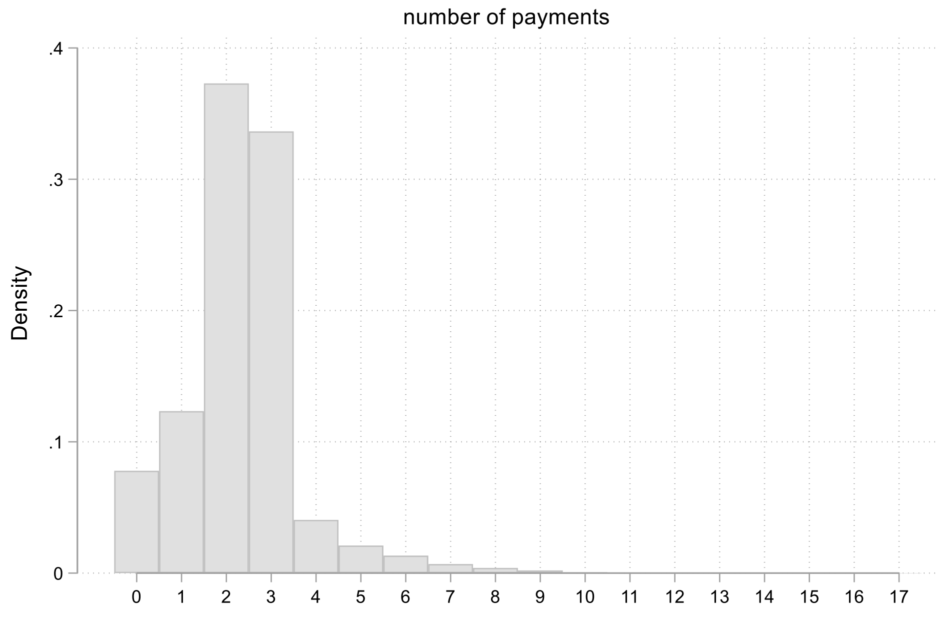

Notes: X-axis presents the bins of the donation sums in KGS. Y-axis presents density of the distribution.

Figure B1c: Distribution of gift levels in the Munich sample of opera goers (Huck and Rasul 2011)




Notes: X-axis presents the bins of the donation sums in Euro. Y-axis presents density of the distribution.

Figure B1d: Histogram of the number of payments by clients in the period under study




Note: X-axis presents the bins of the number of repayments done in the period of the experiment. Y-axis presents density of the distribution.

Table B1a: Determinants of the interest rate

| | Interest rate | Interest rate |
|---|---|---|
| | I | II |
| Sum borrowed in KGS | -0.000*** | |
| | (0.000) | |
| Cycle | -0.437*** | |
| | (0.023) | |
| Term of loan in months | -0.160*** | |
| | (0.013) | |
| Delayed sum | 0.297 | |
| | (0.230) | |
| Product category fixed effects | yes | |
| Income proxy | -0.077* | 0.669*** |
| | (0.041) | (0.135) |
| Dummy for urban area | 0.140* | 0.373 |
| | (0.079) | (0.325) |
| Age | -0.013*** | 0.013** |
| | (0.002) | (0.006) |
| Female dummy | -0.049 | -0.538*** |
| | (0.034) | (0.086) |
| Education category: unknown | -1.418 | 4.875 |
| | (3.716) | (5.362) |
| Education category: less than high school | 1.069*** | 1.380** |
| | (0.394) | (0.609) |
| Education category: high school | 0.850** | 0.559 |
| | (0.338) | (0.360) |
| Education category: unfinished university | 0.671* | 0.569* |
| | (0.348) | (0.332) |
| Education category: university degree | 0.354 | -0.168 |
| | (0.347) | (0.334) |
| Occupation category: employee with salary | -0.261* | 2.148*** |
| | (0.134) | (0.489) |
| Occupation category: agriculture self employed | -0.130* | 3.381*** |
| | (0.068) | (0.503) |
| Occupation category: trade self employed | 0.021 | 2.858*** |
| | (0.092) | (0.425) |
| Occupation category: service self employed | 0.208*** | 3.340*** |
| | (0.065) | (0.430) |
| Occupation category: production self employed | 0.082 | 3.074*** |
| | (0.176) | (0.503) |
| Marital status category: Single | -2.647 | 2.212 |
| | (3.456) | (5.901) |
| Marital status category: Married | -2.962 | 1.979 |
| | (3.451) | (5.907) |
| Marital status category: Divorced | -2.780 | 2.619 |
| | (3.461) | (5.911) |
| Marital status category: Widow | -2.801 | 2.031 |
| | (3.449) | (5.914) |
| Constant | 39.313*** | 20.225*** |
| | (3.531) | (6.199) |
| Observations | 153900 | 153900 |
| $R^2$ | 0.672 | 0.035 |
| Adjusted $R^2$ | 0.672 | 0.034 |

Notes: OLS; Robust errors clustered at the office level; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B1b: Treatment effect on the extensive and the intensive margin by income groups

**Panel A: Treatment effect on the intensive margin (log positive donation)**

| Income threshold | | KGS 15,000 | KGS 20,000 | KGS 25,000 | KGS 30,000 | KGS 35,000 | KGS 40,000 |
|---|---|---|---|---|---|---|---|
| Below income threshold | Coefficient for treatment matching | -0.05 | -0.06 | -0.06 | -0.05 | -0.04 | -0.04 |
| | Std. Error | 0.10 | 0.10 | 0.10 | 0.10 | 0.09 | 0.09 |
| Equal or higher income threshold | Coefficient for treatment matching | -0.08 | -0.08 | -0.07 | -0.12 | -0.18 | -0.21* |
| | Std. Error | 0.09 | 0.10 | 0.09 | 0.10 | 0.11 | 0.12 |
| | **Difference between coefficients** | **-0.03** | **-0.02** | **-0.01** | **-0.07** | **-0.14** | **-0.17** |

**Panel B: Treatment effect on the extensive margin (donation dummy)**

| Income threshold | | KGS 15,000 | KGS 20,000 | KGS 25,000 | KGS 30,000 | KGS 35,000 | KGS 40,000 |
|---|---|---|---|---|---|---|---|
| Below income threshold | Coefficient for treatment matching | 0.009 | 0.011* | 0.012** | 0.012** | 0.012** | 0.012** |
| | Std. Error | 0.006 | 0.006 | 0.006 | 0.006 | 0.006 | 0.005 |
| Equal or higher income threshold | Coefficient for treatment matching | 0.012** | 0.013** | 0.012** | 0.013*** | 0.012*** | 0.011*** |
| | Std. Error | 0.005 | 0.005 | 0.005 | 0.005 | 0.004 | 0.004 |
| | **Difference between coefficients** | **-0.003** | **-0.002** | **0.000** | **-0.001** | **0.00** | **0.001** |

Notes: Sample restricted to identified donations and to the clients with the income below or above respective the threshold; Income thresholds are chosen in increments of 5,000 KGS (approx. $75) such that there are at least 1,000 observations in each category (higher or lower than the threshold); Controls: treatment local dummy.

## B2. Is there a preference for local charitable output?

One of our two main research goals was to test the presence of preferences for local charitable output, keeping the charitable organization constant. We test this through a treatment that decreases (in expectation) the distance to future charitable output. More pronounced preferences for a “close” output should be expressed through a higher amount of donations in the local treatment. In our regressions, Tables 4–6 in the main text, although positive, the coefficient on treatment dummy is only significant for the intensive margin in one of the specifications (Table 5, Column IV). There is no effect on the extensive margin neither an overall effect suggesting that there might be no preference for more local charitable output.

In order to analyze the robustness of this null effect, we explore whether there are any heterogeneous treatment effects between clients of offices that are more or less centrally located within the region. The local treatment could have more appeal to clients who live more centrally as for them the expected distance to the additionally implemented project in case that their region donates the highest average per client should be the lowest.

Clients who are living further away from the center of the region, i.e., closer to the boarders, might have a concern that the next project will be realized far away from their location, though still within the region, and this would mean that local incentives are less appealing for such clients. Even if those, who are close to the border could profit from a projects implemented in the neighboring region, they cannot influence the probability of its realization. Therefore, we define a dummy variable "center" which is equal to 1 for offices which are located in a 60 km radius from the geographical center of each region and interact it with the local benefits treatment dummy. The results are presented in Table B2a. There are no significant effects on any of the outcome variables. This means that our main results are robust to the above concern of centrality.

Table B2a. Heterogeneous treatment effect with respect to location within region

| | Response rate | Response rate | Positive donation (log) | Positive donation (log) | Log donation +1 (including zeros) | Log donation +1 (including zeros) |
|---|---|---|---|---|---|---|
| Treatment matching | 0.013** | 0.013*** | -0.040 | -0.019 | 0.049** | 0.049*** |
| | (0.006) | (0.004) | (0.085) | (0.055) | (0.022) | (0.017) |
| Treatment local | 0.013 | 0.000 | -0.073 | 0.188 | 0.047 | 0.004 |
| | (0.013) | (0.009) | (0.251) | (0.131) | (0.047) | (0.034) |
| Treatment local*center of region | -0.015 | -0.004 | 0.172 | -0.096 | -0.049 | -0.019 |
| | (0.015) | (0.011) | (0.270) | (0.150) | (0.053) | (0.044) |
| Center of region dummy | 0.006 | -0.001 | 0.041 | 0.150* | 0.025 | -0.003 |
| | (0.008) | (0.007) | (0.108) | (0.080) | (0.029) | (0.027) |
| Observations | 185845 | 149969 | 7027 | 5148 | 185845 | 149969 |
| $R^2$ | 0.002 | 0.018 | 0.008 | 0.136 | 0.002 | 0.016 |
| Adjusted $R^2$ | 0.001 | 0.018 | 0.007 | 0.125 | 0.001 | 0.016 |
| Controls | - | yes | - | yes | - | yes |
| Sample | full | conservative + excl. unidentified don. | full | conservative + excl. unidentified don. | full | conservative + excl. unidentified don. |

Notes: OLS; Robust errors clustered at the office level; Conservative sample excludes incomplete blocks from the randomization stage and new offices; Sample full with controls is identical to the one excluding unidentified donors since no controls available; Controls include: dummies for the randomization-level block, client level controls including gender of the client, age of the client, the number of previous loans taken in the company, education level dummies, marital status dummies, occupation type dummies, dummies for taking up and closing the loan in the experiment period, self-reported income, interest rate of the loan, the sum of due repayment delayed for more than 30 days, and the term of the loan in months; office and region level controls including dummy for urban areas, region dummy, number of clients per office; specialist level controls including client number, portfolio size, age, number of children, education dummies, experience in months, family size, female dummy, material status dummies, and nationality dummies. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Alternatively, we can look at the correlation of the donation to the proximity of the currently implemented projects (independent of the treatments). The reason is that the clients cannot affect the distance to the currently implemented projects. All posters showed a map with location of the current projects such that the clients in each treatment knew where they are being implemented. We use geolocation of all offices and projects and calculate the direct distance from each office to each of the projects. We use two approaches: distance to the closest project and the distance to the project within of the respective region.

First, we define a variable distance to the closest project, ignoring the borders between regions. We find no significant correlation between the proximity of the closest project and any of the outcomes. The treatment differences remain the same. The results of the estimation are presented in Table B2b.

Table B2b. Correlation of distance to the closest project with main outcome variables

| | Response rate | Response rate | Positive donation (log) | Positive donation (log) | Log donation +1 (including zeros) | Log donation +1 (including zeros) |
|---|---|---|---|---|---|---|
| Treatment matching | 0.013** | 0.013*** | -0.056 | -0.018 | 0.048** | 0.050*** |
| | (0.006) | (0.004) | (0.088) | (0.059) | (0.021) | (0.016) |
| Treatment local | 0.003 | -0.003 | 0.072 | 0.116** | 0.012 | -0.009 |
| | (0.006) | (0.005) | (0.084) | (0.058) | (0.021) | (0.018) |
| Distance to closest project | 0.000 | 0.000 | -0.003 | 0.001 | 0.000 | 0.001 |
| | (0.000) | (0.000) | (0.002) | (0.002) | (0.000) | (0.000) |
| Observations | 185730 | 149969 | 7025 | 5148 | 185730 | 149969 |
| $R^2$ | 0.002 | 0.018 | 0.018 | 0.135 | 0.001 | 0.016 |
| Adjusted $R^2$ | 0.002 | 0.018 | 0.017 | 0.124 | 0.001 | 0.015 |
| Controls | - | yes | - | yes | - | yes |
| Sample | full | conservative + excl. unidentified don. | full | conservative + excl. unidentified don. | full | conservative + excl. unidentified don. |

Notes: see notes to Table B2a.

Table B2c. Correlation of distance to the local project with main outcome variables

| | Response rate | Response rate | Positive donation (log) | Positive donation (log) | Log donation +1 (including zeros) | Log donation +1 (including zeros) |
|---|---|---|---|---|---|---|
| Treatment matching | 0.012** | 0.013*** | -0.051 | -0.054 | 0.044** | 0.048*** |
| | (0.006) | (0.004) | (0.091) | (0.057) | (0.021) | (0.016) |
| Treatment local | 0.003 | -0.002 | 0.039 | 0.100 | 0.012 | -0.007 |
| | (0.006) | (0.005) | (0.106) | (0.054) | (0.021) | (0.018) |
| Distance to local project | 0.000 | 0.000 | -0.001 | -0.002*** | 0.000* | 0.000 |
| | (0.000) | (0.000) | (0.001) | (0.001) | (0.000) | (0.000) |
| Observations | 185730 | 149969 | 7025 | 5148 | 185730 | 149969 |
| R2 | 0.001 | 0.018 | 0.001 | 0.001 | 0.001 | 0.001 |
| Adjusted R2 | 0.001 | 0.018 | 0.001 | 0.001 | 0.001 | 0.001 |
| Controls | - | yes | - | yes | - | yes |
| Sample | full | conservative + excl. unidentified don. | full | conservative + excl. unidentified don. | full | conservative + excl. unidentified don. |

Notes: see notes to Table B2a.

Second, we define a variable capturing the distance to the project within one's region. Again, we find no significant correlation between the proximity of the project within the region in either specification. The treatment differences remain the same. The results of the estimation are presented in Table B2c. Thus, we conclude that there is indeed no preference for local charitable output in our sample.

## B3 Robustness of main results

Table B3a. Summary statistics by four groups separately

| | Lead donor, non Local | Matching only | Local only | Both |
|---|---|---|---|---|
| Percent of clients who donated | 3.3% | 3.8% | 2.8% | 4.9% |
| Average positive donation, KGS | 59.1 | 63.5 | 71.1 | 61.6 |
| Average donation per client, KGS | 1.98 | 2.43 | 2.02 | 3.05 |
| Average donation per office, KGS | 3,662.8 | 4,984.0 | 3,348.3 | 6,048.6 |
| Share of unidentified donations | 7.2% | 9.3% | 10.5% | 8.1% |
| Number of clients | 47,890 | 49,097 | 41,363 | 47,495 |

Notes: Full sample. Average donation per office is based on total sum of donations in the donation boxes and includes unidentified donations.

Table B3b. Treatment effects on total donations

| Dependent variable: donation amount | | | | |
|---|---|---|---|---|
| | I | II | III | V |
| treatment matching | 0.727** | 0.659* | 0.712* | 0.756*** |
| | (0.353) | (0.334) | (0.390) | (0.265) |
| treatment local | 0.340 | 0.300 | 0.408 | 0.098 |
| | (0.357) | (0.338) | (0.396) | (0.270) |
| Observations | 185845 | 185239 | 152319 | 149969 |
| $R^2$ | 0.001 | 0.001 | 0.001 | 0.006 |
| Adjusted $R^2$ | 0.001 | 0.001 | 0.001 | 0.006 |
| controls | - | - | - | yes |
| sample | full | excl. unidentified don. | conservative + excl. unidentified don. | conservative + excl. unidentified don. |

Notes: OLS; Robust errors clustered at the office level; Conservative sample excludes incomplete blocks of four from the randomization stage and new offices; Sample full with controls is identical to the one excluding unidentified donors since no controls are available for those observations; Controls include: dummies for the randomization-level block, client level controls including gender of the client, age of the client, the number of previous loans taken in the company, education level dummies, marital status dummies, occupation type dummies, dummies for taking up and closing the loan in the experiment period, self-reported income, interest rate of the loan, the sum of due repayment delayed for more than 30 days, and the term of the loan in months; office and region level controls including dummy for urban areas, region dummy, number of clients per office; specialist level controls including client number, portfolio size, age, number of children, education dummies, experience in months, family size, female dummy, material status dummies, and nationality dummies. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## B4 Price elasticity of charitable giving—additional analysis

With 1:1 matching, the price of a one unit donation received by the charity is only half of the unit. Matching would be optimal for price elasticities below -1.[7] Relying on field experiments, Karlan and List (2007) reported a price elasticity of -0.225 while Huck and Rasul (2011) estimate elasticity values closer to -1. However, a review of the methods used to estimate the price elasticity of demand for charitable goods in different papers reveals important differences such that the values are not directly comparable. The most common approach estimates the price elasticity in a log-log specification such that nondonors are automatically dropped (for example, Eckel and Grossman 2008). This is a valid approach only if the price reduction does not induce additional subjects to donate, otherwise one needs to adjust for that.[8] Also note that a log-log specification assumes constant elasticity. Karlan and List (2007) calculate the checkbook (point) elasticity using sample averages: the average donation per letter excluding the match. Note that this elasticity assumes linearity and is only appropriate for small changes in price (that is, it does not appear to be perfect for a price reduction of 50 percent). Moreover, their comparison treatment is a control without a lead gift, that is, the difference between the matching and the control is twofold: there is signaling through the presence of a lead donor (as theoretically proposed by Vesterlund 2003) and a price reduction.

We modify the approach by Karlan and List (2007) such that we include the match amount into the price elasticity formula as we are interested in the total donation received by the charity and we calculate the arc elasticity which is more appropriate for large price changes.[9] The arc elasticity is given by $\frac{d^{r,M}-d^{r,LD}}{p^M-p^{LD}}\frac{p^M+p^{LD}}{d^{r,M}+d^{r,LD}}$, with $d^r$ being donation including the match, $p$ denoting the price, and the superscripts $M$ and $LD$ signifying the matching and lead donor

[7] The literature on the price elasticity of charitable giving started by studying the effectiveness of tax incentives with the price of giving being equal to one minus the marginal tax rate (see, for example, Adena (2022) for a review of this literature). This literature uses data from tax reports, although there is an inherent problem that the marginal tax rate is (usually) related to income and other personal characteristics that affect donation behavior as well. Therefore, the estimates strongly rely on the estimation procedure and, thus, on the validity of various assumptions.
The advent of field experiments provided a new direction in the literature on the price elasticity of giving. In such experiments purely exogenous variations can be studied, for example, by varying the matching rate.

[8] For example, one could take log(donations+1) as the outcome variable and include, additionally to all donors, a share of nondonors in the lead donor treatment such that the shares of individuals included in both treatments are equal. Note that inclusion of all nondonors leads to an inclusion of many never-compliers, and the more never-compliers included, the lower the estimates (in absolute terms).

[9] The point elasticity is defined for marginal changes in price at a starting price level while the arc elasticity measures it at a midpoint between two price levels. When using point elasticity formula for a discrete change in price there are two possible and very different values, one at the price with matching and one without.

treatments respectively. The value of the arc elasticity can be calculated both, at the sample averages or in level-level regression, and, importantly, it does not depend on the inclusion or exclusion of subjects who never donate. Moreover, we can simply repeat this calculation for other studies and compare the price elasticities between different populations. Table B4a, Column VIII shows the relevant results. The price elasticity is the largest (in absolute terms) in our population with -1.393.[10] Our calculation for Karlan and List's (2007) experiment is also relatively large (in absolute terms), but it is based on a comparison without the signaling value of a lead donor, and thus is expected to be lower for a control with a lead donor. In the remaining studies the price elasticity is above -1 except in Adena and Huck (2017b).[11]

Table B4a: Matching-price (arc) elasticity of charitable giving in different field experiments

| | Comparison treatment | Sample | Donors | Share of donors | Price | Donation per letter/customer, excluding match | Donation per letter/customer, including match | Price elasticity |
|---|---|---|---|---|---|---|---|---|
| | I | II | III | IV | V | VI | VII | VIII |
| Karlan List 2007 | pure control | 16,687 | 300 | 0.018 | 1 | 0.81 | 0.81 | |
| | | 11,133 | 234 | 0.021 | 0.5 | 0.94 | 1.88 | -1.193 |
| Rondeau List 2008 | lead donor | 750 | 37 | 0.049 | 1 | 2.16 | 2.16 | |
| | | 750 | 36 | 0.048 | 0.5 | 1.65 | 3.29 | -0.623 |
| Huck Rasul 2011 | lead donor | 3770 | 132 | 0.035 | 1 | 4.62 | 4.62 | |
| | | 3718 | 155 | 0.042 | 0.5 | 3.85 | 7.70 | -0.750 |
| Gneezy, Keenan, and Gneezy 2014 | lead donor | 10000 | 475 | 0.048 | 1 | 1.32 | 1.32 | |
| | | 10000 | 441 | 0.044 | 0.5 | 1.22 | 2.44 | -0.893 |
| Adena Huck 2017 | lead donor | 6143 | 93 | 0.015 | 1 | 1.84 | 1.84 | |
| | | 6143 | 129 | 0.021 | 0.5 | 2.30 | 4.59 | -1.287 |
| Our paper | lead donor | 89,253 | 2,787 | 0.031 | 1 | 2.00 | 2.00 | |
| | | 96,592 | 4,240 | 0.044 | 0.5 | 2.74 | 5.48 | -1.393 |

Notes: We only report the treatments with the price of 1 and 0.5, and take lead donor as a control treatment if available. Price elasticity including the match, see the formula in the text. The numbers provided in the table are based on summary statistics and information provided in the respective papers.

Next, for comparison reasons, we also report the results for a log-log specification. It shows that our subjects are highly price elastic, with a (constant) price elasticity of around -2.5 (that is statistically different from -1). However, unlike previous studies, we do not use all data, but only account for potential compliers while getting rid of potential never-takers. This means that we include in our estimation equal shares of clients from both treatments: 4.4 percent of customers from each treatment which includes all donors and, in the lead donor treatment, 1.3

[10] Analogue level-level regression without controls leads to an elasticity of -1.35, significantly different from -1.
[11] Notice that despite the price elasticity below -1, Adena and Huck (2017) documented a reduction of large gifts in the matching treatment compared to the lead donor control. This seems to be explained by a large heterogeneity of their sample since the opera offers both highly subsidized and very expensive tickets.

percent of non-donors (that constitute our group of potential compliers). Since we do not know the identities of would-be donors, we present results without control variables. Drawing the control subjects at random is a possible alternative, but it does not affect the results and we do not present them here. The dependent variable is the log of the amount received plus one due to the inclusion of zero amounts. Table B4b, Columns I–III present the results of this exercise. For comparison, Columns VII–IX show the common log-log approach that relies on the donor sample only and is not correct if price reduction induces potential compliers to start giving as in our case. Columns IV–VI repeat the previous exercise, but use the log donation received plus one as a dependent variable. This is to show that the difference in the size of the coefficients resulting from adding one before the log is small. Our preferred specification for the constant elasticity assumption is in Columns I–III. It shows that our subjects are highly price elastic, with a (constant) price elasticity of around -2.5 (that is statistically different from -1).

Table B4b: Matching-price (constant) elasticity of charitable giving

| Dependent variable | Log(amount received +1) | | | | | | Log(amount received) | | |
|---|---|---|---|---|---|---|---|---|---|
| | I | II | III | IV | V | VI | VII | VIII | IX |
| Log price | -2.495*** | -2.595*** | -2.549*** | -0.912*** | -0.900*** | -0.892*** | -0.934*** | -0.921*** | -0.913*** |
| | (0.187) | (0.192) | (0.219) | (0.140) | (0.140) | (0.157) | (0.144) | (0.144) | (0.162) |
| Observations | 8151 | 7545 | 6392 | 7027 | 6421 | 5480 | 7027 | 6421 | 5480 |
| $R^2$ | 0.264 | 0.273 | 0.270 | 0.112 | 0.112 | 0.110 | 0.111 | 0.111 | 0.108 |
| Adjusted $R^2$ | 0.264 | 0.273 | 0.270 | 0.112 | 0.112 | 0.109 | 0.111 | 0.111 | 0.108 |
| sample | incl. unidentified don. | excl. unidentified don. | conservative + excl. unidentified don. | incl. unidentified don. | excl. unidentified don. | conservative + excl. unidentified don. | incl. unidentified don. | excl. unidentified don. | conservative + excl. unidentified don. |
| | All donors plus some non-donors in LD treatment such that shares included are equal | | | | Donors only | | | Donors only | |

Notes: OLS; Robust errors clustered at the office level; Conservative sample excludes incomplete blocks from the randomization stage and new offices; no controls; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## B5 Interest price elasticity of charitable giving

In our data, we have in fact two sources of variation in the price for giving. The first results from our treatment manipulation and is purely exogenous (in what follows, we refer to this price as the "matching price"). The second results from the fact that the money donated cannot be used to repay the loan and costs the individual one plus the interest rate (in what follows we refer to this price as "interest price").[12] The typical tax price does not apply in our context as there are no tax deductions for charitable giving in Kyrgyzstan. The interest rate is mainly

[12] The microfinance company allows for flexible repayments on top of the monthly rate. Indeed, we observe a non-negligible number of additional repayments above the required 2–3 times in the period under study; see Figure B1d in Appendix B1. Additionally, the repayment amounts vary.

determined by the type of loan (28 categories) and the individual's loan repayment history. In addition, there is a random component depending on official interest rates at the time of taking the loan and on later interest rate adjustments resulting from recalibrations of the company's portfolio.[13] That means that, after accounting for loan category, repayment history, and observables, we can consistently estimate the interest-price elasticity of charitable giving and compare it to the match-price elasticity implied by our treatments.

Table B5a shows the results from a level-level regression of the (nominal) interest price on donations received which includes controls for the available determinants of the interest rate, other individual characteristics, and the match price. The resulting estimates are -2.5 to -3.0, and very clearly below -1. The coefficients can be interpreted as the point interest-price elasticity calculated at means and we can compare this point elasticity to the arc match-price elasticity calculated in Table B4a (last column and row) to be around -1.4. The conclusion is that our sample is both price elastic with respect to the interest price and with respect to the match price. The fact that the interest-price elasticity seems larger (in absolute terms) than the match-price elasticity might be explained by the higher awareness of own interest rate compared to some clients not being aware of the matching.[14]

Table B5a: Interest-price elasticity of charitable giving

| Dependent variable: donation amount | | |
|---|---|---|
| | I | II |
| Interest-price elasticity | -3.008*** | -2.539*** |
| | (0.459) | (0.6322) |
| Controls | no | yes |
| Observations | 185125 | 126291 |
| R2 | 0.001 | 0.009 |
| Adjusted R2 | 0.001 | 0.008 |
| sample | excl. unidentified don. | conservative + excl. unidentified don. |

Notes: OLS with loan type fixed effects (areg in stata); Robust errors clustered at the office level; Conservative sample excludes incomplete blocks from the randomization stage and new offices Controls include: dummies for the randomization-level block, client level controls including gender of the client, age of the client, the number of previous loans taken in the company, education level dummies, marital status dummies, occupation type dummies, dummies for taking up and closing the loan in the experiment period, self-reported income, interest rate of the loan, the sum of due repayment delayed for more than 30 days, and the term of the loan in months; office and region level controls including dummy for urban areas, region dummy, number of clients per office; specialist level controls including client number, portfolio size, age, number of children, education dummies, experience in months, family size, female dummy, material status dummies, and nationality dummies. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

13 In Table B1a in the Appendix B1, we study the determinants of the interest rate in our sample. Observable characteristics alone do not have much predictive power, with an R squared of 0.035, see Column II. Once controlling for product category and history of loans (see Column I), most of the coefficients on personal characteristics lose significance, while the R squared increases to 0.672. Although we cannot exclude that there are other unobservable determinants of the interest rate that are correlated with charitable behavior, we are confident that they do not have much influence.

14 See Eckel and Grossman (2017) for differences resulting from donor awareness of the offered subsidies in a setting with matching and rebate.

## B6 Treatment effects on credit specialists

One of the design features of our experiment is that, beyond the posters placed in the offices, credit specialists were instructed (and incentivized) to inform the clients about the charitable campaign and the treatments, that is, implicitly they acted as fundraisers. However, the credit specialists could themselves be influenced by treatments, which could lead them to be more active in one treatment than another, resulting in different rates of informed clients and thus confounding the main analysis. See Section A7 for the hypotheses regarding the spread of information from credit specialists to clients.

To test potential treatment effects on the behavior of credit specialists who acted as intermediaries, the company conducted phone surveys with 7,511 randomly chosen customers, with the first surveys starting 10 days after the beginning of the campaign and lasting until the end. In total, 10.6 percent surveyed clients confirmed that they knew about the campaign. This number is relatively low, but it might be a function of the relatively early start of the telephone survey. In Table B6a, in a regression framework, we compare rates of informed clients by treatments and confirm that there are no significant differences in credit specialist motivation to inform more or fewer clients about the campaign in a particular treatment. Thus, we can conclude that potential treatment differences in the likelihood of giving are not driven by different shares of clients being informed about the campaign. In other words, we do find support for hypothesis S1 in the data.

However, this does not exclude the possibility that credit specialists differentially selected the clients to be informed depending on treatments. For example, if they were motivated more by one of the treatments, they could have put more effort into informing richer customers who they expected to be more likely to give while holding the total number of informed clients constant due to time restrictions. To address this concern Tables 3–5 in the main text include or exclude controls. Given that this has no meaningful impact on coefficient size, we deem this scenario unrealistic.

Table B6a: Share of clients informed

| Dependent variable: informed dummy | | |
|---|---|---|
| treatment matching | -0.011 | -0.011 |
| | (0.007) | (0.009) |
| treatment local | 0.003 | 0.003 |
| | (0.007) | (0.009) |
| Observations | 7511 | 7511 |
| $R^2$ | 0.000 | 0.000 |
| errors clustered | No | specialist |

Notes: Sample of surveyed clients; Robust or clustered robust errors; * p < 0.10, ** p < 0.05, *** p < 0.01

Finally, we want to confirm that information is directly linked to donations. To test hypothesis S2, that credit specialists with a higher share of informed clients raise more funds, we run regressions on specialist and client levels separately. The test on the specialist level is a direct one. Here, we regress the average share of clients with the positive donations of a specialist on her average share of informed clients (Table B6b, Columns I and II). This average share of informed clients per specialist is inferred from the subset of clients that were surveyed by phone. Note that we excluded specialists with a zero share of informed clients from the sample as well as those with two or less surveyed clients (the last was most likely for new credit specialists, who did not have many clients at the start of the experiment). The results of the regression show that the higher the share of informed clients per specialist the higher the average share of donors among her clients.

Table B6b: Behavior of the specialists

| Dependent variable: | Share of donors | | Likelihood of giving | | Average return per specialist (log of) | | Donation per client including zeros (+1, log of) | |
|---|---|---|---|---|---|---|---|---|
| | I | II | III | IV | V | VI | VII | VIII |
| average rate of informed clients per specialist | 0.045*<br>(0.023) | 0.050**<br>(0.024) | | | 0.166*<br>(0.090) | 0.184**<br>(0.092) | | |
| average rate of other informed clients of the same specialist | | | 0.044*<br>(0.023) | 0.042**<br>(0.020) | | | 0.161*<br>(0.088) | 0.155**<br>(0.078) |
| Observations | 373 | 362 | 129002 | 128900 | 373 | 362 | 129002 | 128900 |
| Observation-level | specialist | specialist | client | client | specialist | specialist | client | client |
| R2 | 0.024 | 0.082 | 0.001 | 0.007 | 0.023 | 0.087 | 0.001 | 0.006 |
| Controls | - | yes | - | yes | - | yes | - | yes |

Notes: OLS; Robust errors clustered at the office level; Sample: excluding specialists with zero rate of informed and less than three clients surveyed; Controls include: treatment dummies, urban, cycle, age, female, education dummies, business type dummies marital status dummies, taking/closing loan dummies, income; Specialist level regressions (averages by specialist) are weighted by the number of clients; controls include specialist level controls: age, number of children, education category dummies, experience in months, family size, and female dummy; * p < 0.10, ** p < 0.05, *** p < 0.01.

For client level regressions, we regress a dummy equal to one if a client donated on the average rate of other clients of the same specialist being informed. Note that when calculating this average, we exclude for each client his/her own contribution to the specialist's overall average since, especially for specialists with a small number of surveyed clients, the shares of informed clients are highly dependent on the own declaration in the interview and, of course, being informed is expected to affect giving directly. The results are presented in Table B6b, Columns III and IV. Again, each client is more likely to donate the higher the rate of other clients being informed by the same specialist.

The second set of regressions take as an outcome the average donation revenue per client for each specialist in the specialist-level regressions per specialist (log, Table B6b, Columns V and VI) or individual donations (including zeros) in the client-level regressions (donation +1, log, Table B6b, Columns VII and VIII). The results suggest that, the higher the rate of informed clients, the higher the average return per specialist and the higher the average rate of informed other clients, the higher the return per client. Thus, we conclude that hypothesis S2 is also supported by the data.

Altogether, after critically assessing our design, we are confident that our findings are not confounded.

## B7 Individual characteristics and heterogeneous treatment effects

In this section, we report the controls that are significantly correlated with one of the variables of interest and also perform an analysis of heterogeneous treatment effect of the pre-registered variables.

First, we analyze the correlates with the response rate among the control variables. Clients who had more loans previously in this company (long-term clients) are more likely to donate relatively to newer clients. Older clients and women are also more likely to donate than younger ones and men, respectively. Those who took the loan during the duration of the experiment are more likely to donate relatively to those who took loan before the start of the experiment. This effect might be driven either by the intention of the clients to signal their "good" type to the credit specialist who decided on the eligibility of receiving the loan, by displaying some "immediate" reciprocity for the loan agreement, or by the "effect of holding the money in hand." Those who were called during the survey are also more likely to donate, as their attention might be directed towards the campaign. Finally, those clients who had delayed payments to the company by more than 30 days were less likely to donate, as they are likely to never show up in the office and hide from contacts from company's side. Interestingly, self-reported income is not significantly related to the response rate.

Among the controls, we found several significant predictors of the donation amount, conditional on giving. Single clients donate higher sums than other clients. Those who took loan during the experiment donated smaller sums relative to those who took loan before the start of the experiment (although they are more likely to donate). Finally, clients with higher self-reported income donate significantly higher amounts.