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# Conditional Cash Transfers, Debit Cards and Financial Inclusion:

Experimental Evidence from Argentina

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## Abstract\*

Cash transfer and other social protection programs in developing countries have often been accompanied by measures to foster financial inclusion, such as the adoption and use of bank accounts and electronic means of payments. Argentina's social benefits are paid in bank accounts and accessed through debit cards. With the simultaneous objective of fostering formality among beneficiaries and stores, the use of debit cards for purchases has been incentivized by means of additional subsidies. We studied the low take-up of these extra benefits by means of a field experiment involving 400,000 beneficiaries of Argentina's largest conditional cash-transfer program (with 2.2 million beneficiaries who are the parents of four million children, 40% of the country's 0-17-year olds). By using their debit card to spend the allowance, rather than withdrawing cash from ATMs, they can receive a rebate of 15% of their expenditures. However, they systematically fail to claim this benefit: only about 25% of beneficiaries receive this transfer. Our experiment provided information about the effectiveness of an information campaign conducted via text messages or through on-screen messages at ATM machines. The campaign increased purchases with debit cards and subsequent rebates significantly but not substantially in the short run. However, beneficiaries who increased their use of debit cards do not exhibit a higher probability of having access to credit through the financial system, nor higher levels of formal employment. The results indicate that cultural factors (a preference for cash), administrative hassle and citizen security issues are relevant issues that limit the potential of financial inclusion through increased use of digital means of payment.

**JEL classifications:** C93, H26, K34, K42, Z13

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# 1 Introduction

Cash-transfer and other social protection programs have greatly expanded in developing countries in the last two decades. A related issue is financial inclusion in the context of these programs and their target populations. They have often been accompanied with measures to foster financial inclusion, such as the adoption and use of bank accounts and electronic means of payments. The existing literature has focused mainly on debit-card and bank-account use by the poor and on the impact of that use on savings (Bachas et al., 2021; Callen et al., 2019; Higgins, 2019).

Argentina’s social benefits (the conditional cash transfer program, *Asignacion Universal por Hijo - AUH*) are paid in bank accounts and accessed through debit cards. With the simultaneous objective of fostering formality among beneficiaries and stores, the use of debit cards for purchases was incentivized by means of additional subsidies.<sup>1</sup> By using their debit card to spend the allowance, rather than withdrawing cash from ATMs, beneficiaries received a rebate of 15% of their expenditures. However, only about 25% of beneficiaries respond to this incentive and receive this additional transfer.

Drawing on several large-scale administrative datasets on beneficiaries and commercial and financial infrastructure, we leverage a large field experiment involving 400,000 beneficiaries of Argentina’s largest conditional cash transfer program (with 2.2 million beneficiaries who are the parents of four million children, 40% of the country’s 0-17 year olds), conducted in partnership with the social security administration, the national tax administration, and the Ministry of Finance. The experiment consisted of an information campaign conducted via text messages and through on-screen messages at ATM machines.

Previous work on a subset of this experiment focused on benefit take-up. Some of the barriers posited in the literature have been fear of stigma, administrative and transaction costs, hassle, lack of information or inattention, and misinformation/misperceptions about potential negative consequences of signing up. All of these issues are exacerbated in a developing-country context with high levels of informality and complex regulations. Previous results from a subset of this experiment indicates that the campaign significantly increased purchases with debit cards and subsequent rebates, but that the benefit had low salience, with limited information on the debit-card purchase rebate program (Cruces, 2020).

Our information campaign via text messages increased take-up (i.e., purchases with debit cards and subsequent rebate) significantly but not substantially by providing information and raising salience. The original experiment had several arms to attempt to distinguish among hypotheses on benefit take-up – salience, stigma, and lack of informa-

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<sup>1</sup>In a related study, Brockmeyer and Saenz Somarriba (2022) analyze the impact of a similar program aimed at the wider population and not at cash transfer recipients in Uruguay. The focus of this program and of the analysis is tax compliance rather than financial inclusion.

tion, among others.

The additional analysis presented here aims to study a series of additional questions. The main research question is to gauge the impact of increased debit card use (beyond the additional purchase rebate) on access to credit and use of financial tools and institutions, and the factors behind limited use of debit card. We will do so by answering two main questions. On the one hand, we will uncover the determinants of debit card use beyond simple extraction of cash from ATMs: despite substantial financial incentives, beneficiaries may have difficulties finding stores that accept debit cards, and/or the latter might be more expensive than more informal stores, among other reasons. On the other hand, we aim to study the impact of increased access to debit cards and bank accounts on beneficiaries' credit use and subsequent socioeconomic outcomes. We will study whether and how the payment of benefits by means of debit cards fostered financial inclusion by studying whether debit card adoption has led to beneficiaries' increased access to credit, and, indirectly through this potential inclusion effect, to higher levels of formal salaried and formal self-employment. These questions will be addressed by means of several additional data sources. The first is a survey of beneficiaries and experiment participants to unearth longer-term impacts and reasons for limited use of debit cards. Second, we match the original experimental sample with 60 months (including 36 post-treatment months) of data from the Central de Deudores (Banco Central de la Republica Argentina), an open monthly database of all individuals engaging in credit activities (mostly bank loans and credit card use), and administrative records on formal employment and formal self-employment.

This paper builds on and will contribute to the literature on financial inclusion in developing countries (Burgess et al., 2005; Reddy et al., 2013; Bruhn and Love, 2014; Callen et al., 2019; Higgins, 2019; Kast and Pomeranz, 2014; Bachas et al., 2021), on the use of information campaigns to increase take-up and participation (Blanco and Vargas, 2014; Chareyron et al., 2018; Reyes et al., 2021), and on the broader literature on the impacts of electronic means of payments in developing countries (Suri and Jack, 2021).

The paper is organized as follows. We first present a discussion of the institutional context of the program and a motivation for this study (Section 2). Section 3 discusses the experimental setup. Section 4 presents the empirical results, and Section 6 concludes.

## 2 Institutional Context

Launched in November 2009, AUH is a massive, non-means-tested conditional cash transfer program that reaches 3.9 million children in 2.1 million households and represents approximately 7% of total national expenditures (including contribution-based family benefits; Ministerio de Hacienda, 2017). The AUH is Argentina's largest conditional cash transfer program and covers four million beneficiaries, about 40% of the country's 0-17

year olds. As such, it is one of the most important components of the country's current social safety net. Recipients are the parents or legal guardians of children under age 18 who were unemployed, worked in the informal economy, worked under specific tax regimes designed to encourage formalization of low-skilled workers, or were participants in selected employment programs. Households that receive this kind of assistance make up the most vulnerable portion of the population in Argentina (the last available poverty headcount estimate was 25.7%, which rises to 39.7% for children aged 0-14). In particular, 84% of children who receive the allowance belong to the two poorest quintiles of the household income distribution and specifically to the first three deciles.

About 95% of the 2.1 million recipients receive the benefit by means of a transfer to a government-provided bank account. The monthly allotment is \$1,694 Argentinian pesos per child, but beneficiaries receive only 80% of this amount each month (roughly 13% of the mean monthly household income of the second decile of the income distribution). In December, at the end of the school year, they receive the remaining 20% transfer accumulated during the year, conditional on fulfillment of a vaccination plan, health check-ups for children under six, and certified school-year completion for school-age children.

Beneficiaries of this transfer are assigned a special savings account into which their benefit is deposited each month in the name of the legal beneficiary. They are also issued a debit card (from one of the two major worldwide card providers) which they could use to withdraw cash from ATMs or to make purchases at stores equipped with Point-of-Sale (POS) terminals. All stores in Argentina are required by the national tax authority to accept card payments, because tax evasion is much more difficult on these payments (Brockmeyer and Saenz Somarriba, 2022).

Compliance with this requirement has been heterogeneous among small and middle-sized stores, however. In an effort to provide additional support to poor families and, at the same time, promote formality and reduce tax evasion, in 2016 the government established an additional transfer for AUH beneficiaries through purchases made with debit cards. The purpose of the program was to target transfers to the neediest population. At the same time, the transfer provided an additional incentive for beneficiaries to spend their benefits in the formal sector of the economy because purchases made with debit cards (in general) cannot avoid VAT and other taxes. Beneficiaries received an additional transfer into their accounts if they used their government-provided debit card to make purchases.

However, the use of debit cards for purchases among beneficiaries was not widespread, despite this substantial financial incentive. In February 2018, only 248,808 beneficiaries of the AUH program (out of nearly 3.9 million children and their 2.1 million parents/legal guardians) used their government-provided debit cards to purchase goods and thus received the additional transfer (the debit-card-purchase rebate). The total of funds transferred was less than 3% of the actual budget allocated to this program for beneficiaries,

and only about 20 to 30% of beneficiaries received this additional transfer during the year of our study, 2018. The vast majority chose to withdraw cash the day it was deposited in their accounts and to forego the additional benefit. If the typical eligible household, composed of two adults and two minors, had taken advantage of the full debit-card-purchase rebate, their income would have increased by about 5.6%, rendering this a non-trivial supplement to household income.

## 3 Data Sources and Experimental Setup

### 3.1 Data Sources

The benefit take-up analysis of the original experiment focused on differences in impact on debit card use by treatment, with sub-treatments designed to capture elements identified in the benefit take-up literature (stigma, information, salience, etc.). The analysis in this paper focuses on the aggregate effect of the information campaign – i.e., the focus will be the pooled treatment effect without distinguishing the subtle differences between sub-treatments.

The main outcome of interest is use of debit card for purchases—i.e., whether beneficiaries used the debit card to purchase goods (required for them to receive the rebate) rather than simply to withdraw cash from their accounts and using the cash to make purchases. The social security administration (ANSES), our partner in the implementation of this project, and the tax authority (AFIP), which is responsible for transferring the rebate, keep monthly records of which beneficiaries used the program because it is responsible for depositing the rebate in each beneficiary’s bank account. This was how we measured our main outcome of interest: a binary variable indicating take-up or non-take-up of the program during the month when the beneficiary received the message about the rebate, either by text message or through the ATM screen. The main data sources were thus ANSES and AFIP administrative records.

We add two key dimensions missing from the previous analysis. The main research question will be to gauge the impact of increased debit card use (beyond the additional purchase rebate) on access to credit and use of financial tools and institutions. This will be based in matching the original sample with 60 months (including 36 post-treatment months) of data from the Central de Deudores (Banco Central de la República Argentina), an open monthly database of all individuals engaging in credit activities (mostly bank loans and credit card use). As additional outcomes that might result from increased credit and financial inclusion, we will establish whether the campaign had any impact on formal salaried and self-employment. We will draw on existing sources of information that can be matched with our individual-level beneficiary data: formal salaried and formal self-employment inferred from credit rating agencies’ data (that routinely access social secu-

rity databases) and from the tax authority record (“padr<sup>3</sup>nj)oftaxpayerswhoengageinformalself-employment(1autonomosjandimonotributoj<sup>3</sup>asimplifiedtaxregime).

The experimental sample was randomly drawn from a subject pool of more than two million adults who were legal recipients of the AUH program. The total experimental sample consisted of 400,723 individuals, with 205,000 in the control group and 195,723 in the pooled treatment group. We have information on debit card use through 2018 and 2019, eight months before and eight months after the intervention (conducted in October 2018).

## 3.2 Experimental Setup

The causal parameter we estimated was the effect of providing information about the debit-card-purchase rebate for beneficiaries of AUH conditional cash transfers on the take-up of this rebate. We expected the parameter to have a positive sign: exposure to information about the rebate should increase debit card use.

The social security administration (ANSES), our partner in the implementation of this project, routinely runs massive communication campaigns via text messages sent to beneficiaries’ cell phones, and most of our information treatments were therefore conveyed in this way. Moreover, virtually all beneficiaries of the cash transfer go to an ATM to retrieve cash from the benefit. The social security administration has the ability to display messages for beneficiaries when they insert their debit card in the ATM. We used both these mechanisms to convey a series of short messages about the debit-card-purchase-rebate program.

Regarding heterogeneity, we expected that treatments might differ in their impact according to factors that might determine patterns of debit-card use or adoption. The use of the debit card and the potential effects of our treatments may have been related to individual characteristics such as age (younger women may have higher levels of financial literacy but also less experience with financial institutions). Most importantly, debit-card use may vary by previous exposure to financial institutions. For instance, individuals with some credit history by definition interacted with these institutions, and beneficiaries who were formal employees in the past might have had bank accounts because employers made them compulsory for some types of employees to receive their wage payments. We expected large variations in debit-card use by geographical area (for instance, by province, because different levels of economic development were involved), or by financial institution (some beneficiaries were assigned to small or large private banks, whereas others received their accounts and debit cards from national or provincial banks, whose policies might have differed with regard to using debit cards, for example). Alternatively, differences might be more local, which we proxied by characteristics of the specific bank branch assigned to the beneficiary: this may vary by branch size, or some branches might

be in areas with greater opportunities to use debit cards (for instance, supermarkets).

Finally, use of the debit card and the potential effects of our treatments may have been related to individual characteristics such as age (younger women may have higher levels of financial literacy but also less experience with financial institutions). Most importantly, debit-card use may vary by previous exposure to financial institutions. For instance, individuals with some credit history by definition interacted with these institutions, and beneficiaries who were formal employees in the past<sup>2</sup> might have had bank accounts because employers made them compulsory for some types of employees to receive their wage payments. The analysis of heterogeneity along these dimensions is presented in Section 4.2.

### 3.3 Experimental Sample and Treatment Groups

We selected our subject pool from the subset of the 2.1 million AUH beneficiaries (parents and/or legal guardians, more than 90% of whom were women) with valid mobile phone numbers, and sent information messages by text messages for most of our experimental sample. An additional group was shown a message on ATM screens. While the phone numbers were valid, we cannot know whether these messages were actually received by beneficiaries or if they were read. Our estimates were Intention to Treat rather than Treatment on the Treated.

We devised four distinct treatments, divided into eight sub-treatments with two large control groups. The content of the messages was displayed in Table 1. The two channels available (text messages and ATM screen messages) only allowed a limited number of characters, so information had to be conveyed in a very succinct way. The sample sizes were 30,000 for the *baseline* treatment (text messages); 60,000 for an *information* treatment composed of two text messages (*stores* and *no cash*, with 30,000 recipients each); 80,000 for the *salience* treatment (four subgroups according to the number of beneficiary children of the recipient, 20,000 recipients); and 30,000 for the *channel* treatment (ATM screen).

Finally, the implementation agency did not authorize separate control groups for each of the treatments, with the exception of Treatment 4. Instead, our partner offered a large control group of 180,000 for the text-message treatment (1 to 3), and a further 30,000 beneficiaries served as controls for the ATM *channel* treatment (see below for this specific group). These 210,000 individuals received no messages at all. The *salience* treatment groups were selected, in the sense that the number of children was correlated with debit-card use. If we had had four distinct control groups by number of children, with 20,000 observations for each these treatments, our full sample of treated and controls would have been balanced in terms of the number of observations. Instead, the *salience* treatment

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<sup>2</sup>Note that AUH beneficiaries must not be formally employed at the time of receiving the benefit.

groups were over-represented. There were not, for instance, 20,000 beneficiaries with four children in the control group of 180,000. The regressions pool all observations and include either individual fixed effects (panel regressions) or a basic set of controls (last digit of ID number and dummies by number of children—*basic controls*)<sup>3</sup> or individual fixed effects, which controlled for the same differences and ensured that the treatment and control groups were balanced. Our final sample, as described in Table 1, included just over 400,000 observations.<sup>4</sup>

Our treatments (text messages and ATM information campaigns) were applied in the first weeks of November 2018, so that they potentially influenced debit-card-purchase rebates for the months of November and December 2018. The program was discontinued in January 2019. Our pre-treatment period was January to October 2018, whereas the post-treatment period for the full sample was November and December 2018. Finally, for a large state-owned bank, which was used by about a third of our experimental sample, we managed to get additional data for the first six months of 2019. For this additional sample, we therefore had 10 pre-treatment months and eight post-treatment months—two months while the program was in place and six months after it was discontinued.

Finally, in June 2019 ANSES conducted an online survey via email on electronic means of payment in which we were able to introduce a series of questions related to the experiment. Only about 8,000 of our total total subject sample replied to this survey, although these respondents were balanced between our treatment groups. While there are likely issues of selection (starting by those who used email regularly and replied to ANSES' request), we still consider these results to be valuable. We discuss these results in Section 4.4.

## 4 Empirical Results

### 4.1 Overall Impact of the Experiment

To present the overall impact of our experiment, we have provided monthly estimates of the difference in debit-card use for purchases among individuals, the control groups, and the treated, pooling all sub-treatments into one indicator.<sup>5</sup> Figure 3 presents simple

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<sup>3</sup>AUH beneficiaries are paid at the beginning of the month according to the last digit of their national ID number – 0 on the first working day of the month, 1 on the following, etc. We sent a text on payday and another exactly one week later. For logistical reasons, ATM-screen messages were sent only to those with digits 5 to 9, which is why a specific control group was established for this channel. These messages were shown only once when beneficiaries made their first transaction at an ATM.

<sup>4</sup>Our original target was 410,000, but some beneficiaries were dropped in the several revision and matching iterations of the administrative data handling process—different controls were applied at different stages (this 2.3% attrition was evenly distributed across groups).

<sup>5</sup>Baseline debit-card use varied by number of children. Consequently, we included a series of basic controls in all regressions: indicators by number of children and for the last digit of the national ID card, which determined the day of the month on which the benefit was deposited. The panel estimates

estimates of treatment versus control differences from independent, month-by month OLS regressions with a set of basic covariates specified above to control for differences in the compositions of the groups. Figure 2, conversely, presents coefficients from the panel regression with individual fixed effects, which pools all months and all observations and effectively controls for all time-invariant observable and unobservable characteristics of individuals.

The evolution over time of the treatment effects is depicted in Figure 3, panel A. In the pre-treatment months, there was basically no difference in debit-card use between the controls and the pooled treatments. While some of the estimates were marginally significant, this was expected with our very large sample sizes. The results for January-October were reassuring because our main outcomes seemed balanced. Figure 3 also indicates that our treatments were all successful in increasing debit-card use (and subsequent receipt of debit-card-purchase-rebate transfers). The effect for November was about 1.4 percentage points and about 1 percentage point for December.

Panel B in Figure 3 presents estimates for the subsample for which we had six additional months (the “one-third” sample). While the confidence intervals were wider than they were for the full sample, as expected, the treatment effects for November and December were very similar. Moreover, even after the debit-card-rebate benefit was discontinued in 2019, the coefficients were still positive and statistically significant until March, although they clearly decreased over time.

This overall impact of the experiment is confirmed by the results presented in Table 2. These panel regression estimates were based on observations for the full 12 or 18 months, depending on the sample. The regressions included individual fixed effects, and standard errors were clustered at the individual level to account for serial correlation. The overall treatment effect for the full sample was 5.1 percentage points (Column 1), from an adjusted baseline of about 20.5 for the control group. This effect implied an increase of about 25% in debit-card use as a result of our information campaign, though baseline levels were low. Column 2 presents the estimates for the same initial 12 months (10 pre-treatment and two post-treatment) for a restricted sample of the large bank for which we had additional information. The effect was larger, at about 6.2 percentage points, from a higher baseline of 22.1 (a proportional effect of about 28%).

Finally, the coefficient in Column 3 reflects the decline over time of the treatment effect depicted in Figure 3, Panel B. When we included the additional six months (during which the program was no longer in place), the overall effect fell to about 3.8 percentage points, which was smaller but still an increase of about 16.7% with respect to the control group.

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accounted for these differences by including individual fixed effects.

## 4.2 Heterogeneous Effects

We also conducted a series of analyses of heterogeneous effects to better understand the mechanisms behind our findings. The figures discussed in this section correspond to panel estimates of the pooled treatments akin to those in Table 2 but for different subgroups. The rationale for studying these dimensions separately is discussed in Section 3.

In Figure 4, Panel A, we present the treatment effects by province. The treatment effects and take-up levels were roughly similar for the four groups, with larger effects for Buenos Aires Province. Panel B shows that debit-card use was substantially higher for the control groups of larger institutions—from about 20% to 25%, compared to between 15% and 20% for the rest of the banks. This seems to indicate that the supply side (availability of point-of-sale equipment in stores, which was larger in richer and denser areas) was probably one of the factors that affected both the use of debit cards for purchases and the success of campaigns like ours. The treatment effect was substantially larger for the Banco Provincia de Buenos Aires, which ran a campaign with substantial discounts for card payments in supermarkets; our treatment may have acted as a reminder of that very advantageous program.

In Figure 5 we present the results by characteristics of the beneficiary’s bank branch. Panel A shows that average debit-card use for the control group in branches where use was below the median was about 12% as compared to about 26% for those above the median. While proportionally similar, the effect of our campaign was about twice as large in the latter group: an increase of 6.32 percentage points compared to 3.36 for the low. While several factors, such as culture and peer effects, might have influenced use at a specific branch, it is likely that the main determinant was the availability of POS and other infrastructure for the use of debit cards for purchases. Panel B in Figure 5, indicates that little difference seemed to exist between large branches (i.e., those with above the median number of beneficiaries) and small branches.

Finally, Figure 6 shows the results of our analysis of heterogeneous effects by individual characteristics. In Panel A, we illustrate the effect for beneficiaries above and below the median age in our sample. Older beneficiaries were slightly more likely to use their debit cards for purchases. Panel B exploits information about whether subjects were formal employees (registered with the SSA) in the five years preceding our experiments. Employers are required to open bank accounts (“*cuenta sueldo*”, literally wage accounts, with attached debit cards) for their formal employees, so formal employment history should signal previous contact with the financial system. This type of account was not compulsory for domestic workers, however, who constituted a large fraction of our sample with previous formal employment. Moreover, those with previous formal employment were probably the better off among AUH beneficiaries. However, the level of debit-card use for controls in the two groups was fairly similar (about 22% for those with some

formal employment in the past, compared to about 20%). While the treatment effect was larger (6.95 vs. 4.73 percentage points) among those with past formal employment, there was no substantial heterogeneity between the two groups. In Panel C, we show treatment effects for individuals who had some credit history (i.e., they appeared in the Central Bank’s database as having used some kind of credit product in the previous five years) and those who did not. Surprisingly, the level of take-up was again fairly similar for controls in the two groups: about 23% for those with some recent credit history compared to about 19% for those without. The treatment effects were again larger for the expected group (5.57 vs. 4.84 percentage points), but the difference was not substantial.

Panel D compares beneficiaries who did not make any purchases with their debit cards in the pre-treatment period (January to October 2018) to those who did so at least once. The level of take-up between the controls in the two groups was different by orders of magnitude: about 1% for controls in the group who had not used their cards before (suggesting very low churning or adoption of cards over time) compared to 40% for controls in the other group. The effect of the treatment, however, was again about the same for the two groups: 5.3 percentage points for those who had used their cards previously compared to 4.94 for those who did not. The latter result was remarkable: if lack of information or *saliency* were the most relevant barriers to debit-card use, we should have observed a much larger absolute effect for this group, for whom it would have represented real news. This evidence suggests the presence of financial inclusion barriers to take-up—i.e., a lack of opportunities to use debit cards for purchases in the areas where the beneficiaries resided.<sup>6</sup>

### 4.3 Access to Credit and Banking and Socio-Economic Outcomes

Table 3 presents the results of the impact of our treatments on access to credit, measured as appearing on the Argentine Central Bank’s database of debtors, which includes all formal credit products—mainly credit cards but also mortgages and other secured or collateralized loans (such as cars). The outcomes are divided into results in the short run (2019 - the first year after the treatment) and the longer run (2020-2021).

We can see that the informational treatment did not have an impact on the overall probability of having formal credit after the treatment period. Of course, use of credit products per se is not necessarily a positive outcome, since beneficiaries may end up in credit traps (Agarwal et al., 2021), either in the short run or in the longer run. This is also true when the outcome is having a bad credit outcome (unrecoverable credit) in the

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<sup>6</sup>It should be noted, however, that the lack of heterogeneity between groups on the basis of baseline use of debit cards can be rationalized if the marginal users in each group might be similar. The heterogeneity comparison would not be picking up differences between average individuals in each group.

database. Increased use of electronic means of payments does not seem to have induced better use of credit from beneficiaries.

Could the increased use of formal credit lead to other positive outcomes? While far-fetched, one possibility is a direct effect: an increase in the use of credit might lead to greater formalization (for instance, through entrepreneurship) – or, alternatively, increased credit use might indirectly lead to higher formalization (e.g., greater contact with formal stores and companies). We only have one outcome in the database that can capture this effect. Table 4 presents the effects of our treatment on text-message recipients’ formal employment two years after the treatment. The results are presented for formal self-employment (column 1), formal salaried employment (column 2) or any of the two (column 3). As we can see from the table, there are no statistically significant effects of our treatment on employment. Again, it might be too much to

#### **4.4 Experimental Results: Follow-Up Survey**

As described in the data sources section, the social security administration conducted a massive survey by email in June 2019, about eight months after our intervention. The questionnaire included a series of questions related to this project. Table 5 presents the results in some of the key outcomes measured in this survey and related to our experiment.

Most notably, as indicated by the results in the first column, even eight months after our intervention, we find a positive treatment effect of two percentage points in knowledge about the fact that debit cards can be used for purchases, albeit from a high baseline level of 86%. The treatment also has a negative but not statistically significant effect on the proportion of individuals who report withdrawing all of their cash upon payment of the benefit (although this effect is significant for one of the treatment arms -- not reported). Moreover, there is a positive and significant effect of about two percentage points on the proportion of respondents who report using online banking, from a low baseline level of 22.5% for the control group, indicating some sort of spillover from our treatment to other forms of more complex or sophisticated use of technology.

Finally, after our treatment ANSES implemented a new program of soft/low interest loans (basically, advances on future benefit payments). More than 87% of survey respondents report having received a credit from this new program, although there is not a statistically significant difference between treatment and controls. Treated individuals do not seem to have profited more from new form of credit, although the take-up.

## **5 Mechanisms and Underlying Factors**

Finally, Table 6 includes responses to a specific question on why beneficiaries do not use their debit card for purchases. The most cited reason is a plain preference for cash (58%),

followed by the fact that 40% of respondents state that they do not carry their debit cards with them, which points to the hassle of using the card (beneficiaries reported in focus groups that losing the card implied a huge administrative cost in getting it re-issued, receiving it, etc.) and the potential problems of citizen security as limiting factors. Only a minority (17%) report having difficulties in understanding how to use the card, or in stores not accepting it (19%) or charging a fee for its use (13%).

Users not carrying their debit cards might be due to administrative hassle of getting a replacement, and/or issues of crime and citizen security. We do not find substantial differences between users of large public banks (usually rated as more bureaucratic or less efficient), small public banks or private banks in their preference for cash or in the fact that they do not carry their cards with them.

However, about 42% of beneficiaries from the five provinces with the highest levels of reported thefts per population in 2018<sup>7</sup> report not carrying their cards, compared with only 35% from the bottom five provinces in terms of crime levels, with no significant differences in terms of preference for cash.

## 6 Conclusion

Our experiment provided information about the effectiveness of an information campaign conducted via text messages or through on-screen messages at ATM machines. The campaign increased purchases with debit cards and subsequent rebates significantly but not substantially in the short run. However, beneficiaries who increased their use of debit cards do not exhibit a higher probability of having access to credit through the financial system, nor do they exhibit higher levels of formal employment. The results indicate that cultural factors (a preference for cash), administrative hassle and citizen security issues are relevant issues (besides lack of information and of access to point-of-sale terminals in stores) that limit the potential of financial inclusion through increased use of digital means of payment. This suggests a need to streamline the verification and replacement process. This also points out that financial inclusion must be approached from a wide perspective, including issues such as safety and crime.

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<sup>7</sup>Source: Base Sistema Nacional de Informaci3n Criminal, MinisteriodeSeguridad, Argentina, 2022.

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# Tables

Table 1: Treatment and Control Groups: Sample Sizes and Message Content

Group	Message content	Group size
<b>Pooled Treatment group</b>	Information messages	195,693
<b>Pooled Control group</b>	No message	205,030
<b>Total experimental sample</b>		<b>400,723</b>

Notes: Total number of observations by group.

Table 2: Treatment Effects on Debit Card Purchases

	Full Sample (12 Months)	1/3 Sample (12 Months)	1/3 Sample (18 Months)
All treatments (Pooled)	5.129*** (0.064)	6.209*** (0.112)	3.746*** (0.084)
Constant	20.461*** (0.005)	22.139*** (0.009)	22.419*** (0.018)
Observations	4,808,676	1,580,412	2,370,618
Number of Individuals	400,723	131,701	131,701
Controls	FE	FE	FE

Robust standard errors in parentheses

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Clustered at the individual level

Table 3: Treatment Effects on Credit Access

	(1)	(2)	(3)	(4)
	Ever CENDEU-ST	Ever CENDEU-LT	Bad status-ST	Bad status-LT
All treatments	-0.003 (0.002)	-0.002 (0.003)	-0.003 (0.002)	-0.001 (0.002)
Constant	0.425*** (0.003)	0.566*** (0.003)	0.208*** (0.003)	0.317*** (0.003)
Observations	322,254	322,254	322,254	322,254

Robust standard errors in parentheses

\*\*\* p&lt;0.01, \*\* p&lt;0.05, \* p&lt;0.1

Table 4: Treatment Effects on Formal Employment

	(1)	(2)	(3)
	Formal self employed	Formal-salaried	Any formal employment
Treatment group	0.001 (0.001)	-0.000 (0.001)	0.001 (0.001)
Constant	0.125*** (0.002)	0.058*** (0.001)	0.177*** (0.002)
Observations	322,254	322,254	322,254

Robust standard errors in parentheses

\*\*\* p&lt;0.01, \*\* p&lt;0.05, \* p&lt;0.1

Table 5: Treatment Effects on Knowledge about Debit Cards and Related Outcomes, Follow-Up Survey

	(1) Knows About Debit Purchases	(2) Extracts All Cash From ATM	(3) Uses Online Banking	(4) Has ANSES loan
All treatments	2.03*** (0.72)	-1.01 (1.07)	2.09** (0.92)	0.05 (0.71)
Constant	92.89*** (2.12)	36.68*** (4.10)	25.96*** (3.73)	95.56*** (2.01)
Observations	8,788	8,788	8,788	8,788
Control Group Mean	86.117	55.508	22.501	87.494

Robust standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Notes: Robust standard errors in parentheses.

Table 6: Reasons for Not Using Debit Card for Purchases (Follow-Up Survey)

<i>Why do you not use your program's debit card for purchases?</i>	
I prefer cash	58%
I don't carry the card with me	40%
I don't know how to use the card/complicated	17%
The stores I frequent don't accept it	19%
I get a surcharge for using the card	13%
(Non-exclusive options)	

# Figures

Figure 1: Advertising Campaign Sample

### ¿Cómo saber si la tarjeta es de débito o de crédito?

DÉBITO ✓	CRÉDITO
Está incluida en tu cuenta gratuita de la Seguridad Social	No está incluida en tu cuenta de la Seguridad Social y tiene costo
Tiene el reintegro del 15% en las compras	No tiene el reintegro del 15% en las compras
No podés pagar en cuotas	Pago en cuotas con y sin intereses
Permite retirar dinero de los cajeros sin costo	Permite retirar dinero de los cajeros, con costo

[/ansesgob](#)

130 [anses.gob.ar](http://anses.gob.ar)

Todos los trámites son gratuitos.

### Si comprás con tu tarjeta de débito, te devolvemos el 15%

No necesitás hacer ningún trámite.  
Accedés al reintegro si hacés compras con tu tarjeta de débito de la cuenta de la Seguridad Social.

**El beneficio es válido en todos los comercios.**

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### ¿A quiénes les corresponde?

- Jubilados y pensionados que cobran la mínima
- Titulares de la Asignación Universal por Hijo
- Titulares de la Asignación por Embarazo
- Titulares de pensiones no contributivas
- Personal de servicio doméstico
- Titulares de Becas Progresar

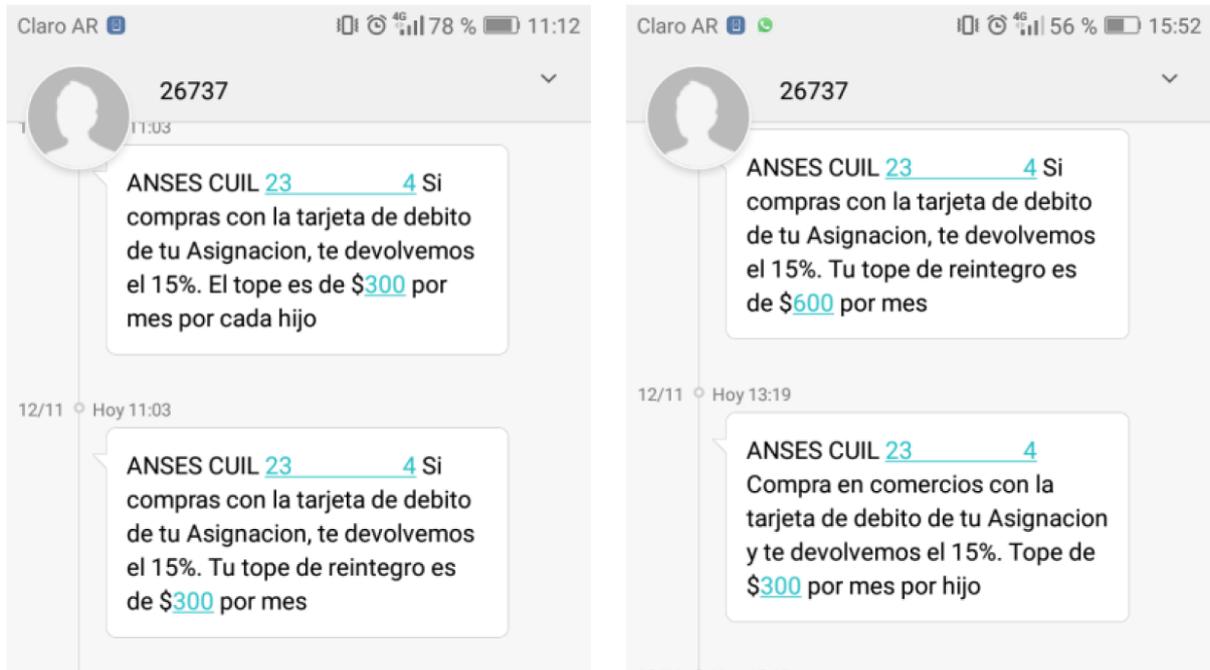
Para consultar si podés acceder al beneficio, ingresá con tu CUIL a: [www.anses.gob.ar/compraondebito](http://www.anses.gob.ar/compraondebito)

### ¿Cómo acceder al reintegro?

- Para recibir el reintegro del 15%, comprá en cualquier comercio y pagá con tu tarjeta de débito de la cuenta de la Seguridad Social, presentando tu DNI.
- El reintegro tiene un tope de \$ 300. No se acredita en el momento de la compra sino que lo recibirás en tu cuenta al mes siguiente.
- Si sos titular de la Asignación Universal por Hijo o de la Asignación por Embarazo, el reintegro es de \$ 300 por cada hijo.

Note: Advertising campaign by ANSES.

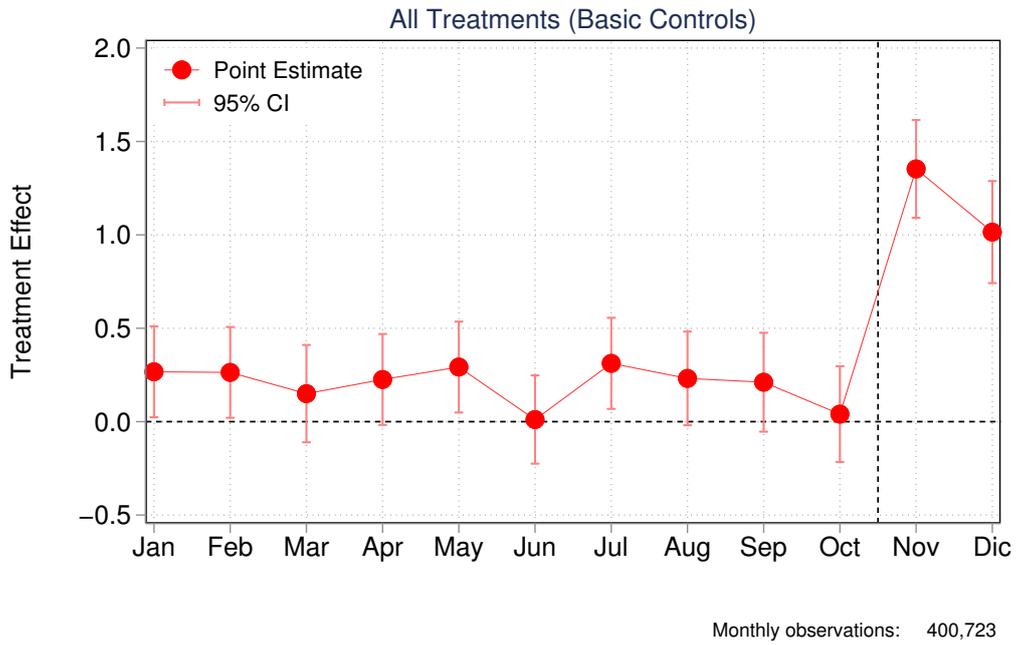
Figure 2: Screen Captures of Selected Text-Message Treatments



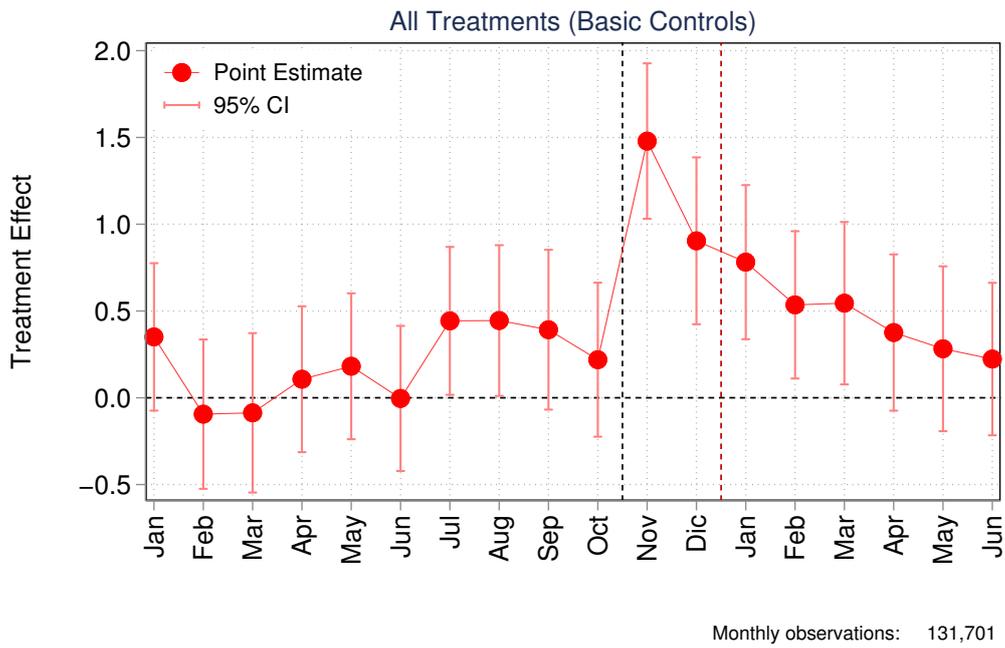
Note: Information treatments as displayed in beneficiaries' phones.

Figure 3: Treatment Effects on Proportion Using Debit Card for Purchases Use Over Time: Pooled Treatments, Full and One Third Samples (12/18 Months)

a. Full Sample (12 Months)



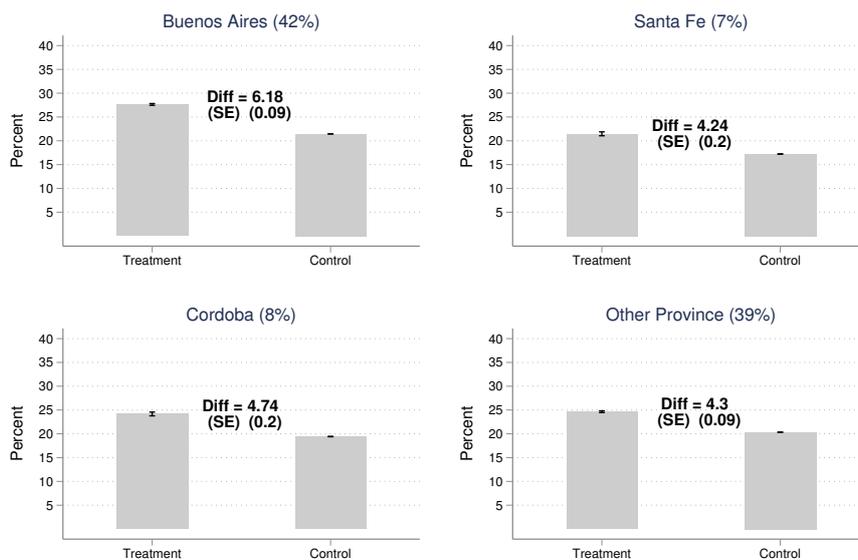
b. 1/3 Sample (18 Months)



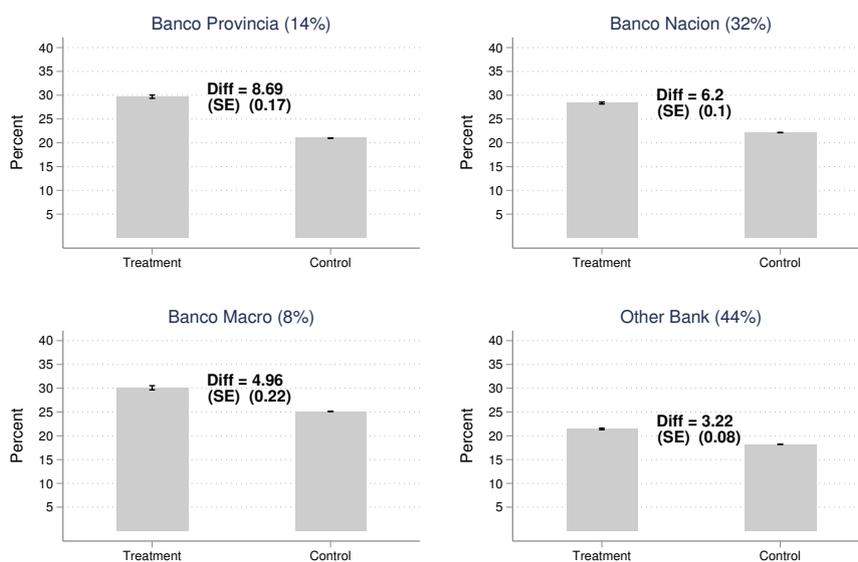
Note: Ninety percent confidence intervals represented by red lines, computed with robust standard errors.

Figure 4: Heterogeneous Effects By Geographical Area and by Bank

a. *By province*



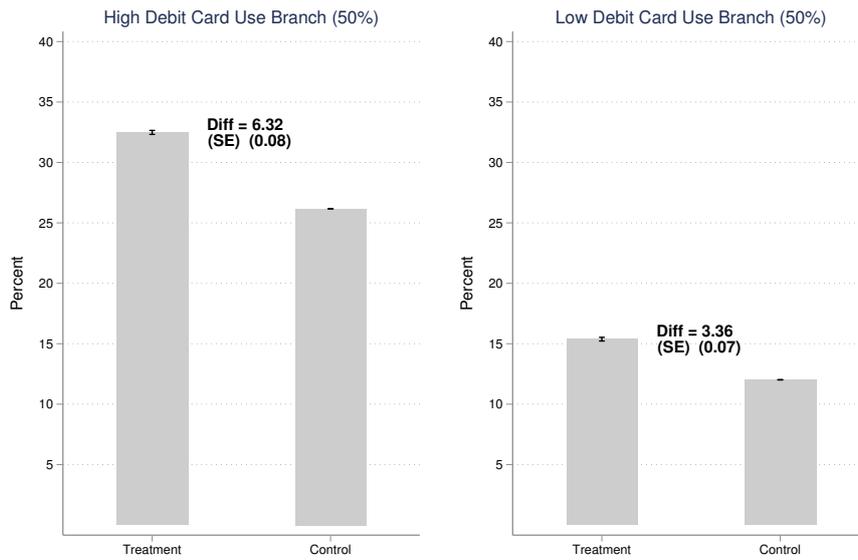
b. *By bank*



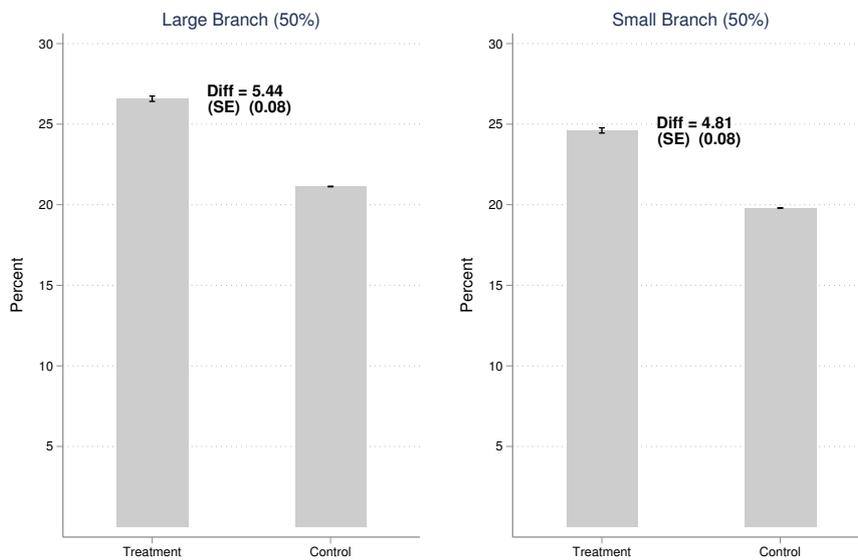
Notes: Ninety percent confidence intervals, represented by red lines, were computed with robust standard errors

Figure 5: Heterogeneous Effects By Geographical Area Bank Branch Characteristics

a. *High/low use of debit in branch*

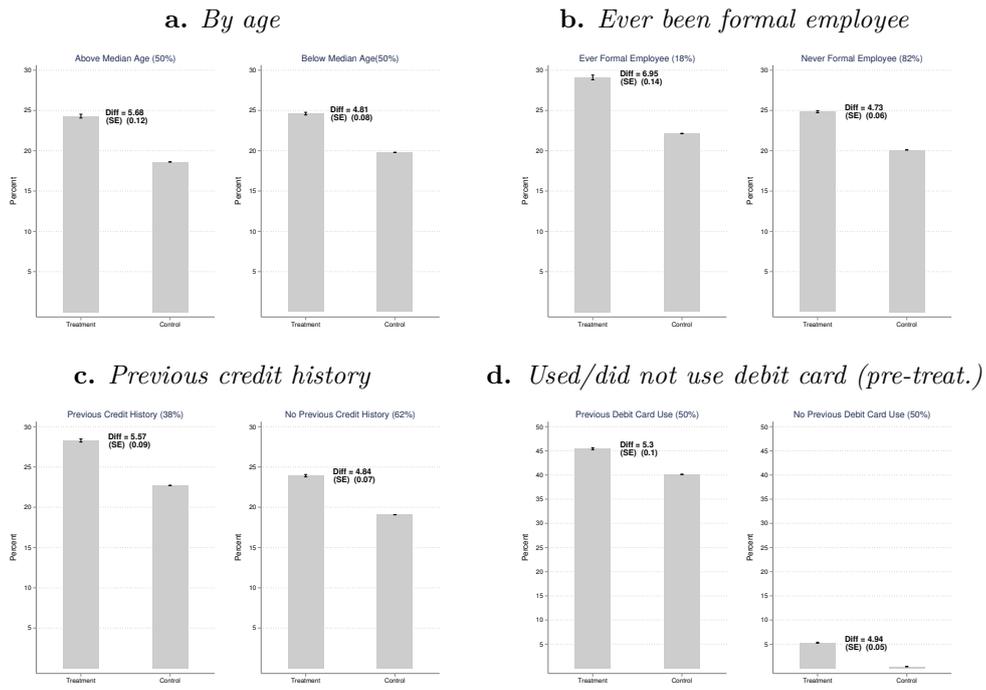


b. *Large/small branch*



Notes: Ninety percent confidence intervals, represented by red lines, were computed with robust standard errors.

Figure 6: Heterogeneous Effects By Individual Characteristics



Note: Ninety percent confidence intervals, represented by red lines, were computed with robust standard errors.