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Low Take-Up and Financial Inclusion: Experimental Evidence from Argentina's Cash Transfers

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Abstract

Cash transfer and other social protection programs have greatly expanded in developing countries in the last two decades, but their coverage varies greatly, even among eligible individuals. We studied the low take-up of 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). Beneficiaries are assigned a bank account and a debit card. 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 take-up (i.e., purchases with debit cards and subsequent rebates) significantly but not substantially. The results indicated that the benefit had low salience, that beneficiaries lacked information about the debit-card program, that frictions existed related to financial inclusion and lack of infrastructure, and that limited information and salience were important but second-order factors.

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Keywords: take-up of social benefits, financial inclusion.

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1 Introduction

Cash-transfer and other social protection programs have greatly expanded in developing countries in the last two decades. Their coverage varies greatly—not all eligible individuals participate, even in universal programs (Hanna and Olken, 2018). The issue of low take-up of benefits, which have been extensively studied in advanced economies (Currie, 2004; Finkelstein and Notowidigdo, 2019),¹ but have received less attention in low- and middle-income countries (Amior et al., 2012; Blanco and Vargas, 2014; Consultative Group to Assist the Poor, 2015; Gupta, 2017). A related issue is financial inclusion in the context of these programs and their target populations. 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., 2017; Callen et al., 2019; Higgins, 2019).

The target population of this study was legal recipients (parents and legal guardians) of the Universal Child Allowance (*Asignación Universal por Hijo*; hereafter, Spanish acronym: AUH), a conditional cash transfer provided by the Argentine federal government to children under the age of 18 whose parents are unemployed or work in the informal economy, among other special cases of vulnerability. 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.

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. Thus, efforts to ensure proper take-up of social benefits within this group are of utmost importance in the quest to reduce poverty and inequality and to guarantee proper access to health and education among Argentina's vulnerable citizens.

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.

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 estab-

¹See also Alba (2018); Bargain et al. (2012); Bettinger et al. (2012); Bhargava and Manoli (2015); Chareyron et al. (2018); Finn and Goodship (2014); Hernanz et al. (2004); Kleven and Kopczuk (2011).

lished an additional transfer for AUH beneficiaries through purchases made with debit cards.² 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 allowances 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 (see Figure 1 for an information leaflet).

Beneficiaries systematically failed to claim this substantial subsidy, however. 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.

The main research question that guided this paper was why eligible households and individuals failed to take advantage of programs that benefited them, which implied a reduction in their welfare and a loss of budgetary and administrative resources. 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.

Moreover, take-up in our specific case implied using debit cards to make purchases, and restrictions to financial inclusion thus constituted additional barriers to take-up. We posited that individuals may have failed to use debit cards connected with the transfer program because they did not want to be identified as beneficiaries when making purchases (stigma), because they did not have access to retailers equipped with debit-card readers, because prices were higher in these more formal stores, because they were unaware of the existence of the benefit, or because they feared that the government might use

²For reasons of fiscal federalism, the transfer is labeled a “VAT rebate of fifteen percentage points” out of the general rate of 21%. By labeling the transfer a VAT rebate, funds can be subtracted from the revenue generated by this tax that the federal government must transfer automatically to sub-national governments (provinces in Argentina). While this benefit is labeled a VAT rebate, then, it works as an additional cash transfer or benefit top-up, and, in fact official ANSES materials (see Figure 1) called it a “rebate for purchases made through the debit card associated to your benefit” because a review of beneficiaries by the authorities found that the VAT labeling only added confusion. We refer to the transfer interchangeably as a “VAT rebate” or a “debit-card-purchases rebate.” Note, however, that the transfer it did not affect VAT chain transactions in any way. That is, it was invisible to stores.

their shopping behavior to reduce future benefits. We designed and implemented a mechanism field experiment to test these hypotheses. The multi-treatment mechanism field experiment, with over 400,000 beneficiaries, helped distinguish among these explanations and unearth the extent to which more rational factors (costs, lack of information) and behavioral factors (inattention, stigma, misperceptions) explained the under-performance of the program.

The results indicated that the benefit had low *saliency*, and that beneficiaries lacked information about it. 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 *saliency*. The evidence points towards frictions related to financial inclusion and lack of infrastructure, with limited information and *saliency* factors important but second order.

This paper contributes to the recent literature on take-up of social programs in developed (Currie, 2004; Finkelstein and Notowidigdo, 2019) and developing countries (Amior et al., 2012; Blanco and Vargas, 2014; Consultative Group to Assist the Poor, 2015; Gupta, 2017) as well as financial inclusion in the developing world (Burgess et al., 2005; Bruhn and Love, 2014; Bachas et al., 2017; Callen et al., 2019; Higgins, 2019; Kast and Pomeranz, 2014). In a broader context, it contributes to recent advances on issues of public finance in developing and middle-income countries (Bachas et al., 2017; Gerard and Naritomi, 2019; Naritomi, 2019; Blanco and Vargas, 2014; Pomeranz et al., 2014).

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 5 concludes.

2 Context and motivation

2.1 Motivation

Social safety nets (hereafter, SSN) are a critical instrument in government attempts to improve the living condition of millions of vulnerable people around the world and especially in Latin America (Fiszbein and Schady, 2009; Robles et al., 2018). A growing body of unequivocal evidence shows that SSN programs are effective at reducing poverty and inequality and improving health and education outcomes among disadvantaged populations. According to a recent World Bank report spanning 142 developing and transition countries (World Bank, 2018), these countries spend an average of 1.5% of gross domestic product (GDP) on SSN programs. The Europe and Central Asia region currently spends the most on SSN programs (an average of 2.2% of GDP), followed by Sub-Saharan Africa, Latin America, and the Caribbean with an average spending of 1.5% of GDP, close to the global average. The lowest spending is in the Middle East and North Africa (1%)

and South Asia (0.9%).

Despite their efforts, all countries, especially developing countries, struggle to close coverage gaps and reduce the non-take-up or incomplete take-up of social benefits. In low-income countries, estimates have shown that, on average, only 19% of the poorest quintile receive any kind of social assistance, while lower-middle-income countries manage to cover roughly half of the poorest quintile. Low coverage and low take-up rates of welfare benefits reduce governments' capacity to anticipate accurately the financial costs of welfare, lead to unjustified disparities of treatments among eligible individuals, and decrease the probability that SSNs will attain their goals of alleviating poverty and inequality (Hernanz et al., 2004).

Cash transfer and other social protection programs have greatly expanded in developing countries in the last two decades, but coverage varies greatly—not all eligible individuals participate, even in universal programs. The issue of low take-up of benefits, which has been extensively studied in advanced economies, has received less attention in low- and middle-income countries. The puzzle of why eligible households and individuals fail to sign up for programs that would benefit them is a top policy concern for countries struggling against poverty and social exclusion. When SSNs fail to reach the people who are entitled to them, potential recipients are unable to realize their rights. Gaps in performance can be attributed to flows in the technical design of targeting as well as implementation issues that impede proper identification of targeted population. Even when identification is possible, programs may not be attractive enough to offset the opportunity costs connected to participation. While targeting techniques have improved in recent years with the development of increasingly sophisticated means-testing methods, analytical work that examines the true causes and possible remedies of non-take-up or incomplete take-up of social benefits in developing countries remains scarce.

2.2 Institutional Context and Target Population

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). AUH is one of the most important components of the Argentine 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. 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.

The experimental sample was randomly drawn from a subject pool of more than two million adults who were legal recipients of the AUH program.

2.3 Related literature

Evidence on the problem of low-take-up of social benefits has been mixed and has mainly focused on the experience of developed countries. In broad terms, the empirical literature has proposed three possible causes for low or incomplete take-up: welfare stigma (Moffitt, 1983), transaction costs, and imperfect information about eligibility (Daponte et al., 1999).

More recent evidence, however, has pointed out that stigma might be the lesser culprit. Low-take-up rates have been pervasive in social programs in the US and the UK for both means-tested programs and non-means-tested programs (Currie, 2004; Finkelstein and Notowidigdo, 2019). In particular, a new body of evidence complements these explanations with the existence of “psychological frictions” associated with low program awareness, confusion, aversion to complexity, and other behavioral biases (Bertrand et al., 2006; Bhargava and Manoli, 2015; Karlan et al., 2016). The issue of salience (Bordalo et al., 2012, 2013) has been studied for taxes in the US (Chetty et al., 2009), and for social security benefits in Norway (Brinch et al., 2017).

Reductions in informational barriers have been found to be causally relevant to improved take-up in some contexts but not others. By conducting a randomized field experiment on American tax payers who failed to claim tax benefits, Bhargava and Manoli (2015) showed that mere fact of receiving a mailing (timely information), simplification, and the heightened *salience* of benefits did increase claims, while attempts to reduce the perceived costs of stigma, application, and audits did not.

Bettinger et al. (2012) found that providing timely information was not enough to increase the enrollment of randomly selected potential applicants in college financial aid. Providing information coupled with assistance, however, did seem to improve the likelihood of college attendance, persistence, and aid receipt. Chareyron et al. (2018) conducted a similar field experiment with benefit claimants in France. A number of randomized controlled trials have been conducted in developing countries with the objective of testing the effectiveness of providing timely information via information “nudges” in promoting more efficient behavior from subjects in the presence of behavioral bias. Finkelstein and Notowidigdo (2019), in turn, evaluated the welfare impact of increasing take-up of SNAP in the presence of potential behavioral biases. They designed and implemented a

randomized controlled trial with elderly individuals, providing them with information and sign-up assistance. They found their intervention had a substantial impact and posited that the results pointed toward optimization frictions.

Karlan et al. (2016) showed that individuals in Bolivia, Perú, and the Philippines who received a reminder of their saving goals via text message or letter increased the likelihood of reaching their goals and the total amount saved. Bachas et al. (2017); Callen et al. (2019); Higgins (2019); Kast and Pomeranz (2014) presented evidence from field experiments designed to increase savings in developing countries, highlighting behavioral factors and issues of financial inclusion.

Experimental evidence that has specifically addressed the puzzle of low take-up of social benefits in developing countries remains scarce. Blanco and Vargas (2014) showed that providing eligibility information to Colombian conflict-driven internal refugees increased their take-up of benefits, although the effect was small. The lack of knowledge in the field has been particularly large in terms of interventions that document both the take-up of social benefits and financial inclusion. This study is intended to close this gap.

3 Experimental Setup

3.1 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 take-up. Because this was a mechanism experiment, we attempted to distinguish the underlying causes of non-take-up by implementing a series of sub-treatments and estimating a causal parameter for each of these. Our prior assumption was that simply providing information about the rebate (which occurred in all sub-treatments) should increase take-up and, therefore, should have about the same effect size. Statistically and economically meaningful differences between the underlying parameters served as a guide to understanding which of the factors at play was more relevant for non-take-up.

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.

The main outcome of interest was take-up of the debit-card-purchase-rebate benefit—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 tax authority (AFIP), which is responsible for transferring the rebate, keeps 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 commissioned a focus group of program beneficiaries to provide auxiliary information, inform and validate our hypotheses, and test our experimental treatments. We planned to carry out post-treatment surveys to establish the degree of knowledge and usage of debit cards.

3.2 Relevant Hypotheses and Treatments

We studied the effectiveness of providing information to encourage full take-up of the debit-card-purchase rebate among AUH beneficiaries. The rebate is provided only if beneficiaries use their government-provided debit cards to make purchases. Most recipients used the debit card to withdraw funds in cash and never to make purchases, resulting in lack of take-up of the debit-card-purchase-rebate benefit. The intervention was based on randomly assigned treatment arms and control groups. Each of the treatment arms consisted of a different message designed to capture the four main reasons identified for non-take-up of the debit-card-purchase rebate.

According to the literature and the results from a qualitative study we commissioned (see the relevant appendix), the reasons why beneficiaries may not use their debit cards in such a way as to receive a rebate included:

- They were unaware of the benefit’s existence (information). The benefit was not advertised, and social security bank accounts were limited: beneficiaries could only access their balances through ATM screens and did not receive printed or electronic statements,
- They were unaware of the benefit’s characteristics and its potential magnitude (*saliency*, inattention). The lack of information might be exacerbated by the fact that beneficiaries were not informed (by text message, email, or any other means of communication) when their rebate was deposited,
- They did not want to be identified as beneficiaries when making purchases. The program debit cards are printed with the social-security-administration logo and

thus can be associated with participation in a welfare program (stigma),

- They did not have access to retailers equipped with debit-card readers or it was not prudent for them to shop in these more formal stores because their prices were higher (cost/hassle).

These elements all emerged in the in-depth interviews in our commissioned qualitative study. The main research question was why eligible households and individuals failed to sign up for programs that would benefit them. The sub-treatments of this mechanism field experiment were designed to distinguish between some of the hypotheses mentioned above, to quantify their relative importance, and to establish the extent to which factors that were more rational (cost, lack of information) and behavioral (salience, inattention, stigma, misperceptions) explained the under-performance of the program.³

To address the hypotheses, we devised four distinct treatments, divided into eight sub-treatments with two large control groups. The content of messages had to be negotiated with the implementation agency, which is why some of the relevant hypotheses (such as stigma) were not addressed in the experiment.

Message content is displayed in 1. The two channels available (text messages and ATM screen messages) allow only a limited number of characters, so information had to be conveyed in a very succinct way. The analysis below is based on the following grouping of sub-treatments in a series of experiments:

1. 1. A *baseline* treatment that consisted of a simple text message with basic information about the program; for example, use of the debit card implies a rebate with a maximum amount per benefit (i.e., number of beneficiary children) per month;
2. 2. An *information* treatment, that was composed of two text messages that highlighted characteristics of debit-card use: a) that credit cards could be used to make purchases without using cash (*no cash*) and b) that credit cards could be used to make purchases and were accepted in all stores (*stores*);
3. 3. A *salience* treatment, that consisted of four text messages that highlighted the maximum total potential rebate (i.e., not per child) according to the recipient's number of beneficiary children;
4. 4. A *channel* treatment in which 30,000 beneficiaries were shown *baseline* information in ATM screens, i.e., when they used their debit cards to withdraw cash.

³Because women are the main beneficiaries of AUH, if their spouses have access to a bank account, they might have a reason to cash the benefit quickly and forgo the rebate. The bank accounts assigned to beneficiaries ("cuentas de la Seguridad Social"), however, have only one account holder. (We thank H. Djebari for this specific point.)

Moreover, we expected that treatments might differ in their impact according to factors that might determine patterns of debit-card use or adoption. We expected large variations in debit-card use by geographical area (for instance, by province, because there 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 past⁴ 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 4.3. (relevant hypotheses and treatments)

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).

⁴Note that AUH beneficiaries must not be formally employed at the time of receiving the benefit.

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 *saliency* 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 *saliency* treatment 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*)⁵ 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.⁶

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 ten pre-treatment months and eight post-treatment months—two months while the program was in place and six months after it was discontinued.

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 take-up (i.e., debit-card use for purchases) among individuals, the

⁵AAUH 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.

⁶Our 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).

control groups, and the treated, pooling all sub-treatments into one indicator.⁷ Figure 3 presents simple 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 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 columns 1, 3 and 5 in Table 2. These panel regression estimates were based on observations for the full twelve or eighteen 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 3 presents the estimates for the same initial twelve months (ten 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 5 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

⁷Baseline 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 accounted for these differences by including individual fixed effects.

points, which was smaller but still an increase of about 16.7% with respect to the control group.

4.2 Information versus Salience

The previous section established that, overall, our information campaign was successful in increasing debit-card purchases among AUH beneficiaries. We found small but significant and non-negligible effects. In this section, we report on the different effects of our treatment arms.

The main result is presented in Column 2 in Table 2. . The regression was equivalent to that of Column 1 (pooled treatments), but it presents the effects for each treatment separately. The pattern is clear: the lowest effect corresponded to the less salient treatment (i.e., the ATM channel, which was displayed only once while text messages were sent twice). While the difference with respect to the baseline text messages, our main benchmark, was small at about half a percentage point (4.266 versus 4.736), it was still statistically significant at the 5% level. The *information* treatment, in turn, was more effective than the *baseline* by about 0.4 percentage point, and the *salience* treatment had an impact of 5.578, more than 0.8 percentage point higher than baseline (all these coefficients were different in pairwise comparisons at the 1% level).

The results in Column 3, which cover the same twelve months but restrict the sample to the large bank for which we had information, indicate the same overall pattern. Differences between the baseline text messages and ATM messages were no longer statistically significant, however. Moreover, for this group, the *salience* message was substantially more effective than the baseline coefficients of 7.224 and 5.181, respectively. Finally, the results shown in Column 5 reinforce the pattern. While the effects were attenuated, the *salience* message was still clearly the most effective (4.407 percentage points, compared to 3.211 for the baseline). While the coefficient was larger than the baseline for the *information* treatment and smaller for the ATM treatment, the smaller sample size and the attenuated effects over time implied that these differences were not statistically significant.

Finally, Figure 2 depicts the evolution of the coefficients—obtained from monthly regressions as shown in Figure 3—for *baseline*, *information*, and *salience* over time. The three treatments exhibited the same pattern: larger for *salience* and, for both the full sample over twelve months (Panel A) and for the one third sample over eighteen months (Panel B), with a similar “rate of decay” over time for all three.

All in all, the results indicated that the *salience* and *information* messages were marginally more effective in increasing debit-card use than our simple benchmark baseline message. While these differences were statistically significant, and comparatively large in some cases, none of the three treatments induced a substantial change in take-up of the

benefit.

Finally, it should be noted that the *channel* treatment had a substantially smaller effect (of about half) than the *baseline* (text messages) treatment. This was probably due to the fact that, as described in Section 3.3, the ATM screen message was shown only one, whereas beneficiaries in the text-message treatments received two messages, one on payday and another a week later.

4.3 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 3.2.

In Figure 5, 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 6 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 6, indicates that little difference seemed to exist between large branches (i.e., those with above the median number of beneficiaries) and small branches.

Finally, in Figure 7 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 *salience* 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.

5 Discussion and conclusions

Our evidence indicated that lack of information and of salience of the benefit were limiting factors in the take-up of the debit-card-purchase-rebate benefit for cash-transfer beneficiaries. Our information campaign through text messages and ATM screen messages increased take-up significantly at the extensive margin, but the effect, while proportionally large, was economically small. Providing more detailed information about how the debit cards functioned led to larger effects, as did messages highlighting total potential benefits, but these statistically significant differences were relatively small. The evidence

regarding differences in debit-card use at the branch level pointed toward frictions related to financial inclusion and lack of infrastructure; limited information and salience were important but second-order factors.

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Tables

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

Group	Message content	Group size
Baseline treatments		
Baseline message-SMS	If you use your AUH debit card for purchases , you will get 15% back.	29.344
Baseline message-ATM	The maximum rebate is \$300 per month per child.	29.285
Information treatments		
Stores message	Buying with your AUH debit card, you will get 15% back up to \$300 per month per child. Valid in all stores.	29.324
No cash message	Make purchases with your AUH debit card, not using cash . You will get 15% back up to \$300 per month per child.	29.271
Salience (maximum rebate) treatments		
1 child	If you use your AUH debit card for purchases, you will get 15% back. Your maximum rebate is \$300 per month .	19.485
2 children	If you use your AUH debit card for purchases, you will get 15% back. Your maximum rebate is \$600 per month .	19.601
3 children	If you use your AUH debit card for purchases, you will get 15% back. Your maximum rebate is \$900 per month .	19.674
4 children	If you use your AUH debit card for purchases, you will get 15% back. Your maximum rebate is \$1200 per month .	19.709
Control groups		
SMS Control group	No message	175.735
ATM Control group		29.295
Total experimental sample		400.723

Notes: Total number of observations by group.

Table 2: Treatment Effects on Take-Up

VARIABLES	(1) Full Sample (12 Months)	(2) Full Sample (12 Months)	(3) 1/3 Sample (12 Months)	(4) 1/3 Sample (12 Months)	(5) 1/3 Sample (18 Months)	(6) 1/3 Sample (18 Months)
All treatments (Pooled)	5.129*** (0.064)		6.209*** (0.112)		3.746*** (0.084)	
Baseline Message		4.736*** (0.163)		5.181*** (0.279)		3.211*** (0.213)
Information Messages		5.157*** (0.117)		5.998*** (0.201)		3.462*** (0.150)
Salience Messages		5.578*** (0.104)		7.224*** (0.182)		4.407*** (0.139)
Baseline Message (ATM)		4.266*** (0.163)		4.970*** (0.279)		3.093*** (0.208)
Constant	20.461*** (0.005)	20.461*** (0.005)	22.139*** (0.009)	22.139*** (0.009)	22.419*** (0.018)	22.419*** (0.018)
Observations	4,808,676	4,808,676	1,580,412	1,580,412	2,370,618	2,370,618
Number of Individuals	400,723	400,723	131,701	131,701	131,701	131,701
P-value Baseline = Information	.	0.035	.	0.017	.	0.334
P-value Baseline = Salience	.	p<0.001	.	p<0.001	.	p<0.001
P-value Information=Salience	.	0.007	.	p<0.001	.	p<0.001
P-value Baseline SMS = ATM	.	0.041	.	0.591	.	0.692
Controls	FE	FE	FE	FE	FE	FE

Robust standard errors in parentheses

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

Clustered at the individual level

Notes: Robust standard errors in parentheses.

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

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.



¿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



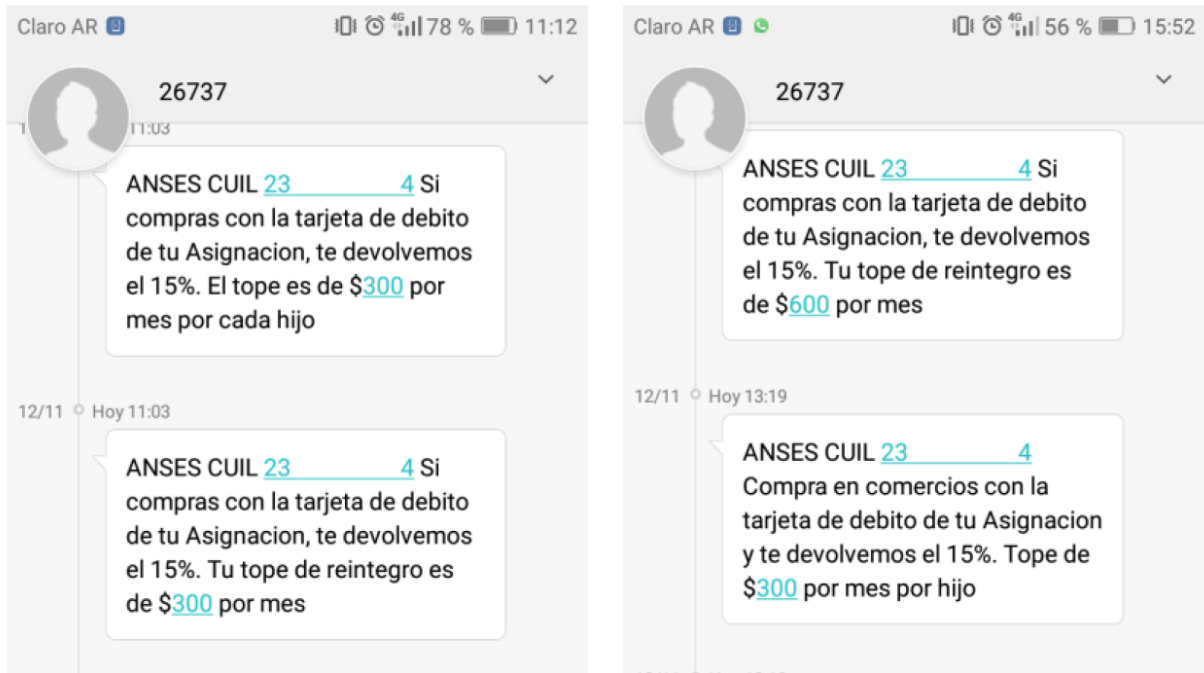
¿Cómo acceder al reintegro?

- 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.

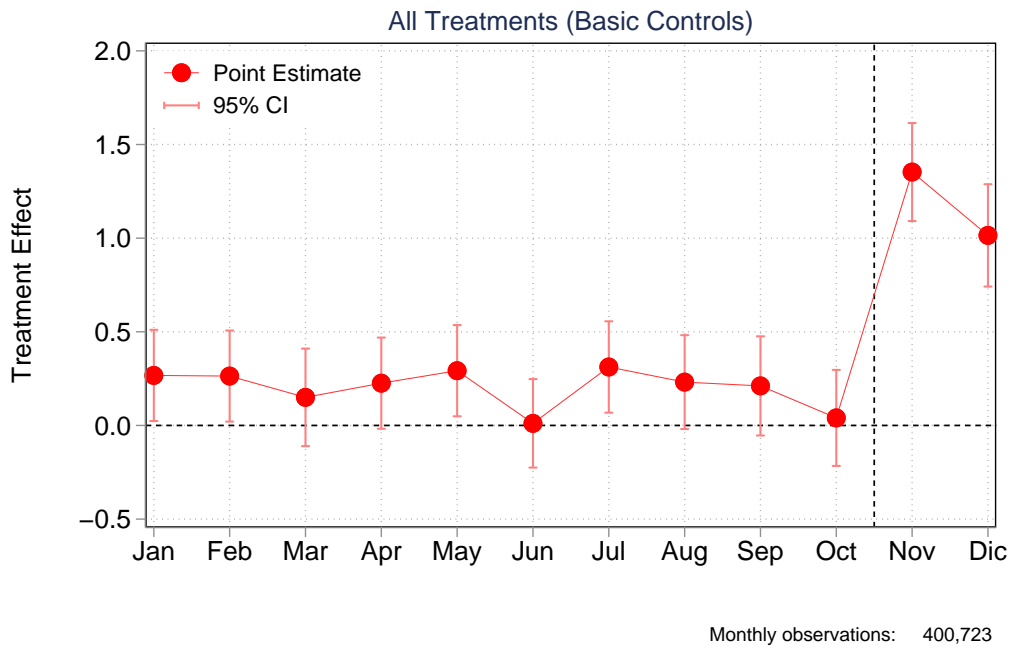
Figure 2: Screen Captures of Selected text-message treatments



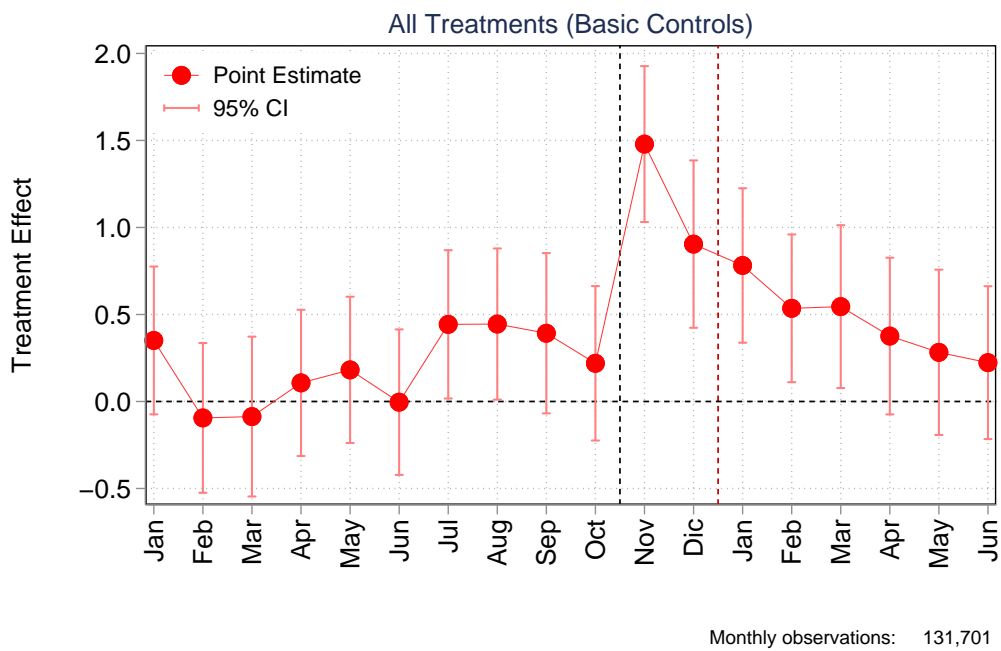
Note: Information treatments as displayed in beneficiaries' phones.

Figure 3: Treatment Effects Over Time: Pooled Treatments, Full and One Third Samples (12/18 Months)

a. Full Sample (12 Months)

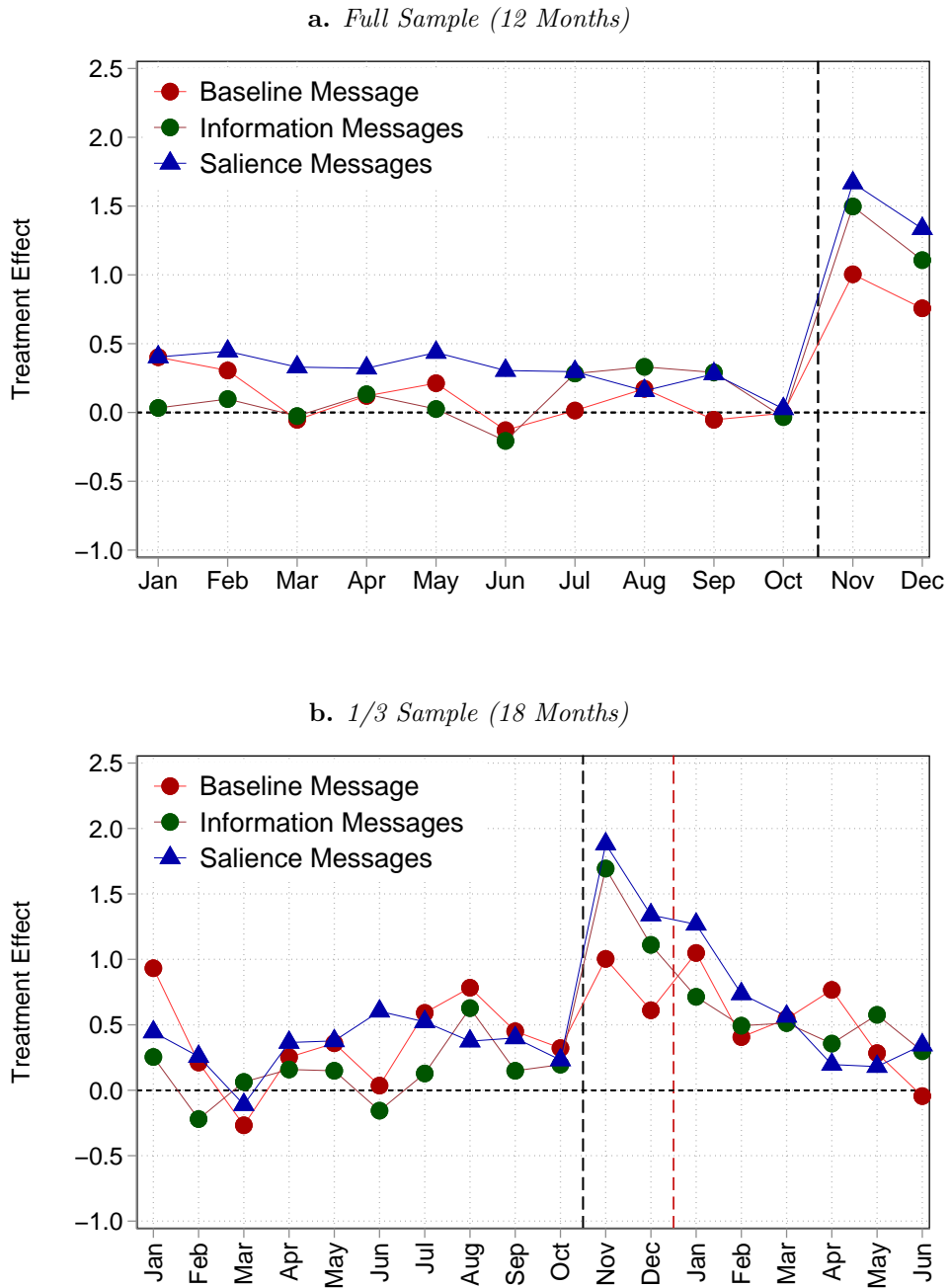


b. 1/3 Sample (18 Months)



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

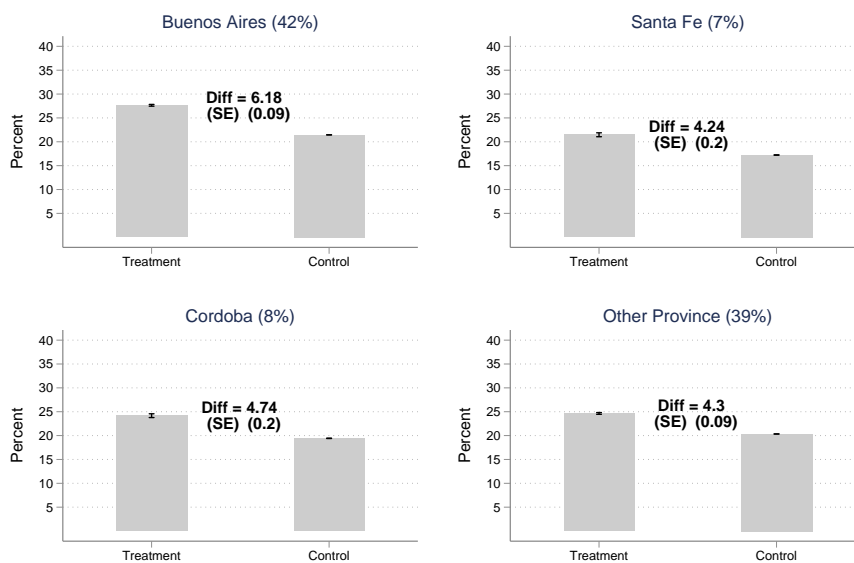
Figure 4: Treatment Effects Over Time: Comparison of Baseline, Information and Salience



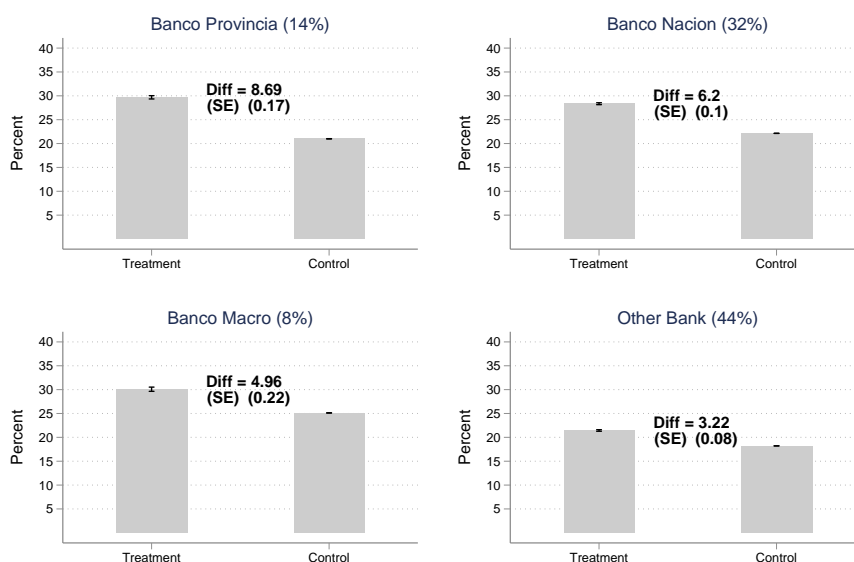
Note: Ninety percent confidence intervals represented by vertical lines, computed with robust standard errors.

Figure 5: Heterogeneous Effects By Geographical Area and by Bank

a. *By province*



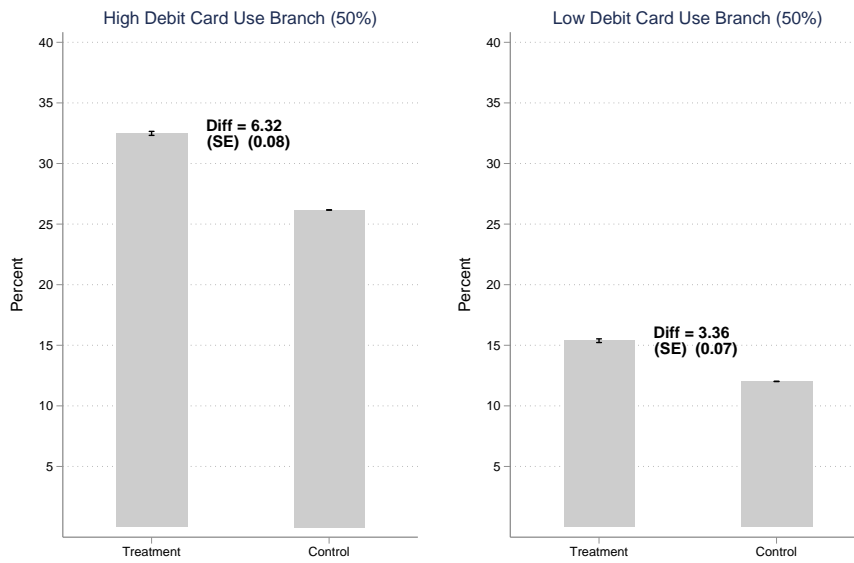
b. *By bank*



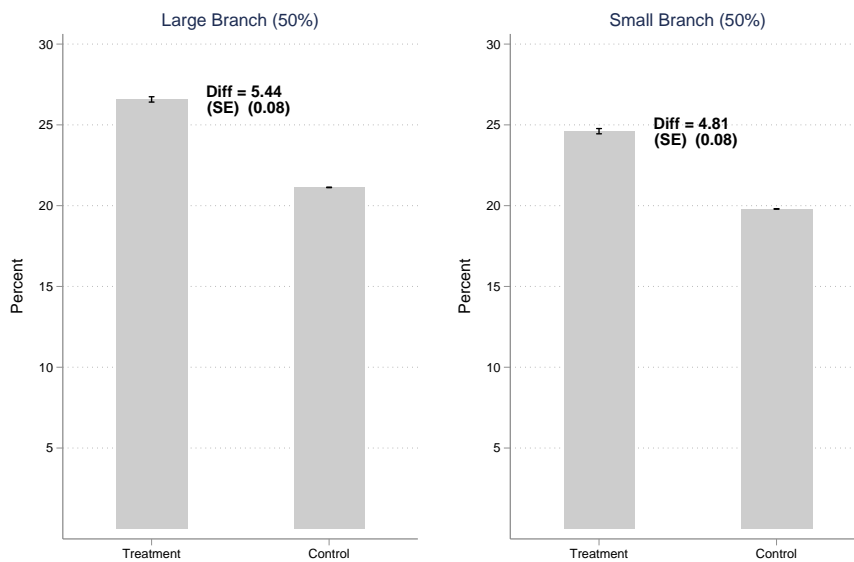
Notes: Ninety percent confidence intervals, represented by red lines, were computed with robust standard errors

Figure 6: Heterogeneous Effects By Geographical Area Bank Branch Characteristics

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

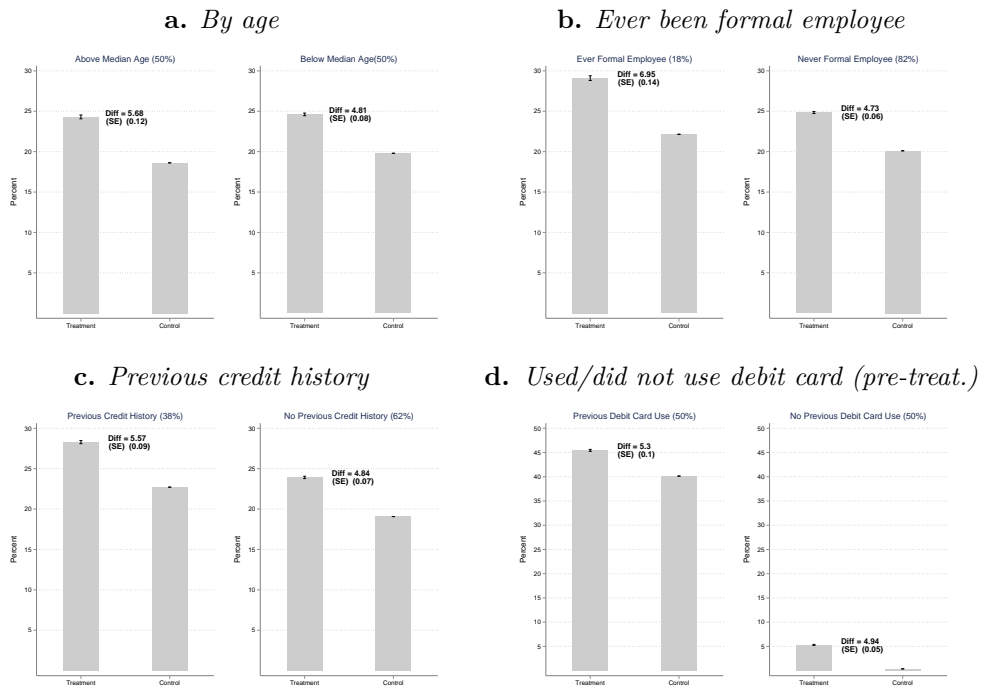


b. *Large/small branch*



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

Figure 7: Heterogeneous Effects By Individual Characteristics



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