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# Charitable giving by the poor: A field experiment on matching and distance to charitable output in Kyrgyzstan

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Abstract

***Charitable giving by the poor A field experiment on matching and distance to charitable output in Kyrgyzstan****

Previous studies of charitable giving have focused on middle or higher income earners in Western countries, neglecting the poor. Despite this focus, the lowest income groups are often shown to contribute substantial shares of their income to charitable causes. In a large-scale natural field experiment with over 180,000 clients of a micro-lending company in Kyrgyzstan, we study charitable giving by a population that is much poorer relative to the typical donors that have been studied so far. In a 2x2 design, we explore two main hypotheses about giving by the poor: (i) that they are more price sensitive and (ii) that they care about their proximity to the charitable project. We find evidence in favor of the former hypothesis but not of the latter.

*Keywords:* Charitable giving, field experiments, matching donations

*JEL classification:* C93; D64; D12

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# 1. Introduction

Most studies on charitable giving focus on middle class individuals in Western countries (see, for example, DellaVigna, List, and Malmendier 2012; List and Lucking-Reiley 2002; Andreoni, Rao, and Trachtman 2017; Landry et al. 2010; Altmann et al. 2018). Poorer parts of the population distribution and those in developing countries have not been studied extensively. Yet, studying giving by the poor in more detail appears important for at least two reasons: on the one hand, it would provide a test of some of the common fundamental findings on giving behavior in the literature; on the other, it matters economically as the poor tend to give substantial fractions of their income to charity (Andreoni 2006a).

In this paper, we focus on two hypotheses about giving behavior among the poor. First, we conjecture that the poor exhibit more price elastic demand for charitable goods. This conjecture follows from two observations: (i) existing studies on charitable giving among the middle classes have shown that demand for charitable goods behaves pretty much like demand for consumer goods (see, for example, Huck and Rasul 2011); (ii) it is well established that demand for consumer goods tends to be more elastic for the poor.[1] Hence, if charitable goods are also like consumer goods for those in the poorer end of the income distribution, we conjecture that the poor exhibit more elastic demand for charitable goods than the rich.

As is common in the literature, we study this conjecture by comparing treatments with and without matching incentives that alter the price of giving. Previous studies on standard linear matching schemes (see, for example, Huck, Rasul, and Shephard 2015 and the references cited therein) have robustly shown that matching induces substantial crowding out of larger donations. Although matching also generates some additional small donations, the first effect dominates in most samples studied. In contrast to this, we expect that in our population crowding will be reduced or perhaps even eliminated as predicted by greater price elasticity.

Our second conjecture deals with the role of distance to the beneficiaries of the charitable good. The experimental literature has documented that social distance affects generosity, see, for example, the seminal study on dictator games by Hoffman et al. (1994). While some studies find

[1] Demand for food and other consumption goods has been shown to be more price-elastic in poor countries than in rich countries (see, for instance, the meta-analysis by Cornelsen et al. (2015) for food, or Regmi and Seale (2010) for other consumption goods) and poorer consumers within a country have been shown to be more price sensitive than richer consumers (Jones, Chern, and Mustiful 1994).

an effect of geographical proximity on charitable giving of middle-income donors (Genç et al. 2019) other find that donors are largely unaffected by geographical proximity (Brown, Meer, and Williams 2017; Meer 2014). However, it is unclear whether revealed preferences relate to the location of the charity or the location of the charitable output. We overcome this by just varying only the distance of charitable output and keeping the charity fixed. At the same time, the donors are socially similar to the beneficiaries. We conjectured that, in our population, we will 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 matters more for the poor; second, our donors may be more likely to actually benefit themselves from the charitable projects if they are near.

In order to test our two hypotheses, we conducted a large-scale field experiment in Kyrgyzstan with over 180,000 customers of a microfinance company. 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 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 up to an upper bound equal to the unconditional lead gift. Therefore, we held the signaling value of the lead gift equal and only varied the price of giving as in Huck, Rasul, and Shephard 2015. In the other dimension, we varied the impact of the current donations on the location of future projects.

We find that, compared to the simple announcement of a lead donor, matching increases the return from our campaign by more than 4 percent. This is driven by a substantial effect on the extensive margin in the order of 25 percent and the absence of any crowding out for larger donations. All in all, we provide strong evidence in favor of our first hypothesis. We find substantially higher price elasticity of giving than previous studies based on Western and richer samples. This once again confirms that charitable goods do behave similarly to most consumer goods—here by exhibiting higher price elasticity for the poor. As a consequence, 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 is in sharp contrast to fundraising among the middle and higher-income donors. This also reminds us that “One

Swallow Doesn't Make a Summer” (Maniadis, Tufano, and List 2014) and that it is worth critically reviewing previous findings even if they appear to result in an absolute consensus. Moreover, given the similarities in demand for consumer and charitable goods, where available, fundraisers may wish to use price elasticity data for consumer goods as an indicator on whether or not matching will cause crowding.

In contrast to the affirmative results for our first conjecture, we do not find support for our second conjecture. The treatment in which donations increase the probability of a future project being implemented locally has no significant effect. Additionally, there is no correlation between donations and the spatial distance between donors and the current projects. We conclude that there is no rough-and-ready rule for the effect of spatial distance between 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 and Section 5 summarizes our hypotheses. Section 6 presents the results and Section 7 concludes.

## 2. Literature

*Charitable giving and public goods contributions by the poor.* While the majority of studies on giving and contributions focus on middle and higher-income individuals, there are a few studies that do include poorer populations in Western countries. Using survey data from independent sector, Andreoni (2006a) documents a u-shaped relationship between income and charitable giving for the US. While, on average, the poor give 4.8 percent of their income to charitable causes, middle-income individuals give 1.3 percent, and high-income earners 3 percent.

In a natural field experiment, Andreoni, Nikiforakis, and Stoop (2017) study the pro-sociality of individuals living in rich versus poor neighborhoods in a Dutch city and conclude that, while the rich appear to be more pro-social in their raw data, this difference is simply explained by rich people being richer and not by any differences in underlying preferences.

From a series of field experiments, Whillans, Caruso, and Dunn (2017) conclude that the rich and poor have different self-concepts: While the poor respond more to charity appeals that emphasize communion, the rich do so when the charity appeal emphasizes agency. More recent field experiments include de Oliveira, Croson, and Eckel (2011), de Oliveira, Eckel, and Croson

(2012), and Li, de Oliveira, and Eckel (2017), who study giving to different organizations in a historically low-income African-American neighborhood in Dallas, Texas, in the US and compare giving patterns and reactions to community identity priming in poor versus low- to middle-income neighborhoods. They demonstrate that giving behavior among low-income people exhibits both persistence and context-dependence. For example, experience with crime increases the likelihood of donations. Bennett (2012 and 2018) conducts comparative studies on giving behavior of London's working and non-working poor. These studies reveal that working poor giving patterns are more similar to those of middle-income people than to those of the non-working poor.

Few studies analyze charitable giving in developing countries. Candelo, Eckel, and Johnson (2018) conduct a lab-in-the-field experiment on dictator giving in low-income Mexican villages. They find higher giving towards family members than towards community members and strangers, with no difference between the last two groups. Jack and Recalde (2015) study leadership giving in a field experiment in rural Bolivia and report that voluntary contributions for the provision of environmental materials for local schools increase when the democratically elected local authorities lead by example. Mahmud and Wahhaj (2018) empirically study voluntary contributions made by credit borrowers to their non-profit microfinance institute in Pakistan and report that clients donate more before they apply for another loan. In a laboratory experiment, Blanco and Dalton (2019) study charitable giving in Bogota by different social strata of individuals and conclude that the rich and poor are equally generous and both the rich and poor are similarly motivated, namely rather by warm-glow than by pure altruism.[2]

*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. Matching has been shown to increase the response rate but to lower the average donation given (also called the checkbook amount or out-of-pocket donation). The emerging consensus is that, relative to fundraising calls where there is an unconditional lead gift of the same size as the amount available for matching, matching leads to crowding out of larger donations, which can harm the overall success of a fundraising drive

[2] For studies concentrating on the rich and very rich see, among other, Kessler, Milkman, and Zhang (2019), Coupe and Monteiro (2016), Andreoni (2006b), and James and Baker (2012).

despite creating additional small gifts (Huck, Rasul, and Shephard 2015; Huck and Rasul 2011; Rondeau and List 2008).[3]

*Local benefits*. Anecdotal evidence suggests that donors prefer local charities. Studies based on laboratory experiments with dictator games confirm that giving increases when social distance is reduced; see, for example, Hoffman et al. (1994). However, In a laboratory experiment with giving to real nonprofits, Brown, Meer, and Williams (2017) find no obvious preferences for local versus national charities. In contrast, using data from an online giving platform in the US, Meer (2014) finds some evidence in favor of local versus national preferences. In a hypothetical survey experiment with participants from New Zealand, Genç et al. (2019) find that donors place more weight on geographic distance (preferring to support a charity being active in New Zealand than in any other country) than they do on the need of the recipient or the expected effectiveness of the donation. Causal evidence for spatial preferences in the charitable giving domain in the field is, to our knowledge, non-existent.

## 3. Background information

We partnered with a recently established charity called "Apake" in Kyrgyzstan.[4] The charity collects donations and implements projects to improve local life in different areas of Kyrgyzstan. The projects are chosen from proposals that can be submitted by all citizens. For the first large-scale campaign, the charity selected nine projects,[5] one in each of the administrative regions of Kyrgyzstan. All nine local projects related to water supply, local infrastructure, hospitals, or school reconstructions. The expected cost of all projects was 2 million KGS (approx. USD

[3] Alternative matching schemes have been shown to reduce or avoid 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 a predetermined, individually set threshold (Adena and Huck 2019). Other innovative matching schemes analyzed include nonconvex matching (Huck, Rasul, and Shephard 2015; Castillo and Petrie 2019), 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).

[4] The mission of the fund is to build "a healthy and developed society with high goals and noble aspirations," and "to initiate social entrepreneurship and support local social needs, mobilizing the civil and business communities through the development of a culture of charity." See www.apake.kg for details.

[5] An advisory board of the fund reviews and chooses the projects to be implemented by means of voting.

28,600).[6] In order to implement these projects, the charity initiated a fundraising campaign. 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. In the period under study, the company's clients were the only individuals targeted by the fundraising campaign. For the fundraising drive, each office of the microfinance company received a transparent donation box to be placed close to the cash desk of the office. In addition the offices received treatment-specific posters, treatment-specific information flyers and flyers with general information about the charity and the nine projects, which were the same across treatments.

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 do not have any direct monetary consequences but were aggregated in the company's established ranking system of credit specialists. Approximately every two months, the best performers received prizes, like certificates, books, tickets for events, and so on. There were no incentives for specialists relating to the amounts of donations collected.

Clients come to the office regularly, either to acquire a loan or to make a repayment for an active loan (see Figure B4 in the Appendix B for a distribution of visits in the sample and period under study). Once a client put his or her donation into the donation box, he or she was asked to write down a telephone number and amount donated. The charity made every donation public on its website, by posting the first five and the last two digits of the cellphone number and the amount given. Clients were informed that this was essential for reasons of transparency and accountability. Thus, all donors could verify whether their donations had reached the fund. However, clients were informed that their donations would appear online only after the end of the two-month campaign due to the off-line system of collecting donations. For us, this was a convenient way of matching the donors with the clients' database and avoiding spill-overs between clients-cum-donors.[7] Appendix A provides additional details of the campaign.

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[6] 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, average exchange rates for the experiment period are used throughout the paper.

[7] There was another way of donating to the fund—through cash-in machines that are typically placed in big shops or banks, and typically used to refill the prepaid cell phones. This method was mentioned on the posters placed in the

The population under study consists mostly of people running small businesses who have, on average, total loans equal to an average monthly income. Average/median self-reported monthly income in our data is KGS 21,304/18,633 (approx. USD 306/268), which compares to a GDP per household of approx. USD 530 monthly.[8] Note that the income data in our sample is self-reported as 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 is 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 would also have access to less expensive credit from banks (provided geographical access). More details on the population under study can be found in the Appendix A.

The loan sums range from around USD 70 up to USD 2,850. The interest rate in our period is between 11 percent and 50 percent,[9] with an average of around 35 percent per year. The interest and the maximum amount of credit depend on the client's credit history and whether the client is eligible for special conditions. The main determinant of the discount on the interest is the number of previous credits that the client has taken out and repaid without any delay (see Table B1 in the Appendix B that shows empirically how the interest rate depends on individual characteristics). 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 credit repayment. These 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 credit-issuing company.

All clients have to re-pay loans monthly, on a pre-specified date without delay, but they are also free to repay more or more often. 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

offices of the microfinance. Only few donations were done through the terminals and could be matched to the customers. Therefore, we count them alongside with the other donations without explicitly distinguishing them.

[8] This number is based on the annual GDP per capita (current US$) of US$ 1.220.47 (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 A1 in the Appendix.

[9] For the Islamic type of credits, we converted the fee to the equivalent interest rate. The sample also contains 740 credits with interest in range of 0–5%. These preferential credits can be issued as a financial help for long-term clients who, for example, either need money for health treatment or went through some accident, like fire of the house.

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 small business purposes, but they also include some consumer loans. 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 credit 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. As a result, the default rate of the loans is very low for the microfinance market, below 1 percent.

## 4. Experimental Design

We implemented a 2x2 design. The first experimental dimension relates to donation matching: One half of the clients were informed that a lead donor had already contributed half a million KGS (around USD 7,000), the other half 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 very 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.[10] The exact source of the money was not mentioned to clients.

The second experimental dimension varied local benefits of donations given. One half of the clients did not get any additional information, while another half was informed that "If clients of [name of the company] 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!" This was implemented later

[10] With expectations being not rational and very low, the signaling value could be lower in the matching treatment. This, however, would lead to an even harder test for the matching treatment to outperform a lead donor treatment with a higher signaling value.

on.[11] 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.

Prior to the implementation, we performed blocked randomization in order to assure the similarity between our treatment groups. This was especially important since at the level of randomization—the office—there were only 104 units, which are quite heterogeneous with respect to location, the numbers of customers, and many other potential confounders. For this reason, we used the blockTools command in R (Moore and Schnakenberg 2016) taking into account a rich set of individual, specialist, and office level variables. For more details, consult Appendix C. The results of the randomization process make us confident that there are no major differences between the treatment groups. Given a 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.

Five offices were closed during the experiment and a small number of new offices opened in the period of the experiment. One office that was not part of randomization but opened before the start of the experiment was included by the management in the experiment in the treatment with local benefits but without matching.[12] Additionally one office that was a separate office in the randomization sample was subsequently merged with another office close by. Thus, the final sample available for analysis includes 99 offices and 185,845 clients. Consistent with the goal of keeping the original randomization balances, we will also replicate our analyses for what we call the conservative sample, which excludes the offices from incomplete randomization blocks and the office which was not part of the original randomization.[13] This procedure leaves us with 80 offices and 152,319 clients. In order to control the spread of information from credit specialists to the clients, the firm's internal call-center, which is usually used for marketing purposes and for verifying clients' contact details, made survey calls to a random sample of clients. The sample was selected such that each specialist had approximately an equal proportion of his/her clients

[11] There was no specific priority implemented for the group that was not informed about priority.

[12] Note that randomization was done based on data from January 2018.

[13] The randomization "created" the blocks of four offices that are most similar on observables. In each of this block, one office was randomly chosen into one of four treatments. In the conservative sample, if one of the offices from the block is excluded from the sample, other three offices belonging to the same block are also excluded. Therefore, on top of five closed offices and one merged office, further 18 offices are excluded, altogether 24 offices and six blocks from the original randomization sample of 104. Additionally, we exclude the office which opened before the start of the experiment but was not part of original randomization. This approach preserves the balance of the sample and leaves us with 80 offices (conservative sample).

surveyed. Due to time restrictions, the call center workers just asked whether the client was aware of the specific fundraising campaign and recorded yes or no as an answer.

## 5. Hypotheses

We pre-registered a set of hypotheses at AEA RCT Registry (AEARCTR-0002693, 05 March 2018). Our central substantive hypotheses are:

*M (Matching)*[14]

**M1 The response rate is higher in the matching than in the control treatment with an unconditional lead gift.**

**M2 There is no difference in the amount given (conditional on giving) 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.**

Motivation: Based on previous research (for example, 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. Since our sample consists of low-income individuals, we expect them to have a substantially higher price elasticity for the charitable good compared to previously studied middle- and high-income individuals. Consequently, we expect only the crowding-in effect to hold, inducing a larger response rate with all donation values being small.

*L (Local benefits)*[15]

**L1 There is no difference in the response rate between treatments with or without local benefits.**

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

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

[14] H3 in the pre-registration.
[15] H4 in the pre-registration.

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. Of course, we could also have stated that we expect an effect on the extensive margin. In actual fact, we were agnostic here and mainly wanted to pre-register the fact that we want to explore both margins.

Additionally, we formulated a couple of supporting hypotheses regarding our specific implementation.

**S1[16] 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[17] Specialists with higher shares of informed clients raise more funds.**

Motivation: Since the shares of clients informed may serve as a proxy for specialists' 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.

Note that M/L1–3 are not independent hypotheses but that 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 the Appendix C. 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 C) and because there is no obvious prior to be derived from theory or the previous literature.

[16] H1 in the pre-registration.
[17] H2 in the pre-registration.

# 6. Results

First, we provide overall results of the campaign in subsection 6.1. Then we provide the results for our main hypotheses regarding matching and local incentives in subsection 6.2. Subsections 6.3. and 6.4. contain more detailed analyses regarding the main hypotheses as well as a discussion. This is followed by the results regarding our supporting hypotheses for the behavior of credit specialists in section 6.5. Some additional analysis of heterogeneity and information regarding the role of individual characteristics is presented in Appendix D.

## 6.1. Campaign results

The total number of donations claimed was 7,027 generating a response rate of 3.8 percent. The average positive donation was KGS 63 (approx. USD 0.90, see Figure B1 in Appendix B for a histogram of donations).[18] Out of all claimed donations, 6,421 donations could be matched to a client of the company. The remaining 606 (8.6 percent of all claimed donation) could only be assigned to the office in which the donation was made. In Table 1 we test and confirm that there are no differences between treatments in the share of non-matched donation claims.[19]

There were sizable differences between claimed donations and the content of the donation boxes with, on average, an additional KGS 409 in the donation boxes (see Figure B2 for the distribution of differences by office). This may have resulted from some donors refusing to write down their telephone number, claiming they had donated less than they actually had, or their donation being overlooked by the cashier. Again, in Table 2, we test and confirm that there were no significant differences between treatments.

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

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

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; $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

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; $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

## 6.2. Treatment effects of clients

First, we consider response rates by treatments. Table 3 presents the results of linear probability regressions of the donation dummy on treatment dummies.[20] Column I presents the results for the 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 full sample, and Column V does so in the conservative sample. Independent of the sample restrictions and the presence of controls, the coefficients of the matching treatment are positive and significant.[21] The effect is estimated to be between 1.1 and 1.3 percentage points, which is a high effect, given the average response rate of 3.1 percent in the lead donor treatment. The coefficient on the local treatment is comparably much smaller and never significant. Thus, the results support the hypotheses M1 and L1.

[20] 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).

[21] All but one of the coefficients for the treatment matching are significant at the 5% level. We refer to significance at 10% as significant results, as we use conservative approach of clustering on the office level, and our hypothesis are directional (when assuming a difference) while the tests are not.

Table 3: Treatment effects on response rate

| Dependent variable: donation dummy | | | | | |
|---|---|---|---|---|---|
| | I | II | III | IV | V |
| treatment matching | 0.013** | 0.012** | 0.013* | 0.011** | 0.012** |
| | (0.006) | (0.005) | (0.006) | (0.005) | (0.006) |
| treatment local | 0.003 | 0.003 | 0.004 | 0.003 | 0.004 |
| | (0.006) | (0.005) | (0.007) | (0.005) | (0.006) |
| Observations | 185845 | 185239 | 152319 | 184974 | 152110 |
| R2 | 0.001 | 0.001 | 0.001 | 0.007 | 0.007 |
| Adjusted R2 | 0.001 | 0.001 | 0.001 | 0.006 | 0.007 |
| errors clustered | office | office | office | office | office |
| controls | - | - | - | yes | yes |
| sample | incl. unidentified don. | excl. unidentified don. | conservative + excl. unidentified don. | excl. unidentified don. | conservative + excl. unidentified don. |

Notes: OLS; 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 available; Clustered robust errors; Controls include: gender of the client, age of the client, the number of previous credits taken in the company, dummy for urban areas, education level dummies, marital status dummies, occupation fields dummies, dummy for the last closing the credit in the period of the experiment, dummies for taking up and closing the credit in the period of experiments, self-reported income, interest rate of the credit, the sum of returns delayed for more than 30 days, and the term of the credit in months. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

The increase in the response rate 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 4 which presents the results of OLS estimations of the log of the positive donation amount on treatment dummies. Columns I-V follow the same sample restrictions and specifications as the ones in Table 3. In all our models, the coefficients on treatments are not significant. Therefore, we confirm hypothesis M2; there is no difference in the amount given between the matching and lead gift treatment.

At the same time we find that there is also no effect of the local benefits treatment, that is, we cannot confirm hypothesis L2.

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 in the section 6.3.

The absence of significant effects of the local benefits treatment goes against our expectations. Though the sign of the coefficient goes in the predicted direction, it remains small and insignificant in all specifications. Therefore, we cannot confirm that there is a difference in preferences for charitable outputs depending on spatial distance. There are several questions arising from this, which relate both to our treatment and to the specific setup of how localness is defined. We discuss these concerns and run some robustness checks in the section 6.4.

Table 4: Treatment effects on intensive margin

| Dependent variable: log positive donation amount | | | | | |
|---|---|---|---|---|---|
| | I | II | III | IV | V |
| treatment matching | -0.050 | -0.060 | -0.061 | -0.069 | -0.067 |
| | (0.093) | (0.092) | (0.105) | (0.100) | (0.109) |
| treatment local | 0.031 | 0.032 | 0.016 | 0.026 | 0.020 |
| | (0.105) | (0.105) | (0.118) | (0.099) | (0.109) |
| Observations | 7027 | 6421 | 5482 | 6194 | 5305 |
| R2 | 0.001 | 0.001 | 0.001 | 0.019 | 0.022 |
| Adjusted R2 | 0.001 | 0.001 | 0.001 | 0.013 | 0.016 |
| errors clustered | Office | office | office | office | office |
| controls | - | - | - | yes | yes |
| sample | incl. unidentified don. | excl. unidentified don. | conservative + excl. unidentified don. | excl. unidentified don. | conservative + excl. unidentified don. |

Notes: See notes to Table 3.

Finally, we study the overall effects of the treatments on the returns from the campaign. Table 5 presents the results of OLS regressions with the dependent variable being the log of donations plus one. Columns I-V apply the same sample restrictions and specifications as the ones in Tables 3 and 4.

Table 5. Treatment effects on total donations

| Dependent variable: log donation amount plus 1 | | | | | |
|---|---|---|---|---|---|
| | I | II | III | IV | V |
| treatment matching | 0.046** | 0.042** | 0.046* | 0.040** | 0.045** |
| | (0.021) | (0.020) | (0.024) | (0.019) | (0.022) |
| treatment local | 0.014 | 0.012 | 0.017 | 0.011 | 0.018 |
| | (0.021) | (0.020) | (0.024) | (0.019) | (0.023) |
| Observations | 185845 | 185239 | 152319 | 184974 | 152110 |
| $R^2$ | 0.001 | 0.001 | 0.001 | 0.006 | 0.006 |
| Adjusted $R^2$ | 0.001 | 0.001 | 0.001 | 0.006 | 0.006 |
| errors clustered | office | office | office | office | office |
| controls | - | - | - | yes | yes |
| sample | full | excl. unidentified don. | conservative + excl. unidentified don. | excl. unidentified don. | conservative + excl. unidentified don. |

Notes: See notes to Table 3.

In all columns, the coefficients of the matching treatment are positive and significant. The size of the effect ranges from 4 to 4.6 percent. Thus, for the population in our study, the use of the matching scheme increases the return by 4 percent or more relative to the lead donor baseline treatment. This result is in sharp contrast to previous findings on the adverse overall effects of matching.

As for the local benefits treatment, in line with the zero significance of our results concerning the response rate and the intensive margin, the overall effect is also not significant.

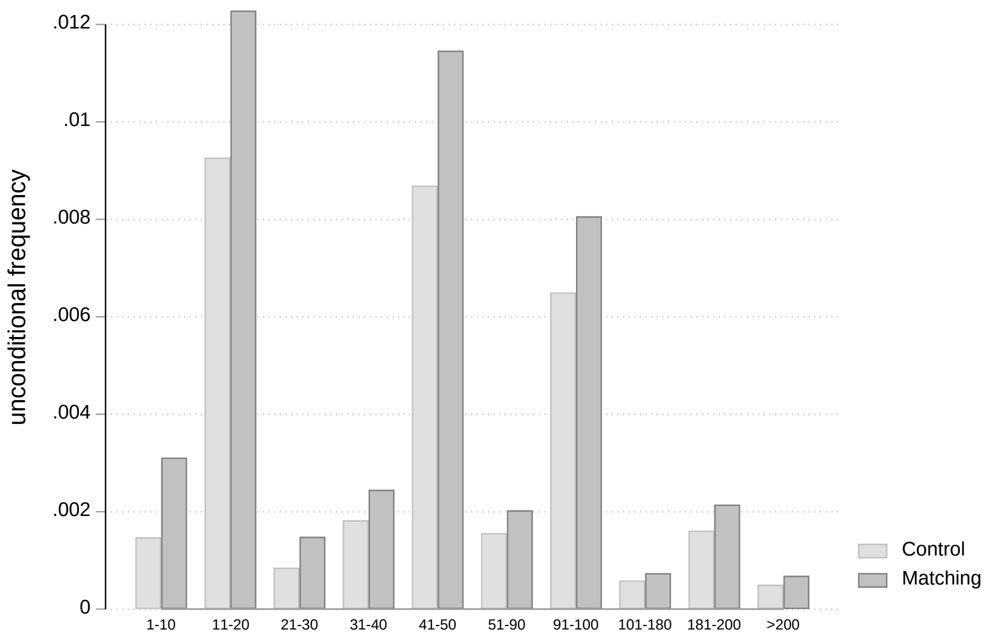
### 6.3. Is there really no crowding out? Estimating price elasticities for our data and previous studies

The question of whether we can really rule out crowding out in our data set is still to be answered. In Table 4, in which the matching dummy is regressed onto log positive donations, we observe a small negative but statistically insignificant coefficient. This 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.[22] Whether the first or second effect is operative can be better assessed once we take a closer look at the unconditional distributions of positive donations in both treatments. Figure 1 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*, while the increases are somewhat more pronounced in lower categories. This strongly suggests that there is no crowding out in our sample.

For comparison, Figure B3 in the Appendix shows the equivalent exercise using data from Huck and Rasul (2011), which documents meaningful crowding out effects. In their sample of Munich opera attendees, we see that, in addition to crowding in small donations, the matching treatment clearly crowds out large donations (the frequency of donations in different categories above EUR 150 is always smaller in the matching treatment than in the lead donor control).

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

Figure 1: Distribution of donations by categories in the matching versus control treatment




Notes: The horizontal axis presents the bins of the donation sums 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 used first bins of 10 for donations up to 100 KGS, of 20 for donations up to 200 KGS, of 50 for donations up to 400, 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 very low frequency in some bins, the above figure combines a number of bins into one.

A more formal assessment of the effectiveness of our matching treatment can be achieved by examining the price elasticity in detail. 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. 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 Adena (2014) 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 donative 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. 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 give, otherwise one needs to adjust for that.[23] 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 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 .[24] 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 received, $p$ denoting the price, and the superscripts $M$ and $LD$ signifying the matching and lead donor treatments respectively. The value of the arc elasticity can be calculated both, at the sample averages or in level-level regression, and 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 6, Column VIII shows the relevant results. The price elasticity is the largest (in absolute terms) in our population with -1.393.[25] Our calculation for Karlan and List's (2007) experiment is relatively large as well (in absolute terms) but it is based on a comparison without the signaling value of a lead donor, thus is expected to be lower

[23] 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 are included, the lower are the estimates.

[24] 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.

[25] Analogue level-level regression without controls leads to an elasticity of -1.35, significantly different from -1.

for a control with a lead donor. In the remaining studies the price elasticity is above -1 except in Adena and Huck (2017b).[26]

In Appendix B, for comparison reasons, we also report the results for a log-log specification which, unlike previous studies, accounts for potential compliers while getting rid of never-takers. This means that we include into 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 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. Table B2 of Appendix B presents the results of this exercise, while using the log of the amount received plus one as the dependent variable due to the inclusion of zero amounts (Columns I-III). For comparison reasons, 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 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 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).

Finally, in our data, we have in actual fact two sources of variations in the price for giving.[27] 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 credit and costs the individual one plus the interest rate (in what follows we refer to this price as "interest price").[28] 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 determined by the type of credit (28 categories) and the individual's credit repayment

[26] 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 tickets and very expensive ones.
[27] We thank Kim Scharf for pointing this out.
[28] 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 B4 in the Appendix B. Also the repayment amounts vary.

history. In addition, there is a random component depending on official interest rates at the time of taking the credit and on later interest rate adjustments resulting from recalibrations of the company's portfolio.[29] That means that, after accounting for credit category, repayment history, and observables, we can consistently estimate the interest-price elasticity of charitable giving and compare it to the price elasticity implied by our treatments.

Table 6: Matching-price (arc) elasticity of charitable giving in different field experiments

| | Comparison treatment | Sample | Donors | Response rate | 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.

[29] In Table B1 in the Appendix, 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 credits, 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.

Table 7: Interest price elasticity of charitable giving

| Dependent variable: donation amount | | |
|---|---|---|
| | I | II |
| Interest-price elasticity | -2.483*** | -2.424*** |
| | (0.556) | (0.633) |
| Controls | yes | yes |
| Observations | 153900 | 126369 |
| R2 | 0.005 | 0.005 |
| Adjusted R2 | 0.005 | 0.005 |
| errors clustered | office | office |
| sample | excl. unidentified don. | conservative + excl. unidentified don. |

Notes: OLS with credit type fixed effects (areg in stata); Conservative sample excludes incomplete blocks from the randomization stage and new offices; Clustered robust errors; Controls include: treatment dummies, fixed effects for product category (absorbed), cycle number, age dummies for urban female education, business type and marital status dummies, closing credit dummy, taking credit dummy, called in the survey dummy, balance left (log), self-reported income (log), due amount delayed for more than 30 days $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 7 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 around -2.4 to -2.3, and very clearly below -1. The coefficients can be interpreted as the point elasticity calculated at means and we compare is to the arc match price elasticity. The conclusion is that our sample is both price elastic with respect to the interest price and with respect to the match price.

Summing up this section, we find indeed *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 to previous studies.

An interesting question is perhaps whether the price elasticity of the charitable good could be predicted before a campaign? How different is the price elasticity for a charitable good from other goods, for which the estimates of price elasticity are potentially available?

While the exact answer to this question is outside of the scope of our study, our results suggest that the price elasticity of charitable goods may be correlated to estimates of price elasticity for consumer goods, as it is established that, on average, populations of poorer countries are more price elastic than population of richer countries (see, for instance, meta-analysis by Cornelsen et al. (2015) for food, Regmi and Seale (2010) for other consumption goods). Thus one could use the elasticities of demand for consumer goods as a proxy in order to decide on the use of donation matching scheme or rather opting out for the lead donor design.

## 6.4. 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 above (Tables 3- 5), although positive, the treatment dummy is never significant 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. Some clients 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. This concern should be higher for those who are living further away from the center of the region, i.e., closer to the boarders. Those, who are close to the border are more likely to be less incentivized by the local treatment while they might potentially expect similar proximity to the projects implemented in the neighboring region, the probability of which they cannot influence. 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 the Appendix B in Table B3. 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.

Alternatively, we can look at the correlation of the donation to the proximity of the currently implemented projects (independent of the treatments). We use geolocation of all offices and projects and estimate 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 the Appendix B in Table B4.

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 the Appendix B in Table B5. Thus, we conclude that there is indeed no preference for local charitable output in our sample.

## 6.5. 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 other, resulting in different rates of informed clients and thus confounding the main analysis.

In order 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 8, in a regression framework, we compare rates of informed clients by treatments and confirm that there are no significant differences in credit specialists' motivation to ask more or fewer clients for donations in a particular treatment. Thus, we can conclude that potential treatment differences in response rate and donations are not driven by different rates of clients being informed about the campaign. In other words, we do find support for hypothesis S1 in the data.

Table 8: Share 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; $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

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. In order to address this concern Tables 3–5 include or exclude controls. Given that this has no meaningful impact on the coefficients' sizes, we deem this scenario unrealistic.

Finally, we want to confirm that information is directly linked to donations. In order 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 response rate of clients of a specialist on the average rate of clients informed per specialist (Table 5, Columns I and II). This average rate of clients informed per specialist is inferred from the subset of clients that were surveyed by phone. Note that we excluded specialists with a zero rate of clients informed from the sample as well as those with two or less clients surveyed (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 rate of informed clients per specialist is the higher is the average response rate of specialist's clients.

For client level regressions, we regress a dummy equal to one if a client donated on the average rate of other clients 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 clients surveyed, 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 4, 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.

Table 5: Behavior of the specialists

| dependent variable: | Average response rate | | Average response rate | | Log of average return per specialist | | Donation per client including zeros (+1, log) | |
|---|---|---|---|---|---|---|---|---|
| | I | II | III | IV | V | VI | VII | VIII |
| average rate of informed clients | 0.045* | 0.050** | | | 0.166* | 0.184** | | |
| | (0.023) | (0.024) | | | (0.090) | (0.092) | | |
| average rate of informed other clients | | | 0.044* | 0.042** | | | 0.161* | 0.155** |
| | | | (0.023) | (0.020) | | | (0.088) | (0.078) |
| Observations | 373 | 362 | 129002 | 128900 | 373 | 362 | 129002 | 128900 |
| Observation-level | specialist | specialist | client | client | specialist | specialist | client | client |
| $R^2$ | 0.024 | 0.082 | 0.001 | 0.007 | 0.023 | 0.087 | 0.001 | 0.006 |
| Controls | - | yes | - | yes | - | yes | - | yes |

Notes: OLS, Sample: excluding specialists with zero rate of informed and less than three clients surveyed; errors clustered at the office level; Controls include: treatment dummies, urban, cycle, age, female, education dummies, business type dummies marital status dummies, taking/closing credit 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; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The second set of regressions take as an outcome the average return per specialist (log, Table 5, Columns V and VI) or return per client (donation +1, log, Table 5, Columns VII and VIII). The results suggest that, the higher the rate of clients informed, the higher the average return per specialist and the higher the average rate of informed other clients, the higher is 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.

## 7. 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 has led us to formulate two conjectures. First, that we will observe more price elastic demand for the charitable good on offer implying that matching should generate less crowding out. Second, we conjectured that local benefits will increase giving as the poor are more likely to benefit from themselves.

In order to study the first conjecture we compare a treatment with matching to a treatment without (making sure that the commitment from a lead donor is constant in both environments).

In contrast to previous findings from fundraising among the middle classes, we find that matching leads to a higher response *without* any crowding out confirming our conjecture about more price elastic demand for charitable goods among the poor. There simply appears to be no important difference in demand for charitable goods and consumer goods. Hence, we do not believe that our results contradict previous findings on matching. On the contrary, they illustrate remarkable consistency in the links between income and price elasticity. The implication for fundraising, of course, is drastic. While generating adverse effects when fundraising among the rich, matching unambiguously improves fundraising among the poor.

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

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# For Online Publication

## Appendix A.

## Details of the fundraising campaign

The company has 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 publically 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, and find out that there is a campaign.

## 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 in all groups at 18,500 soms (see more comparisons in Table A1).[30] In LiK, only 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.[31] In the Global Finance 2016 rank, Kyrgyzstan is number 148 out of 189.[32] 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).[33]

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

Table A1: Life in Kyrgyzstan survey—comparing individuals with and without microcredit

| | | hh has taken a loan from an 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.7055.1

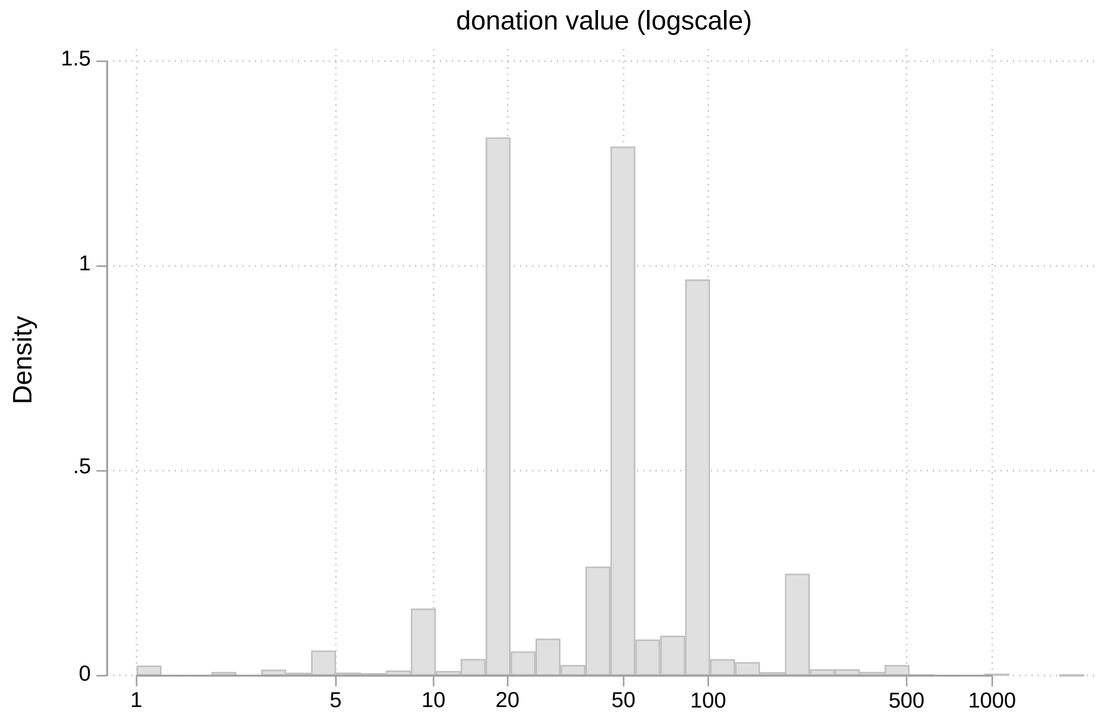
# Appendix B: Additional figures and tables

Table B1: Determinants of the interest rate

| | Interest rate | Interest rate |
|---|---|---|
| Sum borrowed in KGS | -0.000*** | |
| | (0.000) | |
| Cycle | -0.437*** | |
| | (0.023) | |
| Term of credit in months | -0.160*** | |
| | (0.013) | |
| Delayed sum | 0.297 | |
| | (0.230) | |
| 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 |

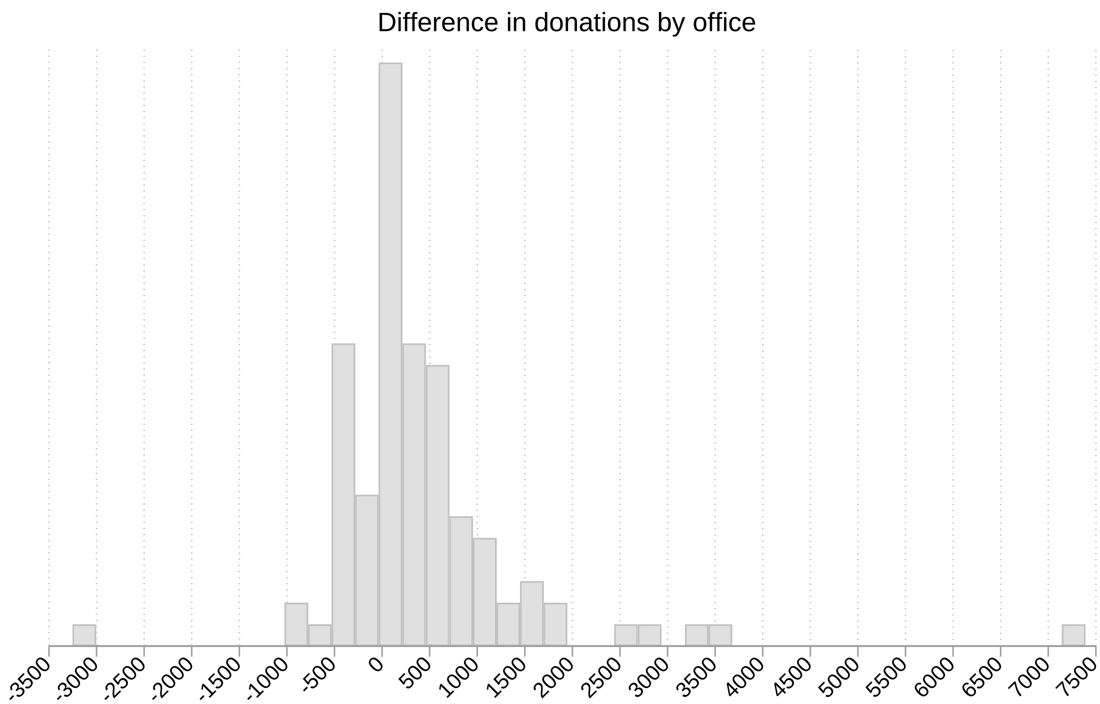
Notes: OLS, first column includes product fixed effects; errors clustered at the office level.
$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Figure B1: Histogram of donation values



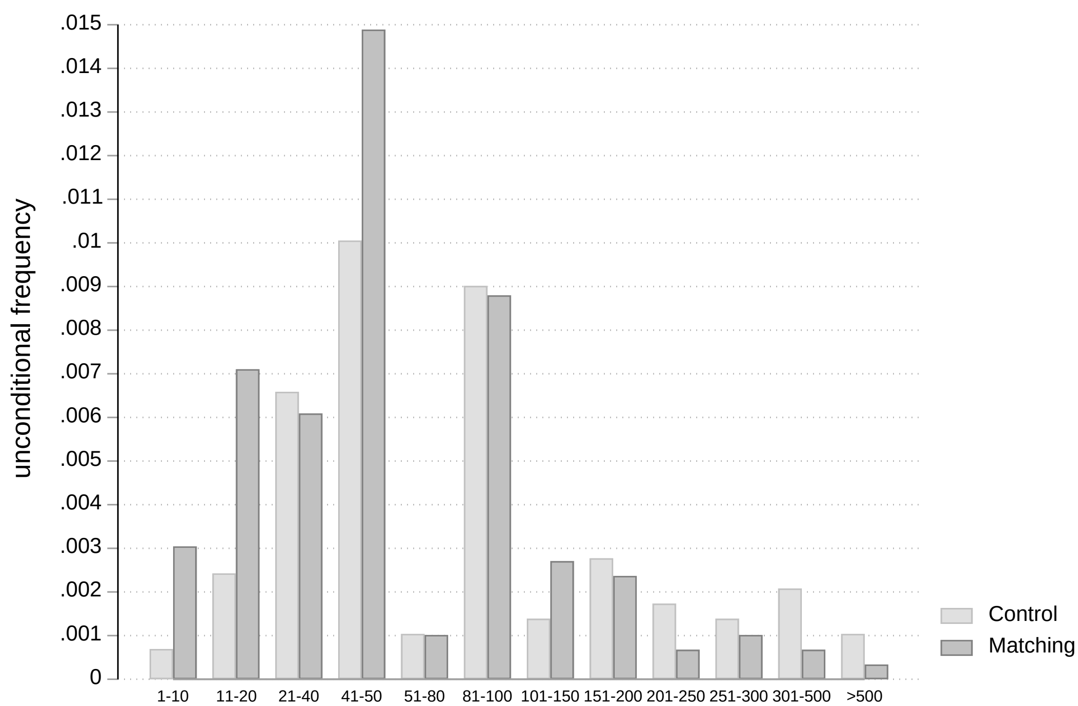

Notes: X-axis presents the bins of the donation sums in KGS. Y-axis presents density of the distribution.

Figure B2: Differences in donation by office




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

Figure B3: Distribution of gift levels in the Munich sample of opera goers (Huck and Rasul 2011)



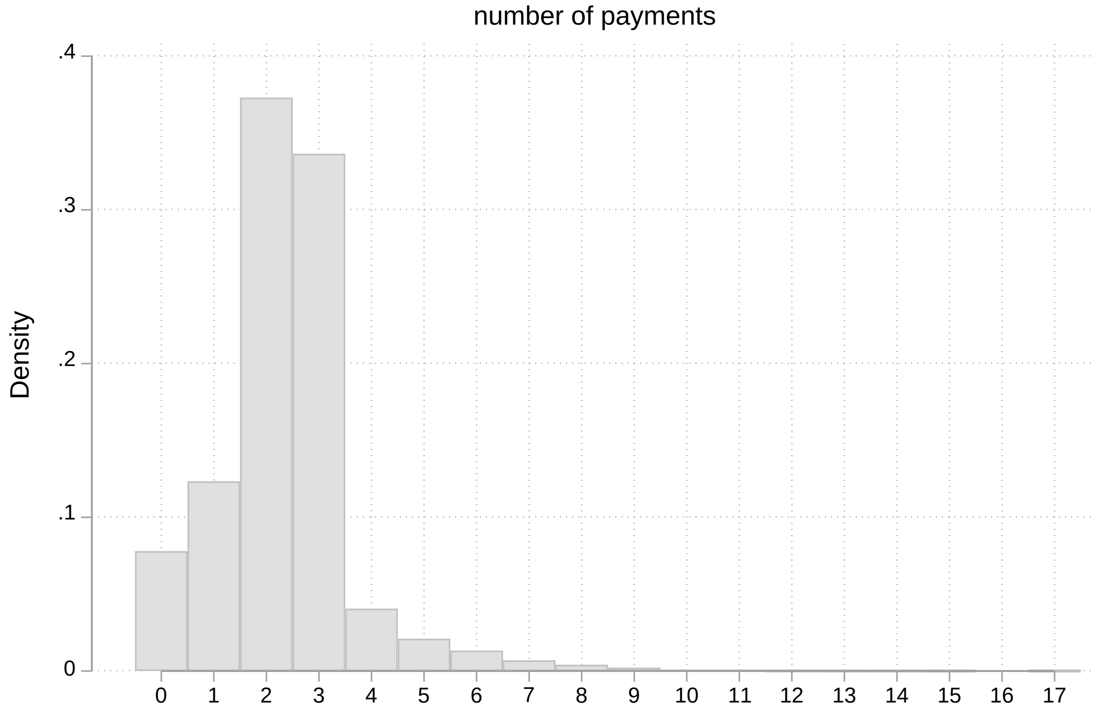
Notes: X-axis presents the bins of the donation sums in Euro. Y-axis presents density of the distribution.

Table B2: 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 | -2.501*** | -2.601*** | -2.535*** | -0.912*** | -0.900*** | -0.892*** | -0.934*** | -0.921*** | -0.913*** |
| | (0.184) | (0.189) | (0.224) | (0.140) | (0.140) | (0.157) | (0.144) | (0.144) | (0.162) |
| Observations | 8157 | 7551 | 6381 | 7027 | 6421 | 5480 | 7027 | 6421 | 5480 |
| $R^2$ | 0.264 | 0.274 | 0.269 | 0.112 | 0.112 | 0.110 | 0.111 | 0.111 | 0.108 |
| Adjusted $R^2$ | 0.264 | 0.274 | 0.269 | 0.112 | 0.112 | 0.109 | 0.111 | 0.111 | 0.108 |
| errors clustered | office | office | office | office | office | office | office | office | office |
| 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 zero giving in LD treatment such that shares included are equal | | | | Donors only | | | Donors only | |

Notes: OLS; 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; Robust or clustered robust errors; no controls; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure B4: 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 B3. 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** | 0.012** | -0.042 | -0.053 | 0.048** | 0.044** |
| | (0.006) | (0.006) | (0.086) | (0.088) | (0.022) | (0.020) |
| treatment local | 0.010 | 0.009 | -0.099 | -0.084 | 0.032 | 0.031 |
| | (0.013) | (0.012) | (0.243) | (0.248) | (0.046) | (0.042) |
| Treatment local*center of region | -0.009 | -0.009 | 0.208 | 0.184 | -0.027 | -0.029 |
| | (0.013) | (0.012) | (0.260) | (0.266) | (0.048) | (0.045) |
| Center of region dummy | 0.001 | 0.002 | 0.006 | 0.013 | 0.004 | 0.007 |
| | (0.006) | (0.006) | (0.087) | (0.092) | (0.025) | (0.022) |
| Observations | 185845 | 185239 | 7027 | 6421 | 185845 | 185239 |
| $R^2$ | 0.002 | 0.007 | 0.005 | 0.029 | 0.002 | 0.007 |
| Adjusted $R^2$ | 0.002 | 0.007 | 0.004 | 0.026 | 0.001 | 0.006 |
| errors clustered | office | office | office | office | office | office |
| controls | - | yes | - | yes | - | yes |
| sample | full | excl. unidentified don. | full | excl. unidentified don. | full | excl. unidentified don. |

Notes: OLS; 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; Robust or clustered robust errors; Controls include: gender of the client, age of the client, the number of previous credits taken in the company, dummy for urban areas, education level dummies, marital status dummies, occupation fields dummies, dummy for the last closing the credit in the period of the experiment, dummies for taking up and closing the credit in the period of experiments, self-reported income, interest rate of the credit, the sum of returns delayed for more than 30 days, and the term of the credit in months. In order to present results in more compact way, we only present results of estimation of full sample without controls, and full sample with controls for each outcome of interest. The results are however robust to sample restrictions.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B4. 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** | 0.012** | 0.014** | 0.011** | 0.048** | 0.044** |
| | (0.006) | (0.005) | (0.006) | (0.005) | (0.021) | (0.020) |
| treatment local | 0.003 | 0.002 | 0.004 | 0.002 | 0.012 | 0.010 |
| | (0.006) | (0.005) | (0.006) | (0.005) | (0.021) | (0.020) |
| Distance to closest project | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Observations | 185730 | 185124 | 152319 | 184859 | 185730 | 185124 |
| $R^2$ | 0.002 | 0.002 | 0.002 | 0.007 | 0.001 | 0.001 |
| Adjusted $R^2$ | 0.002 | 0.002 | 0.002 | 0.007 | 0.001 | 0.001 |
| errors clustered | office | office | office | office | office | office |
| controls | - | yes | - | yes | - | yes |
| sample | full | excl. unidentified don. | full | excl. unidentified don. | full | excl. unidentified don. |

Notes: OLS; Sample full with controls is identical to the one excluding unidentified donors since no controls available; Robust or clustered robust errors; Controls include: gender of the client, age of the client, the number of previous credits taken in the company, dummy for urban areas, education level dummies, marital status dummies, occupation fields dummies, dummy for the last closing the credit in the period of the experiment, dummies for taking up and closing the credit in the period of experiments, self-reported income, interest rate of the credit, the sum of returns delayed for more than 30 days, and the term of the credit in months. In order to present results in more compact way, we only present results of estimation of full sample without controls, and full sample with controls for each outcome of interest. The results are however robust to sample restrictions.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B5. 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** | 0.011** | -0.045 | -0.054 | 0.046** | 0.041* |
| | (0.006) | (0.006) | (0.097) | (0.097) | (0.023) | (0.021) |
| treatment local | 0.004 | 0.003 | 0.028 | 0.029 | 0.014 | 0.013 |
| | (0.006) | (0.006) | (0.101) | (0.101) | (0.022) | (0.020) |
| Distance to local project | -0.000 | -0.000 | 0.000 | 0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.001) | (0.001) | (0.000) | (0.000) |
| Observations | 185730 | 185124 | 7025 | 6419 | 185730 | 185124 |
| $R^2$ | 0.001 | 0.001 | 0.001 | 0.001 | 0.001 | 0.001 |
| Adjusted $R^2$ | 0.001 | 0.001 | 0.001 | 0.001 | 0.001 | 0.001 |
| errors clustered | office | office | office | office | office | office |
| controls | - | yes | - | yes | - | yes |
| sample | full | excl. unidentified don. | full | excl. unidentified don. | full | excl. unidentified don. |

Notes: OLS; Sample full with controls is identical to the one excluding unidentified donors since no controls available; Robust or clustered robust errors; Controls include: gender of the client, age of the client, the number of previous credits taken in the company, dummy for urban areas, education level dummies, marital status dummies, occupation fields dummies, dummy for the last closing the credit in the period of the experiment, dummies for taking up and closing the credit in the period of experiments, self-reported income, interest rate of the credit, the sum of returns delayed for more than 30 days, and the term of the credit in months. In order to present results in more compact way, we only present results of estimation of full sample without controls, and full sample with controls for each outcome of interest. The results are however robust to sample restrictions.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

## Appendix C: Randomization and power calculations

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 credits, average current balance of all current credits, average cycle (number of credits issued to a current credit holder), average share of credit 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 and 3-4 for the treatments A (no local benefits) and B (local benefits) and groups 1, 3 and 2, 4 for the treatments C (no matching) and D (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 A and B, and between C and D. 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 C2. There is no t-test p-value <10 percent.

Credit specialist data: From a total of 492[34] 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

[34] Excluding the dropped office.

characteristics using pairwise t-tests. In 56 comparisons, we find some significant differences (2 at p<0.01, 2 at p<0.05, and 6 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 treatment B or treatment D and all available individual level variables as independent variables. Table A4 presents average marginal effects after logit. The robust standard errors are clustered at the office level. When looking at Table 4, 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 according to a simple t-test comparisons. Therefore, in order to assess the balance at clients level, we run logit regressions with dependent variables being either treatment B or treatment D and all available individual level characteristics as independent variables. Table A5 presents average marginal effects after logit. The robust standard errors are clustered at the office level. When looking at Table 4, 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. Given some potential imbalances, the robustness checks after our main analysis will include control variables.

Table C1: Balance at the office level

| Treatment | No local benefits | | Local benefits | | | No matching | | 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 | 0.10 | 0.04 | 0.09 | 0.04 | 0.80 | 0.12 | 0.04 | 0.07 | 0.03 | 0.34 |

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| specialists | | | | | | | | | | |
| 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 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 credit 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 credit | 27077.33 | 481.98 | 27219.52 | 671.89 | 0.86 | 26803.92 | 652.24 | 27492.94 | 503.68 | 0.41 |
| Sum of credit 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 credits | 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 C2: Balance at the credit specialists' level

| Treatment | No local benefits | | Local benefits | | | No matching | | 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 C4: 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 C5: 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 |

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

## 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 credit issued per client.

ICC based on current balance equals to 0.02 while ICC based on credit 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 effect size of 0.1. While assuming ICC=0.04, there is enough power to detect effect size of 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.

<u>No multiplicity hypothesis testing corrections for the main hypotheses M/L1-3:</u>

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

## Appendix D: 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 credits 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 credit during the duration of the experiment are more likely to donate relatively to those who took credit 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 credit, 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 credit during the experiment donated smaller sums relative to those who took credit before the start of the experiment (although they are more likely to donate). Finally, clients with higher self-reported income donate significantly higher amounts.

Additionally to presenting the controls, we hypothesize potential heterogeneous treatment effects of several variables (as specified in our pre-registration). Given that the fundraising drive was mediated through the credit specialists, we consider heterogeneous treatment effect with respect to the gender of specialists. Table 9 presents the OLS estimations.

Table 9. Heterogeneous treatment effects with respect to gender of credit specialists.

| | 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* | 0.032 | 0.032 | 0.049** | 0.062** |
| | (0.007) | (0.007) | (0.120) | (0.120) | (0.023) | (0.027) |
| treatment local | 0.011 | 0.011 | -0.003 | -0.003 | 0.037 | 0.037 |
| | (0.007) | (0.007) | (0.129) | (0.129) | (0.024) | (0.026) |
| female specialist x matching | -0.005 | -0.005 | -0.126 | -0.126 | -0.024 | -0.037 |
| | (0.006) | (0.006) | (0.111) | (0.111) | (0.022) | (0.026) |
| female specialist x local | -0.014** | -0.014** | 0.060 | 0.060 | -0.047** | -0.041* |
| | (0.006) | (0.006) | (0.104) | (0.104) | (0.023) | (0.025) |
| Female specialist dummy | 0.008 | 0.008 | 0.112 | 0.112 | 0.029 | 0.035 |
| | (0.005) | (0.005) | (0.094) | (0.094) | (0.019) | (0.023) |
| Observations | 181924 | 181924 | 6097 | 6097 | 181763 | 149944 |
| $R^2$ | 0.001 | 0.001 | 0.004 | 0.004 | 0.006 | 0.006 |
| Adjusted $R^2$ | 0.001 | 0.001 | 0.003 | 0.003 | 0.005 | 0.006 |
| errors clustered | office | office | office | office | office | office |
| controls | - | yes | - | yes | - | yes |
| sample | full | excl. unidentified don. | full | excl. unidentified don. | full | excl. unidentified don. |

Notes: OLS; 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; Robust or clustered robust errors; Controls include: gender of the client, age of the client, the number of previous credits taken in the company, dummy for urban areas, education level dummies, Marital status dummies, occupation fields dummies, dummy for the last closing the credit in the period of the experiment, dummies for taking up and closing the credit in the period of experiments, self-reported income, interest rate of the credit, the sum of returns delayed for more than 30 days, and the term of the credit in months. In order to present results in more compact way, we only present results of estimation of full sample without controls, and full sample with controls for each outcome of interest. The results are however robust to sample restrictions. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

The interaction variable of the local benefits treatment with the dummy for female credit specialists is significant for response rate and log donations. Thus, female credit specialists are less likely to elicit donations from their clients in the local benefits treatment than male credit specialists. One explanation of this finding could be that the local benefits treatment has a contest aspect, and female credit specialists are less prone to be involved in the competition in line with the finding of a gender gap in self-selecting into the competition (Niederle and Vesterlund 2007).

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