Do Cash Assistance Programs Create Welfare Traps?

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1. Introduction

A major concern in the design of safety-net programs is the possibility that long-term participation in cash (and near-cash) assistance programs fosters dependency. An extensive body of empirical literature sheds light on whether entering a cash assistance program discourages work and/or induces other behaviors that could inhibit self-sufficiency. However, much less is known about what happens when beneficiaries are *forced* out of welfare, and both margins are key to answering the question of whether cash assistance programs create welfare traps.[1] This study offers an empirical answer to this question by providing separate causal estimates of the impact of entry into and of forced exit from a cash assistance program.

To isolate these causal impacts, we take advantage of the unique way in which Uruguay decided to re-target its main unconditional cash transfer (UCT) program through a proxy-means test for granting and withdrawing the benefit. Through more than 250,000 household visits (during 2012 – 2018) covering roughly one-fifth of the population, the government estimated a socio-economic vulnerability score for each household and determined its eligibility for the program by comparing the score to a pre-specified threshold (i.e., beneficiary households are those whose scores are higher than the pre-specified threshold). This meant that some households that were enrolled in the program prior to the visit lost their benefit, while some households that were not enrolled in the program prior to the visit started receiving the transfer after the visit. The fact that those slightly above the threshold become eligible for the program but those slightly below the threshold do not allows us to estimate the impact of the program based on both regression discontinuity and dynamic differences-in-differences designs.

We use rich administrative longitudinal data (up to five years of monthly data for almost a million individuals), and survey data matched from five different government institutions, to follow individuals after they enter/exit the program. We focus on three key outcomes to assess family welfare dependency.

First, we focus on labor supply and formalization of work. Theoretically, the concern is that welfare could discourage work either through an income effect or (for "visible" forms of work) a perception on the part of individuals that their status in the program could be jeopardized if their earnings surpass a certain threshold. However, the effect could also go in the opposite direction if individuals need the transfer to reach a minimum level of consumption that allows them to be able-bodied workers (the seminal work by Dasgupta & Ray 1986 models the link between malnutrition and unemployment), or if the transfer helps relieve the credit constraint and allows households to invest in small businesses.

[1] There are several definitions of welfare trap. Cooke (2009) defines it as a "decreasing ability to leave social assistance". Guzi et al. (2014) states that a welfare trap is created when "social benefits are accepted as an alternative to low and insecure earnings". Shaoan et al. (2019) takes a completely different approach and defines it as a "situation where the government sacrifices long-term economic growth and welfare to maintain short-term welfare". In this paper, "welfare trap" is meant to represent a situation where the receipt of welfare over time inhibits beneficiaries' ability to be self-sufficient.

Second, we look at human capital investments for children. Theoretically, a cash assistance program could reduce human capital investments under certain scenarios.[2] Dahl & Gielen (2018) finds that children of parents whose disability insurance benefit was reduced complete more schooling, which the authors interpret as being due to the expectation that they will receive less of a disability insurance benefit when they reach adulthood.[3] If children invest (at least partially) in human capital to increase the likelihood of getting a formal job as adults, any form of "tax" on formal employment could reduce human capital investments during childhood. Thus one possibility is that children entering the UCT program raise their expectations of receiving the transfer during adulthood as long as they do not participate in the formal labor market (or their formal income is low enough), which could discourage educational investments. Of course, it is also possible that cash assistance programs lead to *more* human capital investments in children, and in fact most theoretical work on this topic has emphasized this possibility. Education is commonly thought of as a normal good (Lazear 1975), and a transfer that relaxes the household's budget constraint could increase enrollment of children in schools for multiple reasons (e.g., because children no longer need to work to support the household, because they are healthier and thus more able to stay in school, or because the transfer enables the purchase of necessary school supplies).

Third, we study whether welfare begets welfare. In particular, we study whether being enrolled in the UCT program impacts take-up of other safety-net programs.[4] The literature has identified three types of costs that impact take-up of welfare: the stigma associated with it, transaction costs of applying to the program, and costs associated with learning about the program's eligibility and application rules.[5] If enrollment in UCT lowers any of these costs for other welfare programs, then we could see an increase in take-up of other programs. However, we could see a decrease in take-up if enrolling in the UCT program reduces the marginal benefit of enrolling in other welfare programs. The simple argument would be that UCT induces a positive income effect that makes individuals less willing to bear the costs associated with applying to other welfare programs. Also, decreased take-up could occur if enrolling in UCT mechanically decreases eligibility for other welfare programs.

In addition to these three main outcomes, we look at other dimensions that help us interpret our main results and understand some of their mechanisms. As a proxy for how household expenditures change with the program, we look at how receipt of UCT impacts self-reported food insecurity measures, durable goods consumption, and housing conditions. We also look at strategic behaviors in which people engage to try to enter/stay in the program and that do not alter self-sufficiency. In

[2]See Kesselman (1976) for an early theoretical contribution on how welfare could reduce the incentives for human capital accumulation.

[3]"We find intriguing evidence for anticipatory educational investments, consistent with children planning for a future with less reliance on DI" (Dahl & Gielen 2018, p. 3).

[4]We do not study whether there is intergenerational transmission of welfare (i.e., whether children whose parents are enrolled in UCT are more/less likely to enter UCT during adulthood), as our data do not span sufficiently many years to enable us to follow children during adulthood.

[5]See Moffitt (1983) for the seminal theoretical contribution on welfare stigma.

particular, we look at whether people make a formal request for a re-visit if they are forced to exit the program, whether they are "selectively deaf" when a government official tries to visit them (i.e., whether current participation in the program affects an individual's willingness to answer the door when a government official shows up), and whether there is misreporting of information gathered at the visit. Documenting these behaviors enables us not only to show that households do want to enroll in this program (despite the welfare stigma) but also helps us to understand why there is "stickiness" in program participation, which is a feature that is seen in this program and that has also been documented in several other contexts.

We find that the formal labor supply of adults (especially those under 40 years of age) drops three years after entering the program. However, beneficiaries that are forced to exit the program increase their formal labor supply three years later. School enrollment (for youth that should be enrolled in secondary school) decreases when entering welfare. Nevertheless, because secondary school graduation rates in the Uruguayan context are low for low-income families, the school enrollment impacts do not translate into differential educational attainment rates. Entry into the program has a negative impact on enrollment in public housing programs (although we note that this effect is not entirely robust) and take-up of other types of public cash assistance. These are more suggestive of safety-net program substitution than of increased dependency on multiple programs. Finally, we document several behaviors that induce stickiness in program participation and that are orthogonal to an individual's ability to subsist without the cash assistance.

Overall, these results suggest that the program does not induce a welfare trap. If we focus only on entry, we could be tempted to conclude that the program fosters dependency through a reduction in formal labor supply. However, the fact that people that are forced to exit welfare increase their formal labor supply suggests that even if the program decreases employment, it does not decrease beneficiaries' *ability* to find employment. Although we find negative impacts of the program on school enrollment, this does not create a welfare trap, as these impacts do not translate into differential educational attainment rates. Take-up of other welfare programs can also be ruled out as a potential driver of dependency in this context, as we find safety-net programs to be substitutes. Finally, we note that stickiness in program participation (which could be confused with a welfare trap) seems to be mostly associated with strategic behaviors aimed at staying in the program, rather than with a decreased ability to leave social assistance.

One could argue that, perhaps in other contexts, cash assistance programs do create welfare traps. The interesting aspect of finding that our program does not create such a trap is that, if anything, Uruguay is one of the settings where one would most have expected to find evidence of such traps. First, the program we study seems not to be lifting people out of poverty, and households remain in the program for several years.[6] Households enrolled in UCT during September 2018 (the latest date for which data are available) have been receiving the transfer for 5.2 years on average. This could be

[6]The government re-tested a group of households one year (on average) after they were deemed eligible for the program based on to their poverty status; 87% were still not able to exit poverty and remained eligible.

indicative that the program is inhibiting self-sufficiency.[7] Second, it is a setting where beliefs that welfare discourages work are particularly salient, which is probably the main channel associated with the possibility that cash assistance programs foster dependency and create welfare traps.[8]

This paper adds to a large literature on a central topic in labor economics and public finance: the relationship between welfare and the labor market (see Chan & Moffitt 2018 for a review). Its main contribution is to study this topic with causal estimates of both entry into and forced exit from a cash assistance program. To see why both margins are key to the study of welfare traps, we note how studying only one margin may not provide sufficient evidence to reach a conclusion. First, suppose that our estimates indicate a reduction in labor supply (or labor income) while an individual is enrolled in the program. This would not be sufficient to conclude that there is a welfare trap. It could be the case that individuals are reducing their earned income while enrolled in the program but their *ability* to generate income remains unchanged, and that earnings would go back up if they exited the program. Second, suppose we find no positive impacts on labor supply when individuals (quasi-randomly) exit the program. Again, it is not possible to reach a conclusion, as we could be dealing with a program that has no negative impacts on labor supply at entry.

To the best of our knowledge, this is the first study to provide causal estimates of both entry into and exit from a cash assistance program. Previous research has mostly relied on either causal estimates of entry into a program on labor supply (or formalization of work), or descriptive studies of characteristics associated with endogenous exit from cash assistance programs (e.g., Hansen 2007, Blank 1989).[9]

Among the first group of studies, there is previous work in the US on the labor market impacts of Food Stamps (Currie 2003 and Hoynes & Schanzenbach 2016 review this literature), currently called the Supplemental Nutrition Assistance Program (SNAP).[10] While initial work on this topic (Hangstrom 1996, Fraker & Moffitt 1988) shows practically no evidence of work disincentives, recent work has

[7]It could also be the case that individuals are strategically reporting in a way that induces stickiness in program participation but is not necessarily associated with reduced self-sufficiency.

[8]In the Americas Barometer (2012) individuals in Latin America, the US, and Canada are asked: "Some people say that people who get help from government social assistance programs are lazy. How much do you agree or disagree?". Strongest agreement within the 26 countries that participate in the survey can be found in Argentina, and Uruguay is second.

[9]There are a few papers that report quasi-random variation in exit from welfare (or a reduction in the amount of welfare received), although with quite different programs and contexts than ours (Riddell & Riddell 2014, Dahl & Gielen 2018, Deshpande 2016). There is also work on how recipients of unemployment insurance in the US respond at and before benefit exhaustion (Ganong & Noel 2019, Katz & Meyer 1990).

[10]There is also work on the Temporary Assistance for Needy Families program (TANF), as well as on a permanent and universal cash transfer in Alaska (Jones & Marinescu 2019) and on other major welfare programs in the US (e.g., Baicker et al. 2014 on Medicaid, and Chetty et al. 2013 and Miller et al. 2018 on the Earned Income Tax Credit). In this short review, we highlight SNAP, as this is the program (within the US) most closely related to the UCT program in Uruguay. For a thorough review on means tested transfer programs in the US, see Moffitt (2016).

found some evidence, in particular among single women (East 2018, Williamson & Whitmore 2012).[11] Within developing countries there is a massive literature on conditional cash transfer (CCT) programs (see Bastagli et al. 2019 or Fiszbein & Schady (2009) for a survey of the literature). Banerjee et al. (2017) reanalyzes data from seven randomized controlled trials of government-run CCT programs in six developing countries and find no evidence that these discourage work. However, several studies have found that CCT programs discourage formalization of work (e.g., Gasparini et al. 2009, Garganta & Gasparini 2015, Alzúa et al. 2013, Araujo et al. 2017) and argue that the mechanism seems to be the means test.[12] Of particular interest is Bergolo & Cruces (2016), which studies the labor market impacts of a CCT program in Uruguay and finds that it reduces registered employment by 8 percentage points.[13] Amarante et al. (2011) studies a previous poverty-alleviation program in Uruguay (*Plan de Atención Nacional a la Emergencia Social*), and finds that it reduces formal labor supply, primarily among women.[14] Our program is different from these in that it is unconditional and has no time limits.[15] Our program is also particular in that it is indirectly means tested for a subset of beneficiaries (i.e., for those also enrolled in Uruguay's main CCT program). This allows us to exploit a variation (i.e., heterogeneous impacts according to whether the UCT is means tested) that can more directly inform on whether the means test is driving formal labor supply responses, as argued in most of the work that finds that cash assistance programs discourage formal employment.

Our results on education enrollment and attainment also contribute to a closely related literature on welfare and human capital investments for children. While studies on the effects of welfare programs on human capital investment in the US are scarce (Moffitt 2002), there is abundant work on this topic in the context of CCT programs in developing countries (see Saavedra & Garcia 2012 for a meta-analysis of 42 evaluations of CCT programs and their impacts on educational outcomes in developing countries). Once again, our program is a UCT. McIntosh et al. (2010) evaluates a cash transfer program in Malawi that has a CCT and a UCT arm and finds that the conditionality effectively plays a role (i.e., the CCT arm outperformed the UCT arm in terms of school enrollment). Thus it is important to understand the role that a program with no conditionalities could play in educational attainment, in particular given that the literature on this topic is quite limited.[16]

[11] There are also studies in other developed countries (e.g. Mogstad 2012, Autor et al. 2019).

[12] Table 1 in Banerjee et al. (2017) summarizes the findings in the literature on labor supply impacts of CCT programs. Despite the work in this area, evidence of disincentives to formal employment in Latin America is still far from conclusive (Bosch & Manacorda 2012).

[13] The result is concentrated among adults under 30 years of age, and the authors find evidence that the fall in registered employment is mostly due to a shift to informality and is not a labor supply response.

[14] It has also been studied whether this program impacts the incidence of low birth-weight (Amarante et al. 2016), political support (Manacorda et al. 2011), and teenage school attendance (Amarante et al. 2013).

[15] The literature on impacts of UCTs on labor market outcomes in developing countries is considerably smaller than that of CCTs. Bastagli et al. (2019) reviews the literature on the impacts of non-contributory cash transfers in low- and middle-income countries from 2010 to 2015. Among the 165 studies that were identified, 55% addressed CCTs, and only 25% addressed UCTs. Moreover, most of the UCT studies were in Sub-Saharan Africa, and none were in Latin America.

[16] Baird et al. (2014) present a systematic review of the literature of the effects of cash transfer programs on schooling outcomes and conclude that ".. simply there are too few rigorous evaluations of UCTs" (Baird et al. 2014, p.30).

This paper also contributes to a growing literature on welfare take-up.[17] Keane & Moffitt (1998) studies the labor supply effects of multiple program participation and shows that it is extremely common for households enrolled in a welfare program to participate in more than one program. In 1984, 89% of beneficiaries of Aid to Families with Dependent Children (AFDC) also received Food Stamps and Medicaid, and 42% received a fourth benefit (Keane & Moffitt 1998). This raises the question of whether multiple-program participation is a consequence of certain overlap on eligibility criteria for welfare programs, or whether it is a consequence of some causality going from enrolling in one welfare program to enrolling in one or more additional welfare programs. Baicker et al. (2014) studies whether Medicaid increases participation in Food Stamps, TANF, Supplemental Security Income, and Social Security Disability Insurance and finds a positive and statistically significant impact on enrollment in Food Stamps.[18] The UCT program in Uruguay seems to impact enrollment in the opposite direction (i.e., with a negative impact on enrollment in housing assistance and other cash assistance programs). It would be an interesting avenue of research to understand what drives such differences.[19]

Results on government complaints, selective deafness, and misreporting also contribute to the field of forensic economics (see Zitzewitz 2012 for a review). Our results suggest that individuals take actions to stay in the program, which could (perhaps) be considered unethical (e.g., misreporting income, or not answering the door when a government agent shows up, if the individual believes that he will be deemed no longer eligible for the UCT). It is particularly interesting to document these behaviors in a country such as Uruguay where there are high moral standards in regard to participation in welfare, and thus one would not necessarily have expected these behaviors to take place. According to data from the World Value Survey (2010-2014), Uruguay is actually the country in Latin America with the strongest condemnation of claiming government benefits to which one is not entitled.[20]

The remainder of this paper is organized as follows. Section 2 describes the institutional context. Section 3 presents the data and some descriptive summary statistics of our sample. Section 4 describes the empirical strategy. Section 5 presents the main results of the paper. Section 6 presents additional evidence to aid in the interpretation of our results and potential mechanisms that drive them. Section 7 shows robustness checks of our results. Section 8 concludes.

[17] Part of this literature has dealt with understanding the reasons behind incomplete take-up (Finkelstein & Notowidigdo 2019, Kleven & Kopczuk 2011, Bhargava & Manoli 2015) or the intergenerational transmission of welfare (Boschman et al. 2019, Dahl, Kostol & Mogstad 2014, Dahl & Gielen 2018, Antel 1992). There is also work on peer effects in paid paternity leave take-up in Norway (Dahl, Løken & Mogstad 2014).

[18] Mallar (1982) studies the impact of enrollment in the Job Corps on future receipt of AFDC, General Assistance, Food Stamps, public housing, and unemployment insurance. Overall, he finds significant reductions in receipt of AFDC, General Assistance, Food Stamps, and unemployment insurance.

[19] Among the many differences between these programs, Baicker et al. (2014) states that Medicaid case workers are instructed (in certain instances) to offer assistance in applying for TANF and SNAP. In Uruguay, this type of assistance is not automatically provided.

[20] The exact question is: "Please tell me for each of the following actions whether you think it can always be justified, never be justified, or something in between, using this card: Claiming government benefits to which you are not entitled". Possible answers are on a scale from 1 (Always justifiable) to 10 (Never justifiable).

2. Institutional context

This section provides background information relevant to understanding this study. It summarizes the information available from governmental reports and from our own conversations with multiple government officials that clarified several aspects of the design and implementation of the UCT program.

First, we provide an overview of the main components of the Uruguayan welfare state. The objective is to posit the program that we study in the proper context and to understand some of the outcomes that we address later. Second, we describe in detail the UCT program *Tarjeta Uruguay Social*. Third, we explain how the Ministry of Social Development (MIDES) conducts its field work and how the information it gathers is processed to assess eligibility for the UCT program. This is key to understanding our identification strategy.

2.1. The Uruguayan welfare state

Uruguay, a small country with 3.3 million inhabitants, is among the most developed countries in South America. With a GDP per capita of USD 17,000 (2018), it is second only to Chile (comparing at PPP), and the United Nations Development Programme situates it in its highest category of human development ("Very high human development").[21] The main pillars of the Uruguayan welfare state are a far-reaching public education system, a generous and solidary social security system (Filgueira 2005), and a health system with broad coverage.

The Uruguayan government provides free public education at all levels (from elementary school to university) and has established 14 years of mandatory schooling (2 in elementary school, 6 in primary school, and 6 in secondary school). Despite practically universal completion of primary school, secondary school completion rates still pose a serious challenge. A report by the National Institute for Educational Assessment and Evaluation (INEEd 2014) shows that only 28% of youths in the 18-20 age group had finished secondary school in 2013.[22] Among those enrolled in one of the last three grades at the start of the school year, only 57% passed the grade. Moreover, educational attainment is unevenly distributed. Whereas 64.6% of youths in the 18-20 age group that are in the highest income quintile completed secondary school, only 7% of those in the lowest quintile did.

Another pillar of the system is the safety-net available to formal workers. These workers have to contribute 19.5% of their salary to social security and to a national health plan, and they are eligible for unemployment insurance (which has a maximum duration of six months), an old-age pension (retirement starts at 60 years of age if they contributed to social security for 30 years), paid maternity and paternity leave (98 and 13 days, respectively), access to health care with a public or private

[21] According to The Economist's 2018 democracy index, it is also the only "full democracy" in South America.

[22] This number is 39% if we consider the 21-23 age group.

provider, and a family monthly allowance if their formal income is below a certain level. Nevertheless, there is still a non-negligible share of the population that operates in the informal economy, without this safety net and with significantly lower wages (Amarante & Gómez 2016 finds that in 2014 informal workers earned 30% less than formal workers employed in the same activity). Informality has steadily decreased since 2004, when the share of informal workers reached 40.7%. By 2014 that figure dropped to 23.5% and has remained relatively constant since then (Amarante & Gómez 2016).

Economic crisis and cash assistance programs. In 2002 a severe economic crisis hit Uruguay which generated a new wave of cash assistance programs to alleviate material hardship. Poverty rates reached 23.6% and unemployment 17% (the highest level in 20 years), while real per capita income fell 8% between 2001 and 2002 (Manacorda et al. 2011). Capitalizing on the dissatisfaction with the economy and the management of the crisis, in March 2005 a center-left-wing party (*Frente Amplio*) took office. Among the flagship policies of the new government was the creation of an emergency plan (*Plan de Atención Nacional a la Emergencia Social*) that included a cash assistance program targeting the poorest 10% of the population.[23] A newly created Ministry of Social Development (MIDES) was put in charge of its implementation.

By 2008, the crisis was long gone and the government decided to restructure its poverty alleviation programs. The emergency plan was replaced in January 2008 by two new non-contributory cash assistance programs: a CCT program (*Asignaciones Familiares - PE*) and a UCT program (*Tarjeta Uruguay Social*).[24] In practice, these two plans served as a continuation of the emergency plan. All families with minors or pregnant women that were previously enrolled in the emergency plan started receiving CCT and UCT.[25]

The CCT program targets poor families with children under age 18 and pregnant women, and the amount varies with the number of minors in the household and whether they are enrolled in secondary school.[26] The program is managed by the Social Security Administration (SSA), and its selection criterion combines both proxy-means testing and means testing. First, households must complete an application which captures an array of socio-economic data. With these data, the SSA

[23]The monthly transfer, which amounted to USD 55, was conditional on school attendance for all children under age 14, and on regular health checkups for pregnant women and all children. However, in practice these conditionalities were never enforced. Families with children and/or pregnant women received an additional transfer ranging from USD 14 to USD 37, depending on the number of children. Manacorda et al. (2011) describes the emergency plan in detail.

[24]Whenever we refer to the "CCT program", we mean *Asignaciones Familiares - PE* unless stated otherwise. Similarly, whenever we refer to the "UCT program", we mean *Tarjeta Uruguay Social*.

[25]The transition meant changes in the amount transferred to families. Since the amount of the CCT is based on the number of children (unlike the cash transfer associated with the emergency plan), families with more than two children increased their transfer. Families with two children received approximately their former amount, and families with only one child lowered their monthly transfer by about USD 10.

[26]There is no cap on the number of beneficiaries per household. The family receives USD 46 for the first child. If the child is enrolled in secondary school, the amount increases by USD 20. For each additional child, those amounts increase at a decreasing rate. A family with N children and n of whom are enrolled in secondary school receives $46 \times N^{0.6} + 20 \times n^{0.6}$ (DINEM 2012).

computes a predicted-income score (*Índice de Carencias Críticas*, which we refer to hereinafter as the Vulnerability Index); households for whom this score is above a certain threshold can become eligible for CCT. The second condition they need to meet is to earn less than a certain monthly formal per capita income. The Social Security Administration systematically checks this income for CCT beneficiaries; if a beneficiary household surpasses this threshold for three consecutive months, it loses its eligibility for CCT.[27]

2.2. A primer on Uruguay's largest unconditional cash transfer program

Tarjeta Uruguay Social is the main Uruguayan UCT program, and its objective is to "provide food support to people in extreme poverty conditions" (DINEM 2011). Its target population is defined as "those 60,000 households (with or without minors) in a situation of extreme socio-economic vulnerability" (DINEM 2012, p.159), which roughly corresponds to the poorest 5% of households in the country.[28]

Beneficiary households receive only one magnetic card (which we refer to as the food card), and MIDES, which is the government institution that manages the program, adds money to the food card on a monthly basis.[29] The amount depends non-linearly on the number of children comprising each household (up to a maximum of four). In October 2019, a household with two children received the equivalent of 47 current USD.[30] Households with no children receive the amount equivalent to one child. Additionally, a household receives double the amount if MIDES concludes that it is within the poorest 30,000 households in the country.[31]

There are two additional benefits that the UCT provides. First, products paid for with the food card are not taxed with VAT.[32] Second, for each child under 4 years of age, MIDES adds an extra amount of 10 USD to the food card (monthly).[33] All transfers are indexed annually by the inflation rate of food (DINEM 2012).

[27] According to Bergolo & Galvan (2016), by 2014 the maximum monthly formal per capita income was USD 196 for families with two members, and USD 242 for families of more than two.

[28] When we explain the rules of eligibility, we describe in detail how the concept of "socio-economic vulnerability" is operationalized, and thus give a more precise definition of the target population.

[29] A household receives these funds on the same day each month, but different households could get the transfer on different days. Depending on the last digit of the national identity number of the card holder, MIDES adds money on day 10, 12, 14, 16, or 18 of each month. We have only monthly level data, so we do not exploit differences in timing of transfer receipt in this study (we also do not have access to the national identity number of the card holder).

[30] In 2019, a family with one child received USD 31; the amounts for a family with three children and a family with four or more children are USD 60 and USD 84, respectively.

[31] For brevity, we refer to these households as enrolled in the double-UCT program or beneficiaries of double UCT.

[32] The VAT rate is 22% for most products. Some products, such as flour, rice, bread (not all kinds), cooking oil, tea, coffee, and soap, are taxed at 10%.

[33] Initially, MIDES provided iron-fortified milk to UCT beneficiaries instead of this extra amount. However, logistical costs and misconduct of sellers paved the way to switching from the milk to an equivalent amount (money-wise) to be added to the food card, starting in May 2016.

Unlike other Uruguayan cash transfers, the food card is accepted only at "solidary shops". These are grocery stores and supermarkets that voluntarily signed an agreement with MIDES to accept the food card as a means of payment. These shops are forbidden to sell alcohol, tobacco, or carbonated drinks if the costumer is paying with the food card. According to the data provided by the concessionaire of the payment terminals, by 2011 74% of all money transferred was spent on food purchases and 20% was spent on cleaning supplies (CICCA 2012).

The number of solidary shops varied between 700 and 1200 until 2015 (Aguirre et al. 2015, MIDES 2016). The network was expanded in 2016. Since then, the number of shops has steadily increased, reaching 2,695 by September 2019. Though solidary shops are concentrated in areas with high population density (in particular, in Montevideo), there seems to be a reasonable coverage of the whole country. As a matter of fact, virtually 100% of the sum of all UCT transfers was spent in 2011, even before the expansion of the network (CICCA 2012).

Origins and re-targeting policy. UCT was launched in January 2008 as a supplementary payment for those 65,000 households with children who were transitioning from the emergency plan. UCT was conceived of as an extra transfer that aimed to warrant food security for extremely poor families. In addition to the families from the emergency plan, 20,000 households were added in May 2009. The latter were former beneficiaries of a food basket program managed by the National Institute of Nutrition (CICCA 2012).

Both the merging of populations from other programs and the vagueness in the definition of the original target population resulted in a controversial internal report on mistargeting. By August 2011, MIDES authorities recognized that the law that created the UCT was not sufficiently precise in defining extreme poverty or social vulnerability. In a public report the MIDES division in charge of internal monitoring and assessment (DINEM) stated the following:

> *"it is desirable to revise and update the definition of the target population (. . .) it would be of great help to make explicit the UCT objectives and reach (. . .) It is of the greatest importance for the UCT program to implement a systematized and protocolized procedure in order to grant and withdraw benefits. Moreover, it is important to update information on the beneficiaries."* (DINEM 2011, p. 75)

Absent a precise definition of the target population, DINEM tried to diagnose mistargeting under five different definitions of the target population. In the first scenario, DINEM considered the target population to be all households below the poverty line. Under that definition, they estimated that 63% of poor households were not receiving UCT and that 29% of households receiving it were not poor (i.e., type I and type II errors). A second scenario was to assume that the target population was comprised of households that surpassed a critical threshold according to the proxy-means algorithm used by the CCT program. In that case, 42% of those above the threshold were not receiving UCT, and 64% of beneficiaries did not surpass the threshold (DINEM 2011, pp. 71–73). Under any scenario, mistargeting was a clear concern.

By the end of 2011, a re-targeting policy was set in motion. The target population was then precisely defined, and intensive fieldwork was carried out to refocus the policy. This setting is what provides the quasi-random variation in entry into and exit from the program that we exploit in this study. 65,000 household visits were conducted during 2012. By 2013, all the information collected in those visits was used to re-target benefits. In that year, 32% of those previously receiving UCT were cut from the program (25,167 households). In the same period, 17,874 new households were accepted for receipt of UCT, and this re-targeting policy continued in the following years (DINEM 2012).

2.3. Eligibility process and fieldwork

In this subsection, we first describe the proxy-means testing instrument used by MIDES to assess eligibility for the program. Second, we detail how MIDES gathers the necessary information to conduct this test. Third, we comment on the exceptions to this assignment rule.

The Vulnerability Index: a proxy-means test. In 2012 UCT's target population was defined as the 60,000 households in extreme socio-economic vulnerability conditions as measured by the Vulnerability Index (VI). The VI was designed by scholars from an economics research institute of the University of the Republic, initially with the objective of devising a mechanism to select beneficiaries of the CCT program. Given the level of informality among the target population and the possibility of income misreporting, scholars devised an index to predict the probability that a household belongs to the lowest income quintile based on a large set of socio-economic characteristics that were gathered at household visits conducted by MIDES officials (Bergolo & Galvan 2016). Similar targeting mechanisms are applied in other contexts, as proxy-means testing has become the "industry standard" for these types of programs when the poverty status is not directly observable (De Wachter & Galiani 2006).

Specifically, the VI is a highly saturated probit model in which the dependent variable takes value 1 if the household belongs to the lowest quintile of per capita income.[34] The model was estimated initially in 2009, and re-estimated in 2012 using survey data from the 2011 National Household Survey (NHS), which is a representative sample of the population. Only households below the median per capita income and with minors were included in the estimation sample. Additionally, two different model specifications were defined, based on a territorial criterion: one for the capital city of Montevideo, and another for the rest of the country.

[34] Although the VI model is confidential, Manacorda et al. (2011) lists some of the independent variables used in a previous version of the VI, which are suggestive of the independent variables that are currently used. These are "an indicator for public employees in the household, an indicator for pensioners in the household, average years of education of individuals over age 18, the number of household members, the presence of children by age group (0–5 and 12–17), an indicator variable for whether a member of the household had private health insurance, residential overcrowding, whether the household was renting, toilet facilities (no toilet, flush toilet, pit latrine, other), and a wealth index based on durables ownership (e.g., refrigerator, TV, car, etc.)" (p. 6).

Even though the VI was designed specifically for CCT (it was used since 2009 for this purpose), in 2012 MIDES decided to use it to define eligibility for UCT. Given that UCT targets the 60,000 most vulnerable households, a threshold had to be defined. VI scores were computed for each household in the 2011 NHS sample, and a value was selected such that only 60,000 households had a score higher than the threshold. The same procedure was used to select the threshold value for double UCT, by predicting the value that corresponds to only the 30,000 households with the most critical values (those closest to 1).[35] By 2012, in order to receive UCT a household needed an estimated VI score above 0.62 for Montevideo and above 0.70 for the rest of the country. If a household's VI score was above 0.756 in Montevideo or 0.81 in the rest of the country, then it was eligible for double UCT. Importantly, households never learn their score and are not told which variables enter the algorithm or any specifics on the eligibility thresholds (Bergolo & Galvan 2016).

Household visits: feeding the algorithm. The main way a household can enter/exit the program is by receiving a household visit by a MIDES agent. The agent conducts a survey on site to measure a wide range of variables, including those needed to calculate a household's VI score. By the time that the MIDES report on mistargeting went public, the MIDES administration had decided to conduct extensive fieldwork in order to correct the inclusion and exclusion errors detected (DINEM 2012). This meant both withdrawing UCT from ineligible former beneficiaries, and granting UCT to those eligible. Figure 1 shows the steps followed by MIDES that eventually lead to a grant or a withdrawal of an UCT. We explain these steps in what follows.

The first step in order to grant (or withdraw) UCT is the decision of which households to visit. These are chosen with the objective of both reducing leakage (i.e., withdrawing benefits from ineligible beneficiaries) and increasing coverage of the eligible population. We distinguish between two kinds of visits: area visits and targeted visits.

Area visits are conducted to all households in a specific geographical area. All dwellings in the area are intended to be visited by a group of 4 to 7 enumerators and a field supervisor. Particularly at the start of the re-targeting policy, one of the inputs used to decide these areas was the national census conducted in 2011 by the National Institute of Statistics. Satellite images were also used to define these visits.

A second type of visit, which we call targeted visits, is used to survey current beneficiaries (whose address is already known), with the objective of checking whether they are still eligible. This mode also comprises family requests to become a new recipient of UCT. The visits are performed by a single enumerator who is provided with a map and a list of 10 to 20 households to visit.

Single enumerators usually work in a 5- to 10-block area surrounded by other enumerators and a supervisor in charge of the entire neighborhood. Supervisors randomly perform visits along with

[35] One could argue that the VI may be less predictive of household per capita income in the case of households with no children (as the probit was estimated on a sample of households with children). However, to the best of our knowledge, there are no studies of the type I/II errors of the VI as a targeting mechanism for the UCT.

enumerators in order to check the quality of their visits. Specifically, it is required that enumerators go inside houses in order to diminish the probability of misreporting.

Since 2012, most MIDES visits are performed by the Unit of Programs Monitoring (PMU). The PMU is in charge of conducting visits across the country by enumerators from the capital city. Though it is more expensive to send employees from Montevideo to the rest of the country, the authorities decided to do this in an effort to diminish the probability of collusion between local agents and potential beneficiaries. Moreover, a household is not previously notified that a visit is going to be conducted.

The fieldwork can produce different visit outcomes. In order for a visit to be considered successful, enumerators must find an adult capable of answering all questions. In case of a completed visit, the data collected go directly through the selection process described earlier. Besides, if a household is successfully located but could not be visited, either because there were no adults present or because no one at the house answered the door, the "unsuccessful" visit is administratively tagged in order to be considered in a future targeted visit.

Once the visit is performed, it must go through quality control. The first step is a rapid completeness check in the field by a supervisor. Once they are back in the office, reviewers perform different completeness and coherence tests and call the household in case they need to clarify any part of the form. When the quality control process ends, all the data go through a VI algorithm that calculates the household's VI score. If the VI score is above the UCT threshold (and the household was a non-beneficiary of UCT prior to the visit), then the office in charge of UCT creates the respective UCT account and prints a new magnetic card. Once the food card is printed, a MIDES employee calls the head of the household to ask him or her to go to a MIDES office in order to sign the contract and accept the food card. Money is added to the food card once it is accepted. If the VI score is below the threshold (and the household was a beneficiary of UCT prior to the visit), MIDES stops adding money to the food card; there are minor exceptions to this rule. MIDES officials call the household to notify them that they will no longer be beneficiaries. Notification may be done by letter as well. In no case are regional MIDES deputies in charge of notifying beneficiaries of withdrawals. However, both MIDES regional deputies and officials at headquarters usually receive re-visit requests to have the lost benefit restored.

Exceptions to the rule. If a household has a VI score that does not surpass the eligibility threshold, it is still possible to enroll in the program. In February 2012, a special UCT share was created for transsexual people. By July 2013, a lower threshold for acceptance in UCT was instituted for households in which at least one member was enrolled in a priority social program.[36] Moreover, UCT enrollment was not to be removed for a year after enrollment in that priority social program ended. Another way to receive UCT is through a "special share" managed by regional MIDES deputies to deal with critical situations. These deputies have discretion to allocate a small number of UCTs. Transfers

[36]These programs are *Jóvenes en Red, Uruguay Crece Contigo,* and *Cercanías*. They provide intense monitoring and counseling via social workers to extremely poor teenagers, children, and families, respectively.

allocated through this mechanism comprise 0.5% of all UCTs and can last only 6 months or until a visit to the beneficiary is conducted. Additionally, in April 2015 MIDES created an index (similar to the VI, called the Complementary Vulnerability Index) that assigns more weight to variables that measure housing vulnerability. Starting in April 2015, if a household's VI score does not surpass the UCT eligibility threshold, MIDES computes this second index and if it is above a given threshold, then the household is eligible to receive UCT. Moreover, beneficiaries of housing programs were given a grace period of two years in which their benefit is not withdrawn despite losing eligibility by the VI criterion. In 2016, homeless people also became eligible to receive the transfer.

By June 2018, 92% of all UCT beneficiaries had been visited by MIDES agents and had a VI score above the threshold. Of those whose VI is below the threshold, 22% receive UCT because of extreme housing vulnerability (i.e., surpass the threshold for the Complementary Vulnerability Index), 20% are transsexuals, 26% are enrolled in a priority social program, and the others receive UCT via the special shares allocated by the regional office (provisional benefits) and UCT for the homeless (DINEM 2018).

3. Data and summary statistics

Our empirical evidence is based on administrative datasets from 5 different governmental sources that offer a comprehensive view of how beneficiaries are affected by entry into and exit from the UCT program.[37] All our datasets contain information at the individual level which is indexed with a de-identified national identity number that allows us to link the datasets.[38]

3.1. Data from the Ministry of Social Development

Our primary dataset comes from MIDES and consists of the responses to the comprehensive questionnaire applied by MIDES agents during the household visits conducted during the period January 2011 to July 2018. 255,767 households (1,012,170 individuals) were visited during this period, with 35% of these visits taking place in the capital city (Montevideo). Whenever we refer to "our sample of individuals", we mean those that inhabited a visited household (at the actual time of the visit).

Two key variables that we use in all our analysis and that come from this dataset are the date of the visit and the VI score of the household. As will be explained in Section 4, having a VI score above

[37] Some of these datasets were provided directly by the respective government organization, while others were accessed through the recently developed integrated system of social analysis (*Sistema de Información Integrada del Área Social - SIIAS*).

[38] For all citizens and foreign residents of Uruguay, it is mandatory to have a national identity number. Data from the Americas Barometer (2016) indicate that 99.6% have a national identity number in Uruguay.

a certain threshold is the key instrument that allows us to get quasi-random variation in entry into and exit from the program. Figure 2 shows the distribution of household visits according to their VI score. We also indicate the thresholds that are used to assess eligibility for CCT, UCT, and double UCT.

We also exploit other information gathered at the household visits. In particular, we look at responses on food insecurity questions, durable goods consumption, housing conditions, labor supply, and schooling. Table 1 shows some characteristics of the population under study (i.e., individuals living in a visited household).

In addition to the responses gathered at household visits, MIDES provided us with three pieces of information. The first of these was a monthly dataset of UCT recipients that includes their national ID number (de-identified) and the amount transferred each month, from August 2009 to September 2018.[39] These data were made available to us for only the individuals in our sample.[40]

Second, MIDES shared with us a dataset containing the administrative records of re-visit requests with their respective national ID number (de-identified) and the date of the request.

Third, we were provided with information regarding how each household visit form was internally processed at the Ministry. Specifically, we know both the date of the visit and the date that quality control reviewers finished analyzing the visit information and logged it into the system. This allows us to construct a measure of internal processing time for each visit (i.e. time elapsed between the date of the visit and the date on which the information was logged in the system) that will be used in our identification strategy.[41]

3.2. Formal labor supply data

We have monthly level data (for January 2010 to September 2018) on whether an individual is an active worker in the formal sector, as shown by his contributions to the National Health Insurance Plan (FONASA).[42] Since 2008, every formal worker, regardless of his economic activity, must contribute a fixed percentage of his income to FONASA. That contribution entitles the worker and his nuclear family to choose a public or private health care provider.[43] There are two exceptions to this rule. First, the military and the police are exempt from this contribution and are not enrolled in FONASA. However, data from the National Household Survey (2013) show that only about 0.5% of workers that

[39]Hereinafter, we use the term UCT recipients for individuals that actually receive the transfer (i.e., the card holders) and the term UCT beneficiaries for all individuals that live in a household with a UCT recipient.

[40]However, this corresponds to practically the entire UCT population. According to an internal report of the Cash Transfers Division of MIDES, by June 2018 92% of all beneficiaries had been visited at least once (and thus were included in our sample).

[41]The processing time was high (1.5 years) at the start of the re-targeting policy, but in mid/late 2013 it was sharply reduced and it is currently around one month.

[42]McIntosh et al. (2017) also uses this variable to measure formal work in Uruguay.

[43]Every formal worker must contribute at least 4.5% of his income to FONASA. Additionally, a worker's partner and children are entitled to enroll in FONASA; in that case the worker's contribution increases to 8%.

belong to a household in the lowest income ventile (i.e., the UCT target population) are in the military or police. Thus we do not believe that any changes we find in our measure of formal employment are due to a switch to or from police/military work. Second, formal employees that earn less than 150 USD per month or work fewer than 13 days a month are not legally bound to contribute to FONASA. Thus if an individual is listed as an active worker in FONASA in one month but not in the next, it could be because he is no longer working in the formal sector at all or because he reduced his participation at the intensive margin (i.e., the number of days worked and/or the formal income earned).[44]

3.3. Education data

We gained access to administrative data on public education enrollment, generated by the National Administration of Public Education (ANEP). We accessed individual data on enrollment and grade level for the three different systems that comprise ANEP: elementary and primary; secondary; and vocational. The available data on education are for April 2013 to December 2017.

Although ANEP data do not include information on enrollment in private institutions, it is unlikely that children in our sample attend these institutions. According to data from the National Household Survey, during our period of study 99% of those in the lowest income quintile that attend school do so in the public system.

3.4. Data on enrollment in other safety-net programs

There are several programs offered by the state that are designed to act as safety-nets and to support families that are socio-economically vulnerable. We were able to match individual data on enrollment in several social programs that we describe below.

First, we accessed individual administrative records on CCT receipt. Data on CCT transfers are available for January 2012 to September 2018. We can distinguish both the children entitled to the benefit, the adult that receives the transfer, and the total amount transferred. This variable is relevant not as an outcome, but rather as a way of partitioning the sample to test the role of the means test. As described in Section 2.1, the CCT is means tested while the UCT is not. However, the UCT is indirectly means tested if households also receive the CCT. Thus looking at heterogeneous impacts of the UCT according to CCT status can serve as a test for the role of the means test.

[44]We gathered data on a second indicator of formal employment for validation: monthly social security contributions to the Social Security Administration. These contributions are restricted to certain activities, while FONASA is not, but within those activities, all workers have to contribute to social security. We have data on this variable for only a subset of our sample (those enrolled in CCT, and only for their months of enrollment). The correlation between our two measures is 0.9.

Second, we matched our data with individual administrative records on beneficiaries of any housing assistance program managed by the Ministry of Housing (July 2012 to September 2018).[45] We note that the dummy is "cumulative" in the sense that if an individual is a beneficiary of a housing program at some point in time, it will still show up as a beneficiary in the future.

Third, we matched our data with records on receipt of a cash transfer for formal employees with dependents.[46] This benefit is not a universal right, and only people who engage in certain economic activities are entitled to it.[47] The data were recorded monthly and are available for January 2012 to August 2018.

4. Econometric framework

This study uses a regression discontinuity approach to study how entry into and exit from a cash assistance program impact formal labor supply, schooling, and take-up of other safety-net programs. The main empirical strategy consists of a fuzzy regression discontinuity design (fuzzy RD), and it exploits the fact that the probability of assignment to treatment (i.e., being a beneficiary of UCT) changes discontinuously at a given threshold of the VI. We also estimate a dynamic differences-in-differences model that complements the fuzzy RD estimates. We first specify the regression form of our fuzzy RD and examine the plausibility of its identifying assumptions. Then we do the same for our dynamic differences-in-differences design.

4.1. Fuzzy regression discontinuity design

In its most basic setup, a fuzzy RD can be conceived of as an application of instrumental variable regression in which the treatment is instrumented with whether a running variable surpasses a given threshold. In our case, the running variable is the VI score of a household (measured on the basis of the visit), and the treatment is enrollment in the UCT program after the visit. We slightly augment this specification to allow for the fact that surpassing the threshold may have different impacts on beneficiary status depending on the MIDES processing time. As explained in Section 2.3, MIDES does not instantly adjust the beneficiary status after a visit. The information gathered at a household

[45]Programs included are: Plan Juntos, MEVIR, Housing National Agency, National Housing Office, and Neighborhood Improvement Program (PMB).

[46]This transfer is regulated by law No. 15,084.

[47]More in detail, formal workers with children receive a cash assistance if household income is below USD 1,400. "Family Allowances AFAM 15,084" is a contributory and conditional cash transfer granted to parents or tutors of minors under 18-year-old (and disabled with no age limit) and is not compatible with CCT. It must be noticed that this benefit covers workers of only some sectors: rural workers, industry and retail, construction, domestic service, and rural owners of up to 490 acres. Covered workers receive every two months the equivalent to USD 46 if household income is below USD 800, and USD 23 if household income is between 800 to 1,600 USD.

visit has to reach MIDES headquarters in the capital city, and a government official (the *reviewer*) has to check and log the information into an internal system before any change in beneficiary status takes place. Specifically, the first stage is the following:

$$UCT_{h,(t+t_0,t+t_f)} = \alpha_0 + \alpha_1 \mathbb{1}[VI_{h,t} > 0] + \alpha_2 \mathbb{1}[VI_{h,t} > 0] \times ProcTime_{h,t} + \alpha_3 ProcTime_{h,t} + f(VI_{h,t}) + \phi X_{i,h,t} + w_{i,h,t} \quad (1)$$

Where $UCT_{h,(t+t_0,t+t_f)}$ is the share of months in the range $t+t_0$ to $t+t_f$ that any member of household h receives UCT (we generally pick $t_0 = 1$, $t_f = 36$), $VI_{h,t}$ is the VI score of household h that was visited at time t, and $X_{i,h,t}$ denotes additional individual controls for individual i living in household h at time t.[48] $ProcTime_{h,t}$ is a leave-out version of processing time for household h visited at time t (i.e., mean number of months it took *reviewers* to process household visits that took place at time t, except the one corresponding to household h).

The RD polynomial is denoted by the letter f; we choose an asymmetric control function and a polynomial of degree 1 (i.e., a local linear regression). We pick a bandwidth of 0.1 in all our regressions. This is the maximum bandwidth size that we can choose and still satisfy the constraint that we not surpass a threshold where another policy change occurs.[49] Bandwidths selected using data-driven methods (Calonico et al. 2014 and Imbens & Kalyanaraman 2012) generally pick larger bandwidths, so we stick with this smaller bandwidth across all our regressions. Section 7 shows the robustness of our main results to different bandwidth sizes (including the optimal bandwidth selected with data-driven methods) and to a triangular kernel (as suggested by Gelman & Imbens 2019).

The second stage is the following:

$$Y_{i,h,T} = \beta_0 + \beta_1 \hat{UCT}_{i,h,(t+t_0,t+t_f)} + \beta_2 ProcTime_{h,t} + f(VI_{h,t}) + \gamma X_{i,h,t} + \epsilon_{i,h,T} \quad (2)$$

Where $Y_{i,h,T}$ is some outcome variable for individual i from household h measured at a time T.[50] In our baseline fuzzy RD results, we look at outcomes three years after the visit ($T = t + 36$). In all cases, we cluster standard errors at the household level.

4.1.1. Identification

Internal validity of the fuzzy RD estimates is assessed by checking that the following holds around the threshold: 1) all relevant factors other than the beneficiary status vary smoothly; 2) there is no

[48] To improve statistical power, we follow Haushofer & Shapiro (2016) and condition on the pre-visit level of the outcome variable when possible. In addition, we include year and month fixed effects, a dummy for female, a dummy for Montevideo, age, and age squared.

[49] Specifically, we choose a bandwidth of 0.1097. Households not in Montevideo that surpass the UCT threshold by more than 0.1097 duplicate the amount they receive in UCT. In Montevideo they duplicate the amount if they surpass the UCT threshold by more than 0.134. Also, eligibility for some family counseling programs managed by MIDES (such as *Programa Cercanías*) jumps exactly at this level (Perazzo et al. 2016).

[50] Note that in the first stage the dependent variable does not depend on i, but technically speaking, its fitted value could depend on i if we include individual controls in the regression.

selective sorting or manipulation; 3) no other policies change discontinuously; and 4) crossing the threshold induces a jump in the probability of being treated (i.e., of being a beneficiary of the UCT).

We check for balance in multiple pre-visit characteristics (or characteristics measured on the basis of the visit at which MIDES surveys the household), both in the entire sample and in the Montevideo/non-Montevideo split. Table 2 presents the RD estimate (with no controls, and an asymmetric linear control function) with a pre-visit characteristic as dependent variable centered at the threshold (we consider a bandwidth of 0.1).

Out of 47 coefficients, only 3 are statistically significant (and at the 10% level), so we conclude that the conditional expectation of the potential outcomes seems to be continuous at the threshold. We first look at basic demographics such as percent female in the household, mean age, year of the visit, household size, percent of visits in Montevideo, and women in the 18-40 age group that are pregnant. With the exception of age in Montevideo (which is marginally significant at the 10% level), the coefficients are statistically insignificant. Second, we look at two food insecurity measures, and despite some imbalance in the non-Montevideo sample, the other coefficients do not suggest a systematic bias in this domain. Third, we look at variables related to employment, income, and education. For schooling and formal labor supply, we use exactly the same sample that is used to run our regressions, and it is reassuring to find practically no imbalance in these variables. Fourth, we look at whether there is some imbalance in pre-visit enrollment in safety-net programs (including the baseline values of outcomes we study later), or in previous visit requests, as this is related to an outcome variable we study later (re-visit requests after the initial visit). Finally, we look at whether there is an imbalance in voting in previous Participatory Budgeting Elections in Montevideo.[51] This variable is interesting, as it captures political participation, which is not captured in our other variables; we do not find that those who are more "politically active" are differentially selected around the threshold.

To check for selective sorting or manipulation, we test the continuity of the running variable density function around the threshold, following McCrary (2008). The t-statistic of the McCrary test is 0.025, which means we fail to reject the null hypothesis of no discontinuity in the density function of the running variable at the threshold.[52]

To the best of our knowledge, there are no other policy changes occurring on either side of the cutoff (and within the bandwidth). The VI is also used by the Social Security Administration to define CCT eligibility and by MIDES to define eligibility to receive double UCT. However, the cutoff for CCT and double UCT in Montevideo (non-Montevideo) are 0.22488131 (0.25648701) and 0.7568 (0.81), respectively. The cutoff for (single) UCT in Montevideo (non-Montevideo) is 0.62260002 (0.70024848), so by picking a bandwidth of 0.1 we are excluding these other policy changes from our

[51] These are local elections that take place every three years in the capital city, where citizens of age 16 or over can vote on a number of projects to be funded by the municipal government. See Cabannes (2004) for a systematic analysis of participatory budgeting experiences in Latin America and Europe.

[52] If we split the sample into Montevideo/non-Montevideo and we conduct the McCrary test, the same conclusions apply (t-stats are −0.27 and 0.19, respectively).

estimation sample.[53]

Finally, we show that crossing the UCT threshold induces a discrete jump in the probability of receiving UCT after the visit. In Figure 3 we show the beneficiary status $3, 6, 9, 12, 24$, and 36 months after the visit as a function of the VI score for households that were initially not receiving UCT.[54]

Three points are worth mentioning. First, the beneficiary status is not instantly updated after the visit, and it peaks and (partially) stabilizes around the 12th month. Second, although we see a discrete jump (upwards) in the probability of receiving a UCT at the threshold several months after the visit, it does not jump from 0 to 1 (the jump is of size approximately 60 pp one year after the visit). This is due to some exceptions to this assignment mechanism (see Section 2.2), because individuals can get revisited, and also because UCT can be withdrawn if the household is receiving CCT but does not meet CCT's means test at some point in time. Third, this design corresponds to what some authors call a type II fuzzy regression discontinuity design (see Jacob & Zhu 2012), as we have both "crossovers" (i.e., households below the threshold that get treated) and "no-shows" (i.e., households above the threshold that do not get treated).

4.2. Dynamic differences-in-differences design

We employ a dynamic differences-in-differences design (DID) when looking at impacts on formal labor supply (this is the outcome for which we have the longest time series). The advantage of this strategy relative to the fuzzy RD is threefold. First, it allows us to measure the impact of going from 2 to 0 transfers (and vice versa), instead of from 1 to 0. This constitutes a stronger treatment. Second, we no longer restrict ourselves to picking observations within the bandwidth and thus consider a larger sample size, which can help reduce some of the noise in our estimates. Third, our dynamic DID specification is orthogonal to our choice of the endogenous regressor and instruments in the fuzzy RD. Thus it can serve as a sort of robustness check that those decisions are not driving our results.

To estimate the impact of exiting the program, our treatment group is comprised of those individuals that had a VI score below the UCT threshold and that were initially beneficiaries of UCT. Pre-visit beneficiaries with VI scores between the single-UCT threshold and the double-UCT threshold constitute the control group. To estimate the impact of losing two transfers, we consider only individuals that were (pre-visit) receiving double UCT. The treatment group is comprised of those with a VI score below the single-UCT threshold, and the control group corresponds to those with a VI score above the double-UCT threshold.[55] This captures the reduced-form impact of crossing the threshold on an

[53] Another program that uses the VI to define eligibility is *Programa Cercanías*. Our estimates are not confounding the impact of this program, as eligibility is restricted to those with a VI higher than the double-UCT eligibility threshold (Perazzo et al. 2016).

[54] The same conclusions apply if we look at households that were initially recipients of UCT.

[55] Analogous definitions are used to measure the impact of gaining single or double UCT. Also, to rule out the possibility that our results are driven by entry into or exit from the CCT program, we include only individuals that were beneficiaries

outcome of interest when crossing the threshold is associated with a certain change in the beneficiary status in the UCT program.[56] Note that the treatment and control groups used to study the impact of losing one transfer never overlap with the groups used to study the impact of exiting from double UCT. Thus if we find similar results in the two specifications (perhaps stronger effects when exiting from double UCT), it is reassuring that we are estimating an exit from the program, as it is not mechanical that the two results go in the same direction. To study the impact of gaining one or two transfers, there is overlap only in the control group.

The regression equation is the following:[57]

$$Y_{i,h,t} = \beta_0 + \gamma_{i,h} + \sum_{k=-l}^{k=L} \beta_k^{Treated} \times Treated_h \times \mathbb{1}[t = k] + \sum_{k=-l}^{k=L} \beta_k \times \mathbb{1}[t = k] + yearFE_{h,t} + monthFE_{h,t} + age_{i,h,t} + age_{i,h,t}^2 + \epsilon_{i,h,t} \quad (3)$$

Here t represents the number of months before or after the visit (e.g., $t = 2$ would correspond to the observation taking place two months after the visit to household h).

5. Main results

We study how entry into and forced exit from the UCT program impact labor supply and formalization of work, human capital investments for children, and take-up of other safety-net programs. We first show binscatters to visually inspect the reduced-form result of crossing the UCT threshold on the outcome of interest.[58] Second, we estimate our fuzzy RD regression where we show results for the "pooled sample" (i.e., not restricting the sample according to pre-visit beneficiary status), as well as for the sample of pre-visit non-beneficiaries and for the sample of pre-visit beneficiaries. For brevity, and when the binscatter shows a clear pattern, we do not show the fuzzy RD results table in the main text, and rather just describe the results.

of CCT before the visit and one year after the visit.

[56] We could do this event study with the date on which the beneficiary status changed as $t = 0$ instead of the visit date. However, we could expect to see changes in behavior starting with the month of the visit if expectations of receiving UCT change with the visit. This is especially relevant in a DID setting (as opposed to an RD setting), as individuals with high enough VI scores could presumably expect that they will receive UCT at some point, and households with low enough VI scores could expect that they will most likely not be part of the UCT program. Thus the visit itself changes expectations on receipt of UCT differentially for the treatment and control groups.

[57] Two regressors are excluded from the regression ($Treated_h \times 1[t = -1]$ and $1[t = -1]$), which normalize $\beta_{-1}^{Treated}$ to 0.

[58] We do not comment on the exact magnitude or statistical significance of the results when looking at the binscatters, as this could be misleading. However, we do this when we show the fuzzy RD results. As this is a fuzzy RD and not a sharp design, the actual discontinuity found in the binscatters has to be scaled by the appropriate "first stage" estimate.

5.1. Formal labor supply

We start with what is perhaps one of the oldest topics of interest in labor economics: the relationship between welfare programs and the labor market (Chan & Moffitt 2018). Figure 4 shows the reduced-form impact of crossing the eligibility threshold for the UCT on the probability that an individual is formally employed three years after the visit. We focus on individuals in the 18-38 age group three years after the visit whose UCT benefit is (or would be if they receive it) indirectly means tested (i.e., someone in the household is a beneficiary of CCT at the time of the visit), where results are relatively stronger.

We see that there seems to be a drop in the probability of formal employment three years after the visit if an individual is on the UCT-eligible side of the threshold, and Table 3 confirms this observation. Column 1 shows that receiving the transfer during the three years after the visit decreases formal labor supply at the end of those three years by 2.7 percentage points. This effect is driven by those that were initially not receiving the transfer, which decreased their formal labor supply by 4.4 pp. We see no changes among those that were initially receiving the transfer, although in some sub-samples we do find an impact among this population (e.g. if we restrict the sample to Montevideo, then we get a coefficient of −6.7% and statistically significant at the 10% level).

We complement the fuzzy RD estimates with estimates from a dynamic DID model, where we do find statistically significant impacts both of entry into and exit from the program. Panel *a* in Figure 5 shows that losing the transfer has a positive impact on formal labor supply (up to +3 pp). Panel *b* shows the impact of a larger loss that we could not measure in our fuzzy RD: initially (pre-visit) receiving double UCT and losing all UCT benefits after the visit. There is more noise in these estimates, as the sample of people that go from double UCT to none is relatively small, and also it is not as clear as in the previous graph that we have parallel pre-trends. Despite these facts, we see basically the same picture, perhaps with slightly higher estimates (as would be expected, given that the loss is larger than in panel *a*, although it could also be related to the pre-trends). Panels *c* and *d* show the impact of gaining a transfer, going from no transfer to either (single) UCT or double UCT. It is reassuring to see that in these cases the impacts on formal labor supply are negative (consistent with the direction of the results in panels *a* and *b*).

With respect to results for non-beneficiaries of CCT at the time of the visit or the next 20-year cohort (the 39-59 age group; note that the retirement age starts at 60): we basically find no statistically significant impacts on these groups (with the exception that we do find some impacts on the next 20-year cohort in the DID specification, and in the same direction than the results shown in Figure 5). Given that our results on labor supply are being entirely driven by the sample of people whose transfer is means tested (i.e., beneficiaries of CCT), this is suggestive that the mechanism driving the formal labor supply responses is the means testing of the CCT program.

5.2. Education

We look at impacts on enrollment in public schools.[59] As explained in Section 2.1, public education in Uruguay can be divided into three subsystems: CEIP, which offers elementary and primary education; CES, which offers secondary education; and CETP, which offers secondary and vocational education. Panel (*a*) in Figure 6 shows the probability of being enrolled in any of those three subsystems three years after the visit as a function of the VI score. Our sample corresponds to minors that finished primary school (or are in the last year of primary school) and still need at least three more years of study to finish secondary school. We see that crossing the threshold seems to induce a drop in enrollment rates.

Table 4 shows the results of the fuzzy RD estimates, which confirm what was apparent (though not entirely clear) in the binscatter. Being enrolled in the program during the three years following the visit reduces the probability of being enrolled in the public education system three years after the visit by 3.3 pp. This effect is driven entirely by those that were initially not receiving the transfer.

The results are puzzling (and worrisome). It may seem that entering the program decreases human capital accumulation of children. However, we note that passing rates in this context are remarkably low, and so it is not clear whether a decrease in enrollment leads to a drop in completed years of schooling. We have administrative data on the grade in which the individual is enrolled, which allows us to compute the number of completed years of schooling for each individual. Panel (*b*) in Figure 6 shows that crossing the threshold does not seem to have a clear impact on this measure.[60]

5.3. Take-up of safety-net programs

Another form of dependency could take place if receipt of UCT induces individuals to take up more welfare. In particular, we study whether UCT impacts enrollment in housing assistance programs managed by the Ministry of Housing or enrollment in a cash assistance program for formal workers managed by the Social Security Administration. These programs are managed by different institutions that do not use the VI to assess eligibility.

Panel (*a*) in Figure 7 shows the reduced-form impact of crossing the UCT eligibility threshold on the probability that an individual is enrolled in a housing assistance program three years after the visit. There seems to be a drop in that probability if an individual is on the UCT-eligible side of the threshold. However, given the low enrollment rates of this population in housing assistance programs,

[59] If we look at the poorest 5% of households (which would correspond to the target population of the UCT program) in a nationally representative survey in 2013 ("Encuesta Continua de Hogares INE 2013"), among those minors that go to school, 99% do so in the public system. Thus we interpret our estimates as the impact on schooling overall (not only public schooling).

[60] Moreover, while point estimates of the the fuzzy RD are negative in this case, the effect is non-statistically significant.

the binscatter is noisy and it is not clear from just visual inspection whether there is actually a drop in enrollment.

Table 5 shows that there is actually evidence that the program induces a drop in enrollment in housing assistance programs. Column 1 shows that receiving the transfer during the three years after the visit decreases enrollment in housing programs at the end of those three years by 1.2 percentage points.[61]

Finally, we study whether UCT has an impact on enrollment in a cash assistance program offered to formal workers with dependents.[62] Panel (*b*) in Figure 7 shows the reduced-form impact of crossing the UCT threshold on enrollment in this program for adults living in households with children at the time of the visit. Column 1 in Table 6 shows that receiving UCT during the three years after the visit decreases enrollment in this cash assistance program by 0.8 pp (the mean for non-UCT recipients is 2.7 pp, so the effect is non-negligible). This effect is driven entirely by pre-visit non-beneficiaries.

6. Discussion

The previous section presented our main results but did not discuss some aspects of their interpretation and potential mechanisms. Also, we did not look at other outcomes that could be affected and that could impact our overall assessment of the UCT program (relevant for any policy implications that could be derived from this study). In this section, we first show how UCT positively impacts material hardship in the household. This not only indicates that the program has beneficial effects on the main outcome it is supposed to impact but also suggests a possible mechanism behind the substitution effect we find with housing assistance programs. Second, we exploit our survey data, and the fact that a subset of households are revisited, to re-examine the impacts of the program on labor supply and education. These results suggest that formal labor supply responses operate mostly through the formality margin, and corroborate the drop we see in school enrollment with administrative data. Third, we show that individuals engage in several strategic behaviors to try to enter or stay in the program.

6.1. Material hardship

While in this study we focus on three key outcomes to assess family welfare dependency (i.e., formal labor supply, education, and take-up of other safety-net programs), there are other key outcomes that this program impacts. In particular, we look at its effects on food insecurity, durable goods consumption, and housing conditions.

[61] In Montevideo, where results are stronger, we find that both entry (−3.2 pp) and exit (+4.7) have a statistically significant impact on enrollment in housing assistance programs.

[62] See Section 3.4 for details on this program.

There are three questions that MIDES asks at the visit that could serve as proxies for food insecurity: *In the last 30 days, did it ever happen that there was not enough food in the household because of lack of money? In the last 30 days, was it impossible for you or another adult in the household to have breakfast, lunch, or something to eat because of lack of money? In the last 30 days, did it ever happen that a child in your household had less to eat than what it was accustomed to because of lack of money?*

Figure 8 shows results for the question on food insecurity in the household for minors.[63] We see that crossing the threshold seems to induce a sharp drop in food insecurity. These results suggest that the transfer is spent, at least in part, on food. However, part of it could also be spent on other goods or services that the household may need.[64]

Figure 9 shows the impact of UCT on durable goods in the household. We see that the UCT increases durable goods consumption in the household across the board (perhaps with the exception of cell phones, computers, and washing machines).[65]

Figure 10 shows the impact of receipt of UCT on variables related to the "quality" of the dwelling and two neighborhood characteristics that we use as a sort of placebo check. We see that UCT seems to have a positive impact on the quality of floors, roofs, and the general condition of the dwelling, and a negative impact on undesirable housing conditions such as overcrowding or having an exposed electrical connection. We add two variables that measure not what happens in the household per se but instead in the block where the home is located. Specifically, the household is asked whether there are illegal dump sites or toxic waters in the neighborhood, and we find that the fact that a single household (quasi-randomly) receives the transfer has no impact on these variables.

The results on material hardship not only indicate that the program positively affects the (material) well-being of beneficiary households but could also suggest a mechanism behind the impacts we found in regard to take-up of housing assistance programs. It would be hard to argue that the reason we see that UCT negatively impacts take-up of housing assistance is that enrollment in UCT increases the costs of enrolling in a housing assistance program (either costs of acquiring information, cost of applying, or stigma). One hypothesis is that receipt of UCT decreases the marginal benefit of enrolling in a housing assistance program. Most of these programs involve relocation, and we see that receipt of UCT improves housing conditions, which in theory should decrease the marginal benefit of moving to a different location.

[63] We get similar results by looking at the other two measures.

[64] We refer to the additional expenditures that the transfer induces, not the actual purchases made with the food card. There is a large literature that studies the marginal propensity to consume food as a result of a cash transfer (e.g., Bruich 2014) and finds that it is less than 1, hence it seems plausible that there are impacts on other consumption goods.

[65] The null impact on cell phones is expected, given that practically all households in our sample have cell phones (91%).

6.2. Labor supply and human capital investment responses with survey data

We exploit data on self-reported employment status at future visits to assess whether the impacts we find on the administrative data on formal employment are due to a drop in labor supply or are associated more with a shift to the informal sector. We also look at self-reported school attendance in order to cross-validate the results we find on enrollment in the administrative data.[66] Figure 11 shows the reduced-form impact of crossing the eligibility threshold on self-reported employment and school attendance, as measured in a future visit.

First, there does not seem to be an impact on labor supply. This evidence, together with the fact that formal labor supply responses are driven by beneficiaries whose UCT is indirectly means tested, suggests that the formal labor supply responses we observe in the administrative data are due to a decrease in the formalization of work and probably not associated with decreased employment. Second, we see a drop in self-reported school attendance (the RD coefficient is actually statistically significant at the 5% level). This brings supporting evidence to our school enrollment findings with administrative data.[67]

6.3. Strategic behaviors: complaints, selective deafness, and misreporting

Individuals seem to take several actions to sustain/gain eligibility. In this section we document three of these. One action is to call or show up at one of the MIDES offices across the country and request a visit to have the household situation assessed/re-assessed. Another is the decision whether to answer the door when a MIDES agent shows up to survey the household. A third one is to misreport information on the MIDES questionnaire.

Figure 12 shows the reduced-form impact of crossing the eligibility threshold on the probability that the household requested a re-visit within a year after the visit took place. We consider only visits that took place between January 2015 and December 2017, as data on visit requests are available for only January 2015 to December 2018. There is a large drop just above the threshold, which suggests that gaining/keeping receipt of UCT is associated with a drop in the probability of requesting a re-visit.[68]

[66] In both cases, we consider the same sample definitions we used when looking at the impacts on administrative data. However, within these samples we have data (on the self-reported outcome) on only those that were visited at least once after the initial visit.

[67] Although not entirely comparable, results in self-reported data seem to be even stronger than in the administrative data. One reason for this could be that enrollment may not necessarily adjust "instantly" when an individual quits school, and individuals could stay enrolled even if they do not regularly attend school. Self-reported attendance could presumably be more elastic.

[68] The RD results indicate that being a beneficiary after the visit induces a drop in the probability of requesting a re-visit by 24 percentage points. Moreover, the impact seems to be stronger for those initially enrolled in the program. Put differently, while losing the transfer increases the probability that a household requests a re-visit by 31 percentage points,

Another behavior we observe is that individuals seem to answer the door in different ways, depending on their initial beneficiary status. We first introduce some indirect evidence of this behavior by looking at area re-visit rates. After that, we show more direct evidence by studying the history of visit attempts for targeted visits.

As explained in Section 2.3, whether MIDES decides to visit a household within an area visit should be orthogonal to its characteristics (controlling for the characteristics of its neighbors). Thus we would expect to find the same probability of an area re-visit for households that were visited and registered a VI score right below or above the threshold. However, panel (*a*) in Figure 13 shows that this is not the case. We notice that crossing the threshold induces a sharp drop in the probability of getting an area re-visit in the future.

Our reading of this result is that individuals either spend less time in the house or decide (in a higher proportion) not to open the door when a MIDES official shows up if they are enrolled in the UCT program.[69] The logic is that beneficiaries may have more to lose than to gain from a re-visit, so they may be resistant to getting re-tested (especially beneficiaries that "just" entered the UCT program in the first place, even if they are not fully aware that their entry was a borderline case). Nevertheless, we acknowledge that, at best, this is only indirect evidence that households are selectively deaf when a MIDES official knocks at the door.

More evidence of this behavior can be found by looking at targeted visits instead. For these visits, MIDES has the history of all the visit attempts before the household was successfully surveyed. We do not have the actual dates on which MIDES attempted to visit the household, but we have the number of attempts before each (successful) targeted visit occurred. We first look at the raw data, where we find that among those visited households that were (pre-visit) beneficiaries, the mean number of attempts before the (successful) visit takes place is 0.58. The corresponding number for (pre-visit) non-beneficiaries is 0.42. Thus in a quick comparison among (pre-visit) beneficiaries and non-beneficiaries, we do find that beneficiaries seem to be the harder population to survey.

Of course, the previous comparison does not show causality, and in fact given that we have data on attempts for successful visits but not for visits that never materialized, we cannot conclusively identify the effect. The closest we can get to identifying an effect is as follows: we take all individuals visited on a targeted visit with at least one (successful) previous visit. We run a regression with the number of unsuccessful attempts before the last successful visit to an individual as dependent variable, beneficiary status at the last successful visit as the endogenous regressor, and construct our usual instrument with the VI score from the first visit.

gaining a transfer reduces the probability of requesting a re-visit by only 23 pp. These differences could be due to how people differentially respond to losses and gains, or it could also be a function of observable and unobservable differences across our (pre-visit) beneficiary and non-beneficiary populations.

[69] If anything, our results on formal labor supply and education would suggest that people enrolled in UCT would be more likely to spend time in the house, so labor supply/education responses should not be driving these differences.

Panel (*b*) in Figure 13 shows the mean number of attempts by MIDES before successfully surveying the household as a function of the VI score computed on the basis of the previous successful visit. We see that being on the right side of the threshold (i.e., eligible for UCT) on a given visit seems to be associated with a higher number of visit attempts for the next visit (among the group of individuals that have at least two visits).

While the three pieces of evidence on selective deafness (i.e., differential area re-visit rates, differences in mean visit attempts in the raw data, and fuzzy RD estimates) do not constitute proof of this behavior, they are suggestive of it.

Finally, we study whether individuals misreport information in the interview when asked about their receipt of CCT. We generate a variable equal to 1 if a household is enrolled in CCT according to administrative records from the Social Security Administration but reports that it is not enrolled in CCT during the household visit conducted by MIDES. We find that receipt of UCT has no impact on this variable (misreporting) at future visits (both by visually inspecting the binscatter and by looking at the RD results). However, the binscatters show positive values of 8% around the threshold. Thus while receipt of UCT seems to have no impact on future misreporting, the evidence suggests that households misreport their status in the CCT program.

7. Robustness of the main results

Bandwidth size and kernel specification. We investigate the robustness of our fuzzy RD estimates to the choice of bandwidth size and kernel specification (we consider both rectangular and triangular kernels). Assessing the robustness of our results to different bandwidth sizes is especially warranted in this setting, given that our bandwidth (0.1) was not selected with a data-driven procedure. A triangular kernel is a popular choice in RD studies (see Gelman & Imbens 2019), so it also seems natural to check that our results are robust to this kernel specification. We run regression (2) for each of our main outcomes with bandwidths in $\{0.05, 0.06, \ldots, 0.2\}$. A vertical green line indicates the bandwidth chosen by a data-driven method ("MSERD"; see Calonico et al. 2014), and a blue line indicates our baseline bandwidth for reference.

The first row of Figure 14 shows the robustness of our main formal labor supply result (which corresponds to column 1 in Table 3). The estimates maintain similar magnitudes and mostly retain significance for all bandwidths considered, perhaps with the exception of very small bandwidths in panel *b*. However, these bandwidths are quite far away from the data-driven optimal bandwidth in this specification.

The stronger results we found on education were actually within the sample of (pre-visit) non-beneficiaries. Row 2 in Figure 14 shows the robustness of this estimate to different bandwidth and kernel specifications. The estimates maintain similar magnitudes and mostly retain significance for

all bandwidths considered, at least for the bandwidths that are close to the optimal one.[70]

Falsification tests. Even though our results seem to be mostly robust to the choice of bandwidth and kernel specification, visual inspection of the binscatters raises the concern of whether we would have found similar impacts if we had just set the threshold at a different value. Because in several of our outcomes mean values around the threshold are noisy, and because we are dealing with a fuzzy RD (not a sharp RD), binscatters alone hardly tell a conclusive story in our setting. To check that changes in outcomes at the threshold are not a random feature of the data, we estimate the reduced-form impact of crossing a given threshold (i.e., the "gap" between the two regression lines that we see in binscatters) considering all possible thresholds that do not confound effects with other policy changes (i.e., CCT and double UCT).[71] In all cases, we consider the same bandwidth size (0.1).

To be clear, this is the regression specification:[72]

$$Y_{i,h,t+36} = \beta_0 + \beta_1 \mathbb{1}[VI_{h,t} > 0] + f(VI_{h,t}) + \gamma X_{i,h,t} + \epsilon_{i,h,T} \tag{4}$$

Figure 15 shows results of these falsification tests for the main outcomes studied.[73] We find that the most negative – and the only statistically significant – coefficient (out of the 40 falsification tests we perform for each variable) is the one that corresponds to the "real" (non-placebo) threshold. This holds true for all variables except housing assistance (panel c). With respect to this variable, and despite the fact that we see a negative coefficient at the real threshold, this also occurs in 4 of the other falsification tests. Moreover, there is a large positive coefficient at the placebo threshold -0.2 which suggests that the negative coefficient we observe at the real threshold could be directly related, at least in part, to what happens in a small neighborhood of our sample. Nevertheless, we see in the third row of Figure 14 that even with large bandwidths our result retains significance, so the upward jump right below the threshold does not seem to be the sole explanation of this result.

Overall, we read this evidence as strongly suggestive that our results on formal labor supply, education, and take-up of cash assistance for formal workers are not a random feature of the data, but rather are related to receipt of UCT. The evidence is less clear with respect to housing assistance, and that result should be considered with caution.

[70] Row 3 and 4 in Figure 14 shows the robustness of our main result on take-up of housing assistance, and take-up of cash assistance for formal workers. In both cases, the estimates maintain similar magnitudes and mostly retain significance for all bandwidths considered.

[71] We actually take all of these possible thresholds considering values that are 0.01 VI score points away from each other. The maximum value we take is the actual UCT threshold (above that value, the group to the right of the threshold would be confounded with other policy changes), and the minimum is -0.39 (we would confound effects with receipt of CCT if we chose a lower value).

[72] See Section 4 and the description of equation (2) for definitions of the variables.

[73] For formal labor supply and school enrollment, we perform these placebos on the sample of pre-visit non-beneficiaries, which are the samples driving our results on those outcomes.

8. Conclusions

Cash assistance programs have been criticized for inducing behaviors that make individuals more reliant on these transfers and less on their own means to reach a basic standard of living. Most evidence on this topic measures the impact of receipt of cash assistance while an individual is still enrolled in the program or at some point after he stops receiving the transfer. However, assessing the possibility of welfare traps with this type of evidence could be misleading.[74] This is especially the case if individuals that enter a cash assistance program have no pre-specified exit date (as in many government-run cash assistance programs around the world) and exit is endogenous to their behaviors.

In this paper we assess the existence of welfare traps by providing separate causal estimates of the impact of entry into and forced exit from an unconditional cash transfer program based on both regression discontinuity and dynamic differences-in-differences designs. To identify these effects, we exploit the unique way in which the Uruguayan government decided to re-target its main unconditional cash transfer program through a proxy-means test. Through more than 250,000 household visits (during 2012 – 2018) covering roughly one-fifth of the population, the government estimated a socio-economic vulnerability score for each household and determined its eligibility for the program by comparing that score to a pre-specified value (i.e., if the score is higher than that pre-specified value, the household is eligible for the transfer). We use rich administrative longitudinal data and survey data to focus on three key indicators associated with family welfare dependency: labor supply and formalization of work, human capital investments for children, and take-up of other safety-net programs.

First, we find that labor supply of adults drops 3 pp three years after they enter the program, and that this effect is concentrated among adults under the age of 40. Nevertheless, this does not constitute a welfare trap: long-term recipients that are forced to exit the program increase their formal labor supply by 3 pp. We exploit the fact that the program is indirectly means tested initially for only a subset of the population (80%), and we find that our results are driven by this population. This suggests that the mechanism that drives people to reduce their formal labor supply is the means test per se and not an income effect. This also suggests that our results on formal labor supply probably operate mostly through the formality margin and not labor supply. We also do not find statistically significant drops in self-reported labor supply, which considers formal and informal work.

Second, we find that the transfer is associated with a lower (−3 pp) probability of being enrolled in school three years after the visit (for minors that should be enrolled in secondary school three years after the visit). However, because secondary school graduation rates in the Uruguayan context are low for low-income families, the school enrollment impacts do not translate into differential educational

[74] In programs that have a pre-specified exit date (such as a "one-time" cash transfer), welfare traps can be assessed by looking at the persistence of the effects after the individual or household stops receiving the transfer (see section "Do Cash Transfers Create Dependency?" in Haushofer & Shapiro 2016).

attainment rates. The result on enrollment is nevertheless surprising, given that we generally expect cash assistance to increase enrollment in school (or to have a null impact). Dahl & Gielen (2018) also finds that one form of cash assistance (disability insurance) negatively impacts human capital investments in children (albeit in a completely different context). The authors hypothesize that the driver of this result is that government assistance during youth impacts expectations of government assistance during adulthood, which is similar to the scarring effect in Malmendier & Nagel (2011). Testing this hypothesis in our context could be an interesting avenue for future research.

Third, we find that the program has a negative impact on enrollment in public housing programs (−1.2 pp, although this result should be taken with caution) and take-up of other types of public cash assistance (−0.8 pp). This is more suggestive of safety-net program substitution than of increased dependency on multiple programs. A back-of-the-envelope calculation suggests that for every $100 that the government reduces today in UCT benefits, total public spending is reduced by only $87 three years later, as a result of safety-net program substitution and a targeting mechanism that allows former beneficiaries to re-enter the program.[75]

Overall, these results suggest that the program does not induce a welfare trap. While it is true that beneficiaries reduce their formal labor supply on entry, the fact that beneficiaries that are forced to exit welfare increase their formal labor supply suggests that the program does not decrease beneficiaries' *ability* to find employment. Although we find negative impacts of the program on education enrollment, this does not create a welfare trap, as these impacts do not translate into different educational attainment rates. Take-up of other welfare programs can also be ruled out as a potential driver of dependency, as we find safety-net programs to be substitutes in this context.

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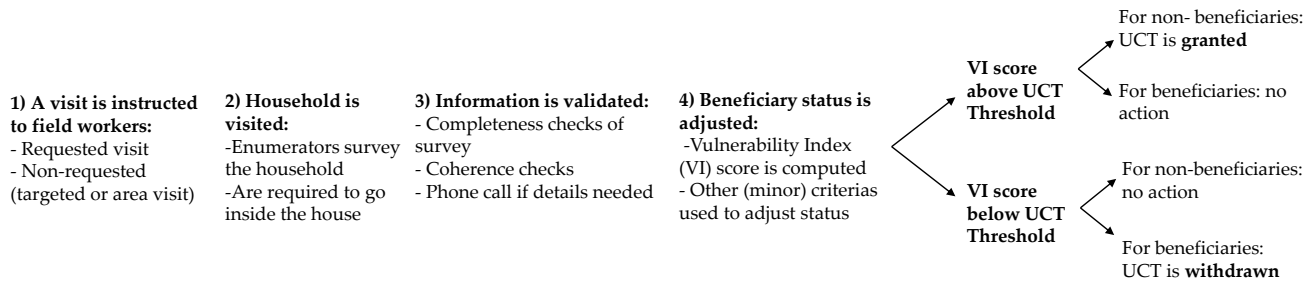
Figures

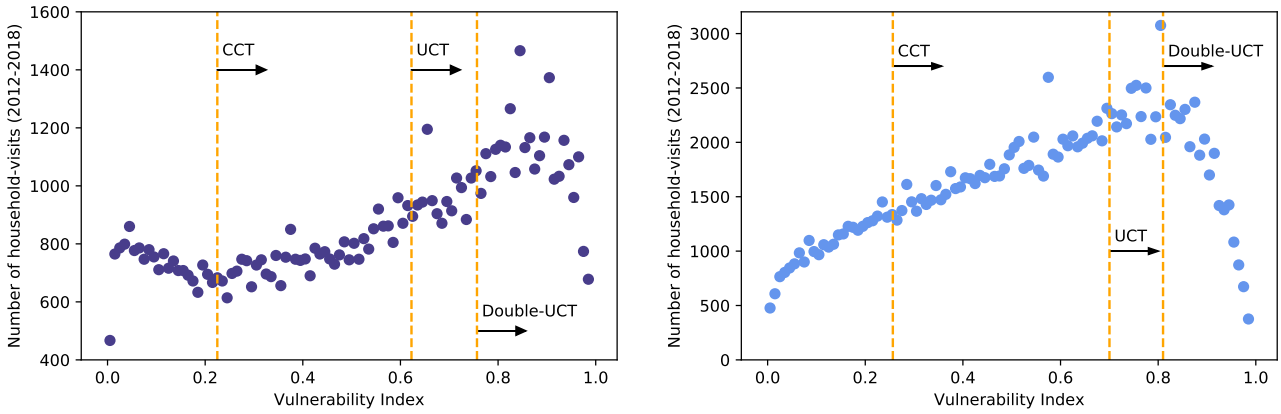
Figure 1: Steps followed by MIDES to grant or withdraw UCT



Notes: This figure schematically shows the steps taken by MIDES (since 2012) to grant/withdraw an UCT. It is based on information obtained from regulatory decrees, internal reports elaborated by MIDES (DINEM 2011, 2012), and interviews to MIDES officials.

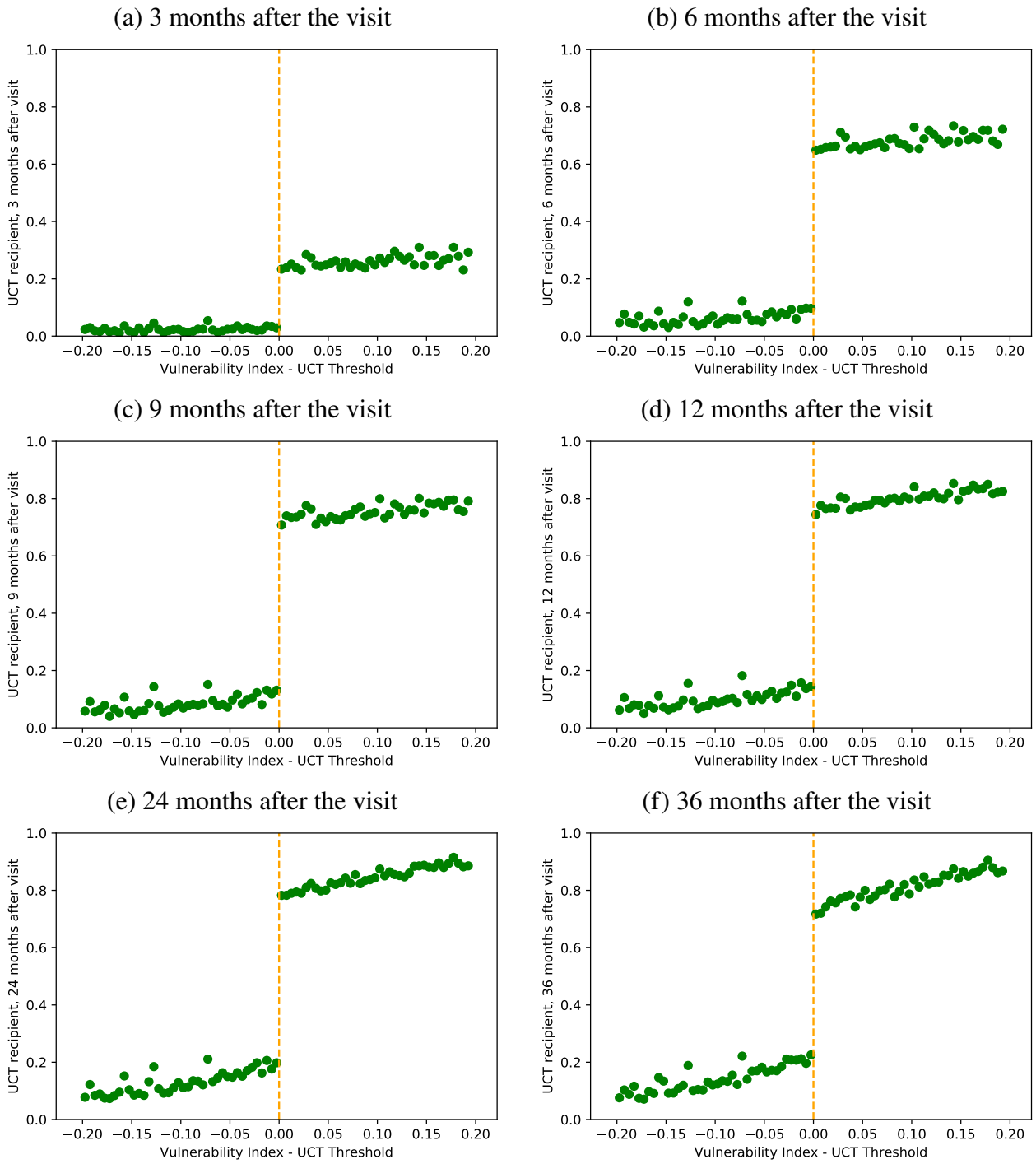
Figure 2: Density of household visits by Vulnerability Index

(a) Montevideo (b) Rest of the country



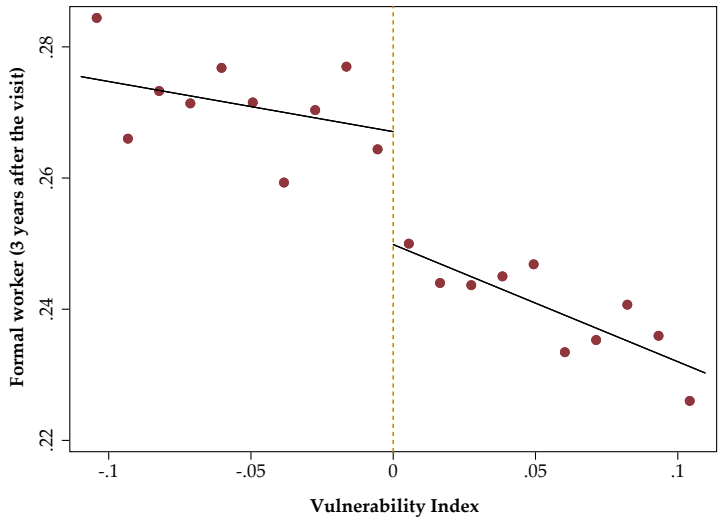
Notes: This figure shows the number of households visited by MIDES from January 2012 to July 2018, as a function of their Vulnerability Index score computed on the basis of the visit (for Montevideo in panel *a* and the rest of the country in panel *b*). From left to right: the first orange line indicates the minimum score needed to gain eligibility for the CCT program, the second line corresponds to the UCT program, and the third line to double UCT. The source of the data is MIDES.

Figure 3: First stage for pre-visit non-beneficiaries



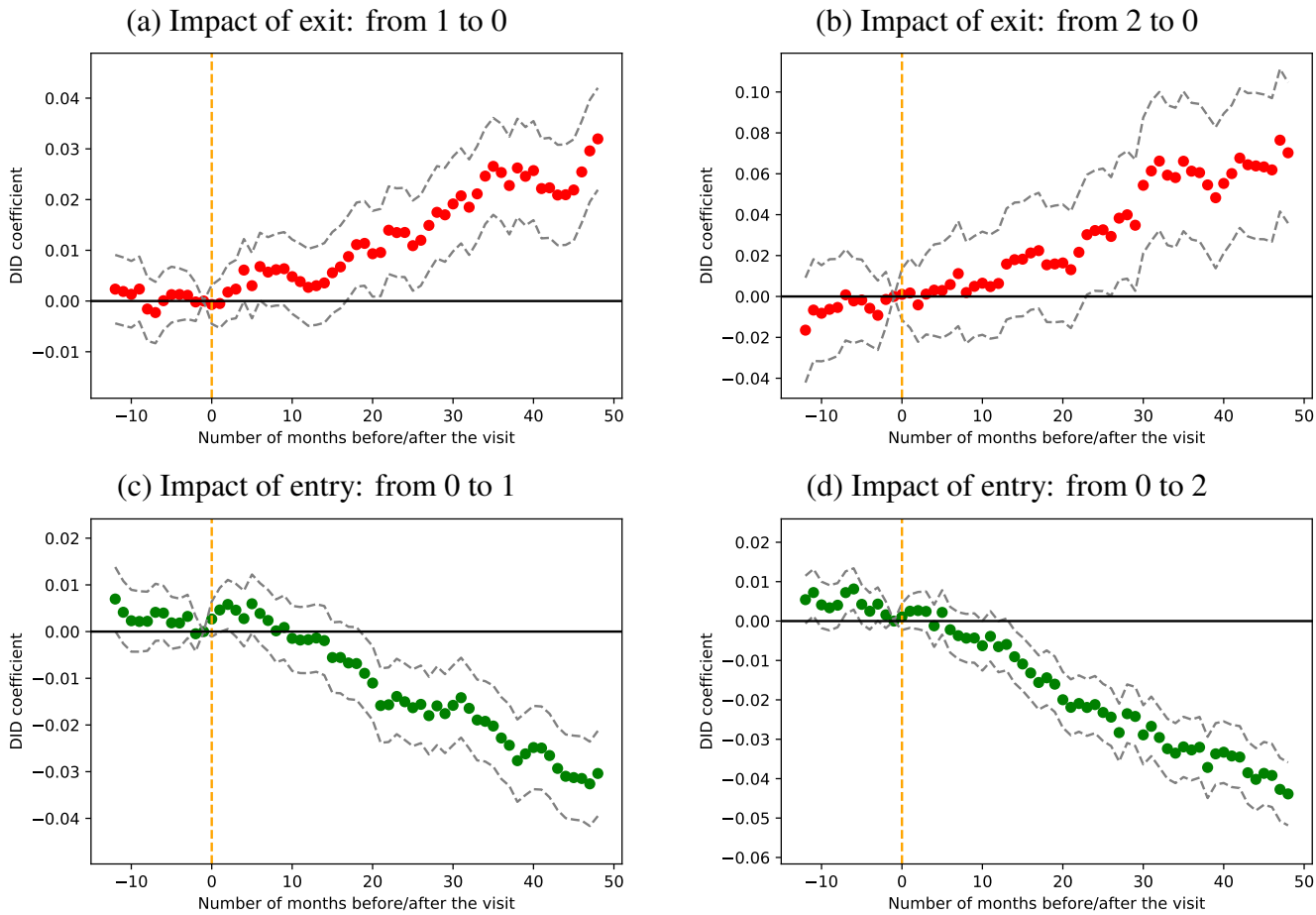
Notes: This figure shows binscatters of the share of households enrolled in the UCT program $3, 6, 9, 12, 24$, and 36 months after a visit takes place as a function of their Vulnerability Index scores (normalized to 0 at the UCT eligibility threshold). These binscatters use only the sample of pre-visit non-beneficiaries of UCT. Those with Vulnerability Index scores to the *right* of the cutoff are eligible to receive UCT, and those to the *left* of the cutoff are not (with exceptions).

Figure 4: Impact of UCT on formal labor supply



Notes: The variable plotted on the vertical axis is the percentage of individuals within a VI bin that are formally occupied three years after the visit. The horizontal axis shows the Vulnerability Index score computed on the basis of the visit (normalized to 0 at the UCT eligibility threshold). We consider 10 equal-sized bins on both sides of the cutoff. The sample corresponds to all individuals that were in the 18-38 age group three years after the visit. Both the dependent variable and the running variable are residualized on controls before plotting. The controls considered are female, dummy for being formally employed during the month before the visit, year FE, month FE, and Montevideo FE. Those with Vulnerability Index scores to the *right* of the cutoff are eligible to receive UCT, and those to the *left* of the cutoff are not (with exceptions).

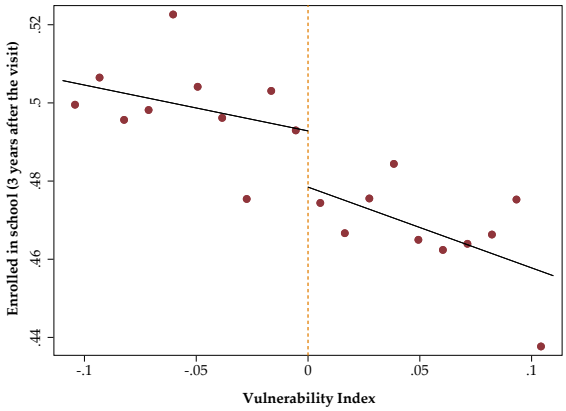
Figure 5: Dynamic DID estimates: formal employment



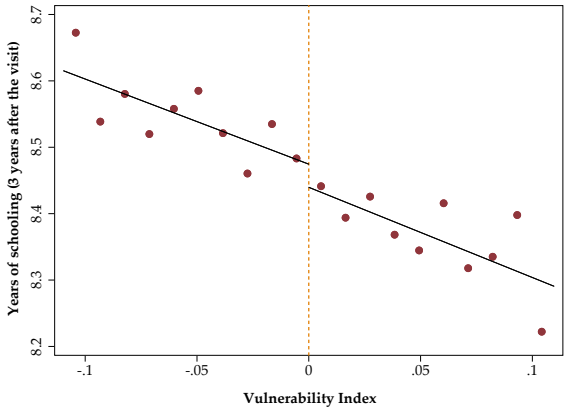
Notes: This figure shows regression coefficients and associated 95% confidence intervals for the difference between treatment and control individuals k months before/after the visit, that is, the $\beta_k^{Treated}$ from equation (3). The coefficient for $k = -1$ is normalized to 0. The outcome variable is a dummy equal to 1 if the individual is formally employed. Besides leads and lags and their interaction with treatment, the specification includes individual-visit FE, age, age squared, year FE, and month FE. Standard errors are clustered at the household level. All regressions include individuals in the 18-38 age group three years after the visit that were beneficiaries of the CCT program, both before and after the visit. Panel (a): The treatment group are those whose VI is below the single-UCT threshold and that were receiving single UCT at the time of the visit; the control group are those with VI between the single-UCT threshold and the double-UCT threshold. Panel (b): The treatment group are those whose VI is below the single-UCT threshold and that were receiving double UCT at the time of the visit; the control group are those with VI above the double-UCT threshold. Panel (c): The treatment group are those whose VI is between the single-UCT threshold and the double-UCT threshold and that were not receiving UCT at the time of the visit; the control group are those with VI below the single-UCT threshold. Panel (d): The treatment group are those whose VI is above the double-UCT threshold and that were not receiving UCT at the time of the visit; the control group are those with VI below the single-UCT threshold).

Figure 6: Impact of UCT on schooling

(a) Enrollment



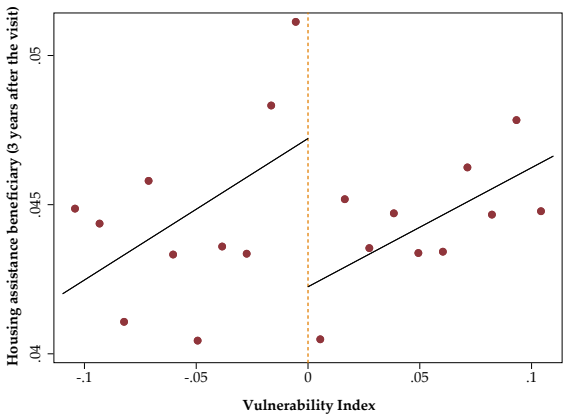
(b) Years of school



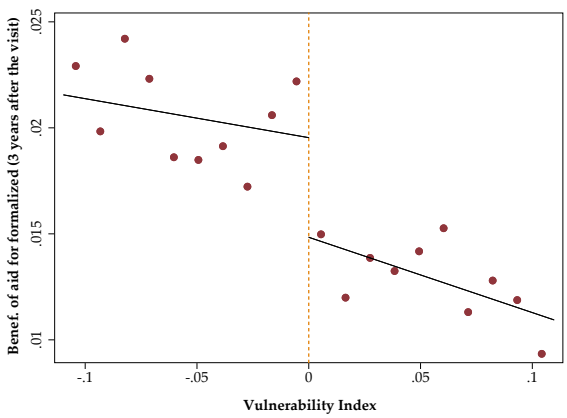
Notes: In panel *a* the variable plotted is the percentage of individuals within a VI bin that are enrolled in a public school (primary, secondary, and/or vocational) three years after the visit. The horizontal axis shows the Vulnerability Index score computed on the basis of the visit (normalized to 0 at the UCT eligibility threshold). We consider 10 equal-sized bins on both sides of the cutoff. The sample corresponds to minors that finished primary school (or are in the last year of primary school) and need at least three more years to finish secondary school. Both the dependent variable and the running variable are residualized on controls before plotting. The controls considered are female, a dummy for enrollment in public school during the month before the visit, year FE, month FE, and Montevideo FE. The same applies to panel *b*, with the exception that the outcome variable is the mean number of completed years of schooling three years after the visit.

Figure 7: Impact of UCT on take-up of safety-net programs

(a) Housing

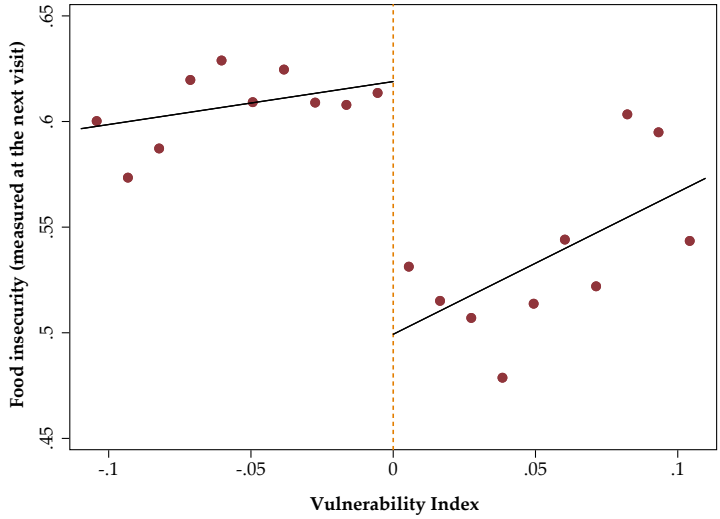


(b) Cash assistance



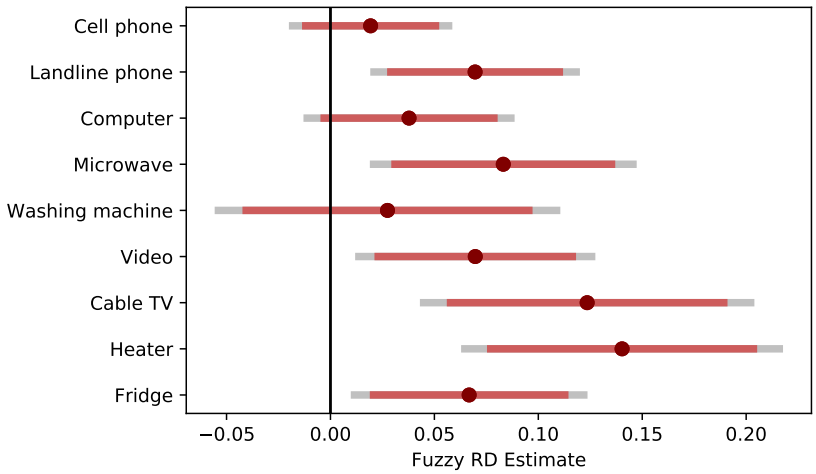
Notes: In panel *a* the variable plotted is the percentage of individuals within a VI bin that are enrolled in a housing assistance program offered by the Ministry of Housing three years after the visit. The horizontal axis shows the Vulnerability Index score computed on the basis of the visit (normalized to 0 at the UCT eligibility threshold). We consider 10 equal-sized bins on both sides of the cutoff. Both the dependent variable and the running variable are residualized on controls before plotting. The controls considered are female, year FE, month FE, and Montevideo FE. The same applies to panel *b*, with the exception that the outcome variable is the percentage of individuals within a VI bin that were receiving cash assistance for formal workers (in the program managed by the SSA) three years after the visit, the sample corresponds to adults living in households with children at the time of the visit.

Figure 8: Impact of UCT on food insecurity



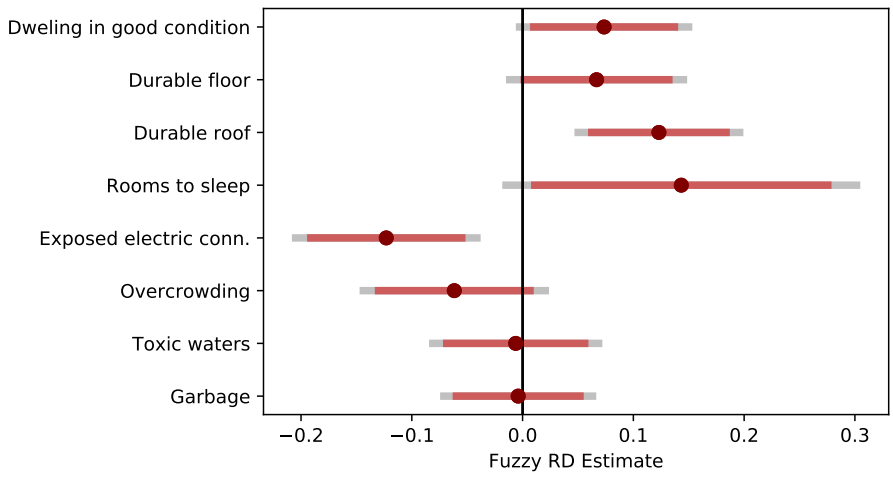
Notes: The variable plotted on the vertical axis is the percentage of individuals within a VI bin that live in a household where minors are food insecure (self-reported) in the next visit ($n+1$). The horizontal axis shows the Vulnerability Index score computed on the basis of visit n (normalized to 0 at the UCT eligibility threshold). We consider 10 equal-sized bins on both sides of the cutoff. Both the dependent variable and the running variable are residualized on controls before plotting. The controls considered are value of the outcome variable at visit n, female, age at visit $n+1$ (and its square), year of visit $n+1$ FE, month of visit $n+1$ FE, and Montevideo FE.

Figure 9: Impact of UCT on durable goods consumption



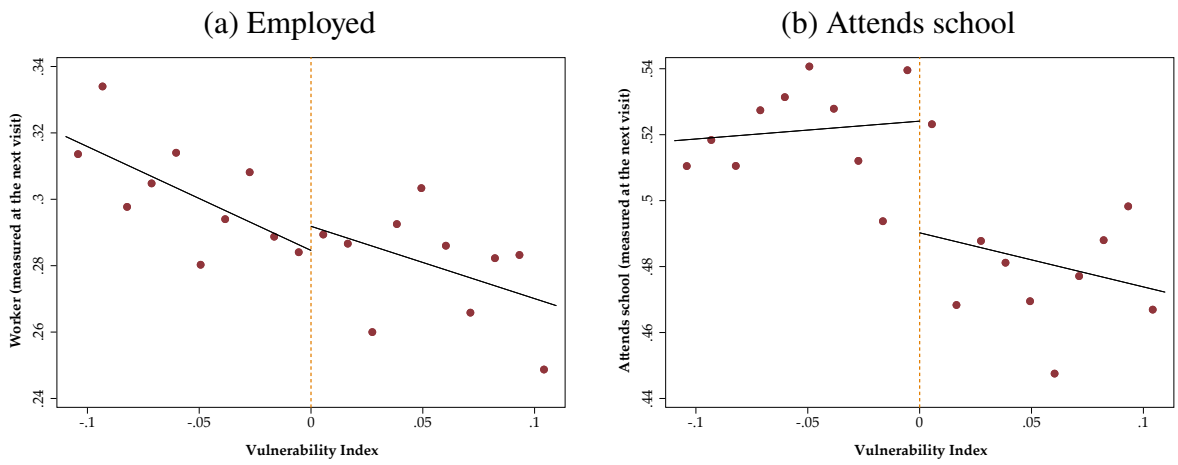
Notes: This figure shows fuzzy RD estimates of the impact of UCT receipt on the availability of durable goods in the household at the next visit ($n+1$), and associated 90% and 95% confidence intervals. The Vulnerability Index score computed on the basis of visit n is used to instrument for beneficiary status at visit $n+1$ (see "Measuring impacts on outcomes measured at a re-visit" in Section 4).

Figure 10: Impact of UCT on housing conditions



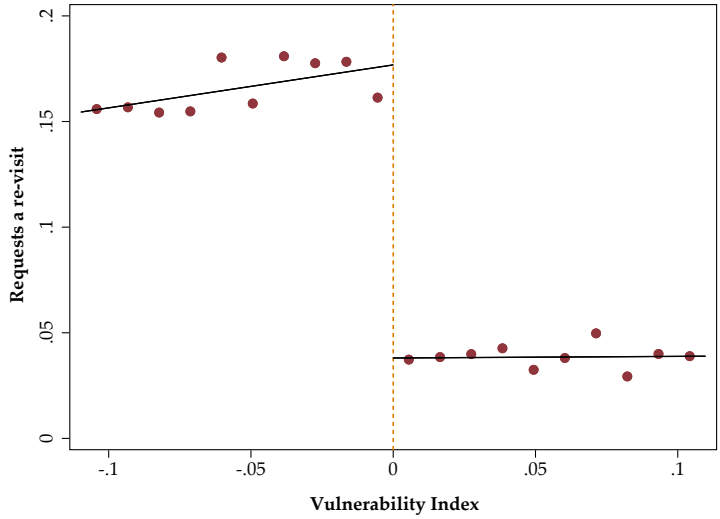
Notes: This figure shows fuzzy RD estimates of the impact of UCT receipt on housing conditions at the next visit ($n+1$), and associated 90% and 95% confidence intervals. The Vulnerability Index score computed on the basis of visit n is used to instrument for beneficiary status at visit $n+1$. *Dwelling in good condition* is a dummy that takes value 1 if the home is in good condition or needs minor repairs. *Durable floor* codes 1 for permanently covered floors and 0 otherwise. *Durable roof* is a dummy variable that takes value 1 if the roof is made primarily of concrete or other durable materials. *Overcrowding* reports whether there are children that sleep in the same bed as adults. *Rooms to sleep* corresponds to the number of rooms used for sleeping. *Exposed electrical connection* records whether there are electrical connections and wiring not embedded in the walls. *Toxic waters* is a dummy that measures the presence of wastewater or accumulation of contaminated water. *Garbage* is a dummy that measures whether there is an accumulation of waste or dumps in the block where the home is located.

Figure 11: Impact of UCT on self-reported employment status and school attendance



Notes: In panel *a* the variable plotted is the percentage of individuals within a VI bin that reported being employed at the next visit ($n+1$). The horizontal axis shows the Vulnerability Index score computed on the basis of visit n (normalized to 0 at the UCT eligibility threshold). We consider 10 equal-sized bins on both sides of the cutoff. The sample corresponds to all individuals that were in the 18-38 age group three years after visit n. Both the dependent variable and the running variable are residualized on controls before plotting. The controls considered are value of the outcome variable at visit n, female, age at visit $n+1$ (and its square), year of visit $n+1$ FE, month of visit $n+1$ FE, and Montevideo FE. The same applies to panel *b*, with two exceptions. First, the outcome variable is a dummy equal to 1 if an individual reports attending school. Second, the sample corresponds to minors that finished primary school (or are in the last year of primary school) and need at least three more years to finish secondary school.

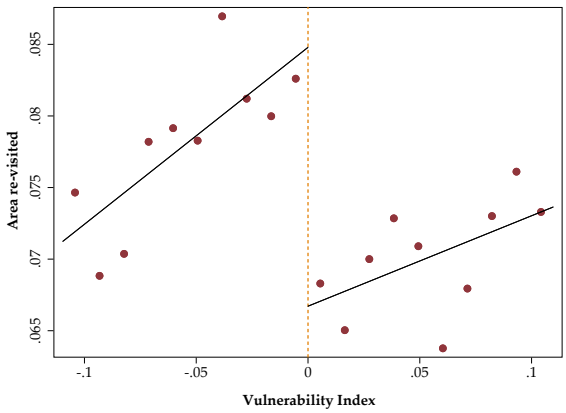
Figure 12: Impact of UCT on re-visit requests



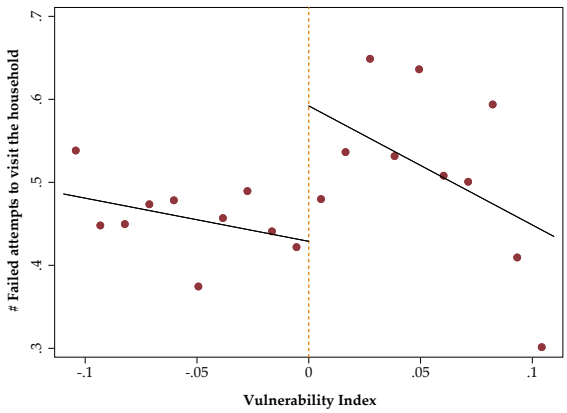
Notes: The variable plotted on the vertical axis is the percentage of individuals within a VI bin that live in households where a member requested a re-visit within a year after a visit. The horizontal axis shows the Vulnerability Index score computed on the basis of the visit (normalized to 0 at the UCT eligibility threshold). We consider 10 equal-sized bins on both sides of the cutoff. The sample corresponds to all individuals visited between January 2015 and December 2017. Both the dependent variable and the running variable are residualized on controls before plotting. The controls considered are female, year FE, month FE, and Montevideo FE.

Figure 13: Impact of UCT on area re-visit probability and visit attempts

(a) Area re-visit probability

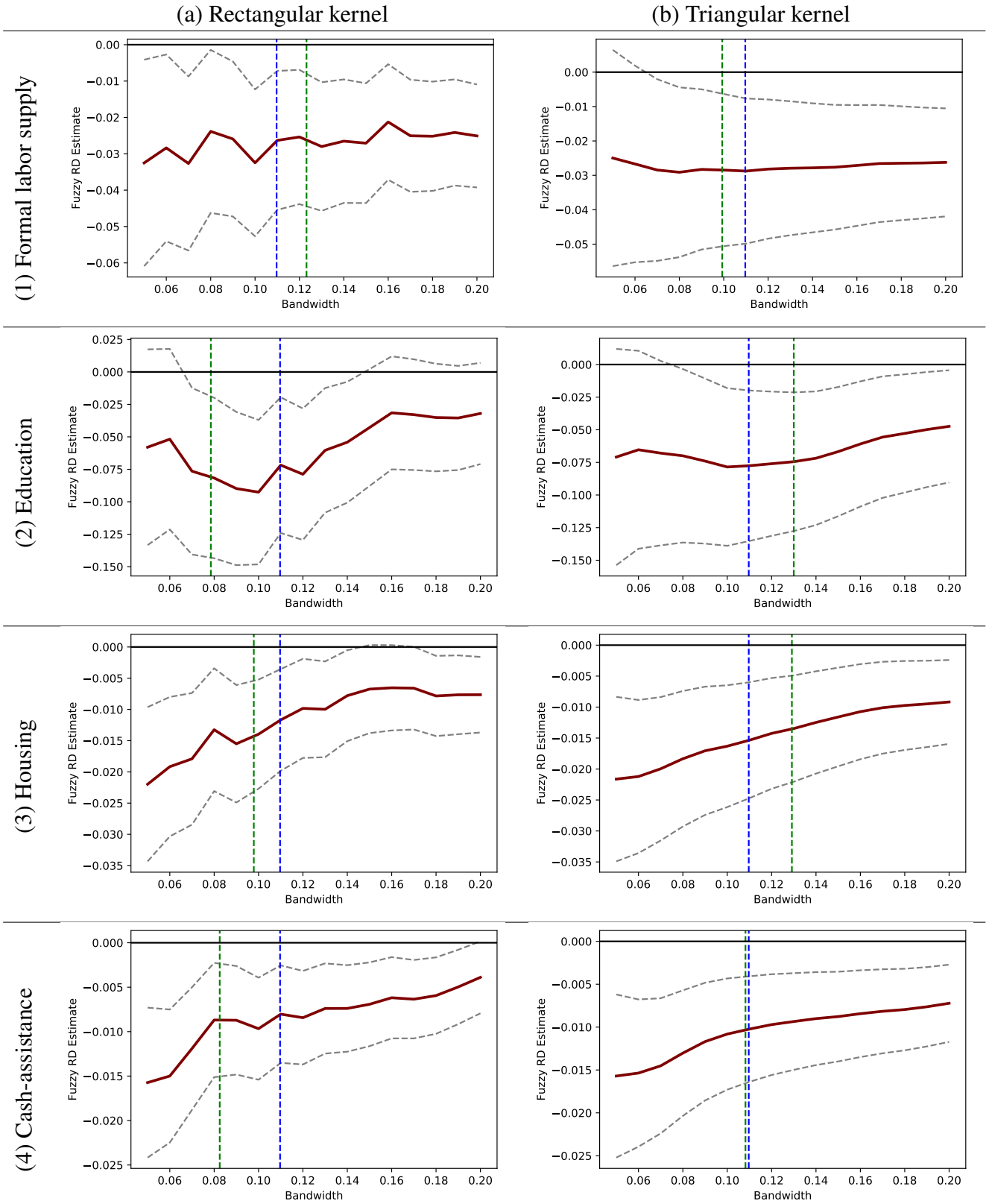


(b) Visit attempts



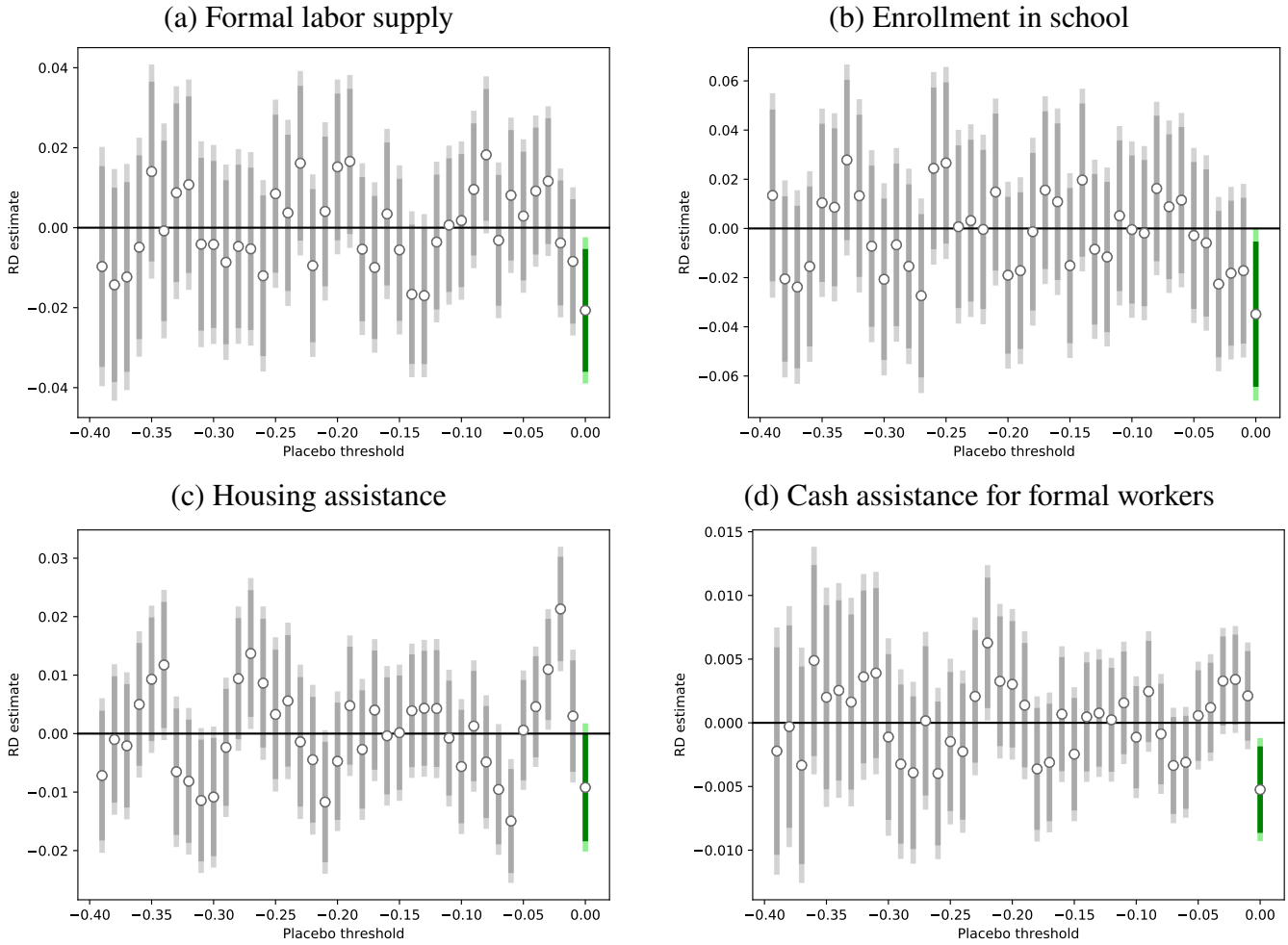
Notes: In panel *a* the variable plotted is the percentage of individuals within a VI bin that are revisited in an area visit after a first visit. The horizontal axis shows the Vulnerability Index score computed on the basis of the (first) visit (normalized to 0 at the UCT eligibility threshold). We consider 10 equal-sized bins on both sides of the cutoff. Both the dependent variable and the running variable are residualized on controls before plotting. The controls considered are female, year FE, month FE, and Montevideo FE. The same applies to panel *b*, with these exceptions: outcome variable is the mean number of visit attempts by VI bin before the targeted visit was successfully completed; the horizontal axis shows the Vulnerability Index score computed on the basis of the previous visit (normalized to 0 at the UCT eligibility threshold); additional control considered is visit attempts for the previous visit.

Figure 14: Robustness to bandwidth and kernel specification supply



Notes: This figure shows, for different bandwidth values, fuzzy RD estimates ($\hat{\beta}_1$ from equation (2)) and 90% confidence intervals. The green vertical line represents the CCT optimal bandwidth (Calonico et al. 2014), and the blue line represents the bandwidth chosen in our baseline regressions. Panel *a* uses a rectangular kernel, and panel *b* uses a triangular kernel.

Figure 15: Falsification tests using placebo thresholds



Notes: This figure shows regression coefficients and associated 90% and 95% confidence intervals for the reduced-form impact of crossing placebo thresholds on a given outcome (i.e., the β_1 from equation (4)). We consider all placebo thresholds that do not confound effects of other policy changes (i.e., CCT and double UCT) and that are 0.01 VI score points away from each other. In panel *a* the outcome variable is a dummy equal to 1 if the individual is formally employed three years after the visit. In panel *b* the outcome variable is a dummy equal to 1 if the individual is enrolled in school three years after the visit. In panel *c* the outcome variable is a dummy equal to 1 if the individual is enrolled in a housing assistance program offered by the Ministry of Housing. In panel *d* the outcome variable is a dummy variable equal to 1 if the individual receives a cash assistance transfer for formal workers.

Tables

Table 1: Characteristics of visited households and individuals (mean values)

Demographics	
Household size	4.0
Number of minors in the household	2.0
Female	55.6%
Years of education (adults)	7.0
Pregnant (for women, ages 18-40)	4.9%
In Montevideo	34.9%
Safety net programs	
Receives no UCT	69.4%
Receives UCT (not double)	25.0%
Receives double UCT	5.5%
UCT (not double) amount (% household income, all sources)	17.4%
Double UCT amount (% household income, all sources)	30.3%
Beneficiary of cash-assistance for formal workers	2.0%
Participates in a housing program	5.3%
Receives CCT	69.3%
Income and material well-being	
Household income (in USD; all sources)	440.2
Household income (in USD; excludes transfers)	346.5
Reports food insecurity	45.4%
A household member went to a soup kitchen (last month)	8.0%
Computer	17.4%
Heater	45.5%
Status in the job market (14 years old and older)	
Private sector employee	23.6%
Public sector employee	2.9%
Worker at a cooperative	0.1%
Self-employed	26.6%
Employer (*Patrón*)	0.1%
Unpaid worker	0.4%
Unemployed	14.5%
Retired (*Jubilado*)	5.2%
Receives a pension (*Pensionista*)	5.6%
In charge of household chores	16.0%
Other inactive	5.0%

Notes: Source of the data on take-up of safety net programs is MIDES (for UCT), the Social Security Bank (for CCT), and SIIAS (for housing and cash assistance for formal workers). Household income from all sources is constructed using households' responses to the survey conducted by MIDES agents and administrative data on UCT and CCT amounts transfered: self reported income from all work sources, plus self-reported income on pensions, plus CCT and UCT amounts. Household income that excludes transfers only considers the self reported income from all work sources. For the rest of the variables, the source is the survey conducted by MIDES agents.

Table 2: Balance on observables

	Uruguay	Montevideo	Non-Montevideo
Demographics			
Female	0.002	0.009	-0.001
	(0.003)	(0.006)	(0.004)
Age	0.117	0.434*	-0.011
	(0.126)	(0.255)	(0.144)
Year	0.012	0.037	0.004
	(0.027)	(0.051)	(0.032)
Household size	0.016	0.003	0.022
	(0.028)	(0.055)	(0.032)
Montevideo	0.003	-	-
	(0.007)	-	-
Pregnant (ages 18-40)	0.003	-0.003	0.005
	(0.004)	(0.006)	(0.005)
Food insecurity			
Food insecurity	-0.002	-0.005	-0.0
	(0.008)	(0.015)	(0.01)
Food insecurity, minors	-0.013	0.011	-0.022*
	(0.011)	(0.021)	(0.013)
Employment, Income and Education			
Formal worker (t-1)	0.003	0.008	0.0
	(0.005)	(0.012)	(0.006)
Household income (USD)	9.97	3.625	12.041
	(7.124)	(14.97)	(7.929)
In school (t-1)	-0.014	0.003	-0.02
	(0.016)	(0.033)	(0.019)
Yrs of education (t-1)	-0.027	0.055	-0.06*
	(0.028)	(0.053)	(0.034)
Welfare and Political Participation			
CCT benficiary (t-1)	-0.003	0.001	-0.004
	(0.005)	(0.012)	(0.006)
Housing program (t-1)	-0.0	-0.01	0.004
	(0.004)	(0.009)	(0.005)
Cash-assistance for formal workers (t-1)	0.0	-0.001	0.0
	(0.001)	(0.003)	(0.002)
Voted in Participatory Budgeting	-	-0.004	-
	-	(0.008)	-
Houehold requested visit	-0.004	0.006	-0.006
	(0.012)	(0.025)	(0.014)

Notes: Table shows RD estimates considering an asymmetric linear control function, and a bandwidth of 0.1097 around the threshold that defines UCT eligibility. Standard errors clustered at the household level are shown in brackets. * p<0.10, ** p<0.05, *** p<0.01. Except when stated otherwise, all variables are measured at the time of the visit (t).

Table 3: Impact of UCT on formal labor supply: fuzzy RD estimates

	Perc. formally employed 36 mths after		
	Pooled	Non-benef.	Benef.
	(1)	(2)	(3)
UCT (1-36 mths after)	-0.027**	-0.044***	-0.002
	(0.012)	(0.016)	(0.017)
Observations	65885	38517	27368
Mean non - recipients	0.304	0.306	0.247
Bandwidth	0.1097	0.1097	0.1097
First stage estimate	0.60	0.56	0.67
F-Stat (First Stage)	8477.0	4551.0	4739.0
P-val: (2) = (3)		0.092	

Notes: Standard errors clustered at the household-visit level. * p<0.10, ** p<0.05, *** p<0.01. The dependent variable is a dummy indicating whether the individual is formally employed, 36 months after the visit. Endogeneous regressor is the share of months (within 1-36 months after the visit) in which someone in the household received an UCT from MIDES. Asymmetric and linear RD polynomial considered. Control variables considered are: year FE, month FE, and dummy for Montevideo. Sample consists of individuals that were in the 18-38 age group three years after the visit and that were CCT beneficiaries at the time of the visit. First stage estimate corresponds to the coefficient of the impact of crossing the UCT threshold on the endogeneous regressor (i.e., α_1 in equation 1). p-value of the t-test between the difference in the estimates of column 2 vs 3 are presented in the table.

Table 4: Impact of UCT on youth enrollment in public schools: fuzzy RD estimates

	In public school 36 mths after		
	Pooled	Non-benef.	Benef.
	(1)	(2)	(3)
UCT (1-36 mths after)	-0.033*	-0.075**	0.005
	(0.02)	(0.032)	(0.025)
Observations	24132	10721	13411
Mean non - recipients	0.522	0.523	0.502
Bandwidth	0.1097	0.1097	0.1097
First stage estimate	0.64	0.56	0.72
F-Stat (First Stage)	3351.0	1245.0	2661.0
P-val: (2) = (3)		0.295	

Notes: Standard errors clustered at the household-visit level. * p<0.10, ** p<0.05, *** p<0.01. The dependent variable is a dummy indicating whether the individual is enrolled in any of the educational subsystems of ANEP, 36 months after the visit. Endogeneous regressor is the share of months (within 1-36 months after the visit) in which someone in the household received an UCT from MIDES. Asymmetric and linear RD polynomial considered. Control variables considered are: year FE, month FE, and dummy for Montevideo. Sample consists of minors that either finished primary school or are studying in its last year and still need at least three more years of study to finish secondary school. First stage estimate corresponds to the coefficient of the impact of crossing the UCT threshold on the endogeneous regressor (i.e., α_1 in equation 1). p-value of the t-test between the difference in the estimates of column 2 vs 3 are presented in the table.

Table 5: Impact of UCT on take-up of housing assistance: fuzzy RD estimates

	In a housing program, 3yrs after		
	Pooled	Non-benef.	Benef.
	(1)	(2)	(3)
UCT (1-36 mths after)	-0.012**	-0.01	-0.014*
	(0.005)	(0.007)	(0.007)
Observations	74882	38367	36515
Mean non - recipients	0.042	0.041	0.064
Bandwidth	0.1097	0.1097	0.1097
First stage estimate	0.60	0.55	0.67
F-Stat (First Stage)	9013.0	4147.0	6225.0
P-val: (2) = (3)		0.538	

Notes: Standard errors clustered at the household-visit level. * p<0.10, ** p<0.05, *** p<0.01. The dependent variable is a dummy variable indicating enrollment in a housing assistance program three years after the visit. Endogeneous regressor is the share of months (within 1-36 months after the visit) in which someone in the household received an UCT from MIDES. Asymmetric and linear RD polynomial considered. Control variables considered are: year FE, month FE, and dummy for Montevideo. Sample consists of home owners. First stage estimate corresponds to the coefficient of the impact of crossing the UCT threshold on the endogeneous regressor (i.e., α_1 in equation 1). p-value of the t-test between the difference in the estimates of column 2 vs 3 are presented in the table.

Table 6: Impact of UCT on take-up of cash-assistance for formal workers: fuzzy RD estimates

	Beneficiary, 3yrs after		
	Pooled	Non-benef.	Benef.
	(1)	(2)	(3)
UCT (1-36 mths after)	-0.008**	-0.011**	-0.003
	(0.003)	(0.005)	(0.004)
Observations	81454	49056	32398
Mean non - recipients	0.027	0.027	0.03
Bandwidth	0.1097	0.1097	0.1097
First stage estimate	0.60	0.56	0.68
F-Stat (First Stage)	10189.0	5671.0	5719.0
P-val: (2) = (3)		0.252	

Notes: Standard errors clustered at the household-visit level. * p<0.10, ** p<0.05, *** p<0.01. The dependent variable is a dummy variable indicating enrollment in the cash assistance program for formal workers three years after the visit. Endogeneous regressor is the share of months (within 1-36 months after the visit) in which someone in the household received an UCT from MIDES. Asymmetric and linear RD polynomial considered. Control variables considered are: year FE, month FE, and dummy for Montevideo. Sample consists of adults living with minors in the household. First stage estimate corresponds to the coefficient of the impact of crossing the UCT threshold on the endogeneous regressor (i.e., α_1 in equation 1). p-value of the t-test between the difference in the estimates of column 2 vs 3 are presented in the table.