Strategies to increase the take-up of social benefits.

Evidence from a field experiment in a deeply vulnerable population[*].

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Abstract

This is the first paper to identify, using a field experiment, the effects of intense one-on-one assistance by a professional social worker on the take-up of social benefits within a population of deeply disadvantaged informal workers. A municipal program exists that entails providing these disadvantaged informal workers with a formal permit to work on the streets and make contributions to the retirement pension system. We randomly assign one-on-one assistance to these informal workers, and within this treatment group, we randomly assign money to cover the cost of the documents required by the municipality. We find that a worker who receives one-on-one assistance is three times more likely to receive the municipal permit than a worker in the control group. We also find that a worker who receives both one-on-one assistance and cost coverage is four times more likely to obtain the municipal permit. Providing information alone does not have an impact. The program has no spillover effect on the take-up of other national support programs that are not targeted by the one-on-one assistance intervention. These findings identify possible strategies to remove barriers to increase the take-up of social benefits within deeply vulnerable populations.

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I. Introduction

A substantial number of disadvantaged workers who are eligible for welfare benefits do not always use them, leading to potentially detrimental consequences (Aizer 2003; Currie 2004). One example is those individuals who work informally on the street. This phenomenon is spread out in many countries, regardless of whether they are developed (Boels 2014), developing (Cabrera and Cid 2017) or underdeveloped (Bhowmik 2012). In some cities, street vendors are very common, selling anything from snacks or beverages to flowers, books and paintings. There are many other examples of street markets where goods are sold in informally assigned areas, e.g., the squeegee men wiping the windshields of cars stopped at traffic lights; street hawkers selling bags, sunglasses or handicrafts; and rag-and-bone men collecting unwanted household items. In our field experiment, we work in one of these settings: the market of *cuidacoches*. They are socially excluded workers who unsolicitedly look after parked cars, hoping to receive a voluntary tip from drivers in Montevideo (Uruguay). This is a common practice in many Latin American countries. They are known as *viene-viene* or *franeleros* in Mexico, *cuida autos* or *guardias* in Chile, *franelinhas* in Brazil, *celadores*, *vigilantes* or *guachimanes* in Colombia, *cuidacarros* in Peru, and *trapitos* in Argentina.

Montevideo is an interesting setting to study the use of welfare programs for these disadvantaged workers. Due to the most severe economic crisis in its history, Montevideo experienced substantial growth in the number of *cuidacoches* in 2002, a phenomenon still present fifteen years later. The municipality has been offering a social program to these street workers. The social benefits consist of a permit that guarantees both the right to work, looking after cars in an assigned segment of a street, and the right to make contributions to receive a retirement pension in the future. To grant a permit, the municipality demands basic documentation (i.e., identity card, health card, and crime record report), which is also required to access any other welfare program. The uptake of these programs may be low due to bureaucratic and behavioral barriers.

To our knowledge, we are the first to identify a successful intervention focused on helping disadvantaged individuals that work in street markets, where goods are sold in informally assigned areas. We design a field experiment in the market of *cuidacoches* to answer two straightforward questions. Are deeply disadvantaged individuals more likely to take-up the municipal social program when they receive one-on-one assistance from a professional social worker (an intense and involved intervention)? Is this likelihood affected by adding cost coverage of the legal requirements to this one-on-one assistance? Our randomized control trial includes two treatments: one-on-one assistance and one-on-one assistance plus cost coverage to pay for the required documentation to apply for its social program. We find that one-on-one assistance, both as an isolated treatment and combined with cost coverage, is effective in lifting the barriers that make it difficult to receive this permit. A worker who receives one-on-one assistance is three times more likely to receive the

municipal permit (those who receive both one-on-one assistance and cost coverage of the documentation are four times more likely to receive the municipal permit).

The previous literature offers several hypotheses as to why people may not take-up a benefit for which they are eligible. Take-up may be framed as a tradeoff between costs and benefits. Consistent with 'utility-maximizing' rational agent behavior (Nichols and Zeckhauser 1982), individuals will apply if the benefits exceed the costs. These costs can be broadly categorized as (a) *information costs* (Bhargava and Manoli 2015); (b) *administration costs*, for example, the complexity of the application process (Bettinger et al. 2012); and (c) *social costs*, such as stigmatization (Currie 2004). However, recent studies have challenged that individuals are able to sensibly compare the expected costs and benefits due to cognitive, motivational, or emotional limits to decision-making. In this vein, the failure to take-up a government support program may be a consequence of procrastination, inattention and aversion to the program complexity ("psychological frictions" according to Bhargava and Manoli 2015). Thus, providing intensive and personalized help to remove those barriers can have significant effects on program participation.

Our study has implications for a wide range of welfare programs that require overcoming bureaucratic and behavioral barriers within deeply vulnerable populations. We contribute to a strand of the literature that studies the impact of offering one-on-one assistance to vulnerable individuals for taking-up the benefits of government support programs. In the area of education, Bettinger et al. (2012) run a field experiment offering personal help to low-income students to complete the Free Application for Federal Student Aid (FAFSA). In the labor market literature, Gautier et al. (2018) study the equilibrium effects of a job search assistance program where unemployed workers received personalized assistance and monitoring in order to get a job in Denmark. There are several studies that look at how some combination of individual assistance and making formalization as costless as possible work in getting small informal firms to get municipal permits or tax licenses. Campos, Goldstein and McKenzie (2018) randomly offer personal assistance for business regularization to small firms in Malawi. Bruhn and McKenzie (2014) is a survey of this literature, giving examples such as Jaramillo (2009) and Alcázar, Andrade and Jaramillo (2010) in Peru, de Mel, McKenzie and Woodruff (2013) in Sri Lanka, and Andrade, Bruhn and McKenzie (2013) in Brazil. A more recent example is Benhassine et al. (2018) in Benin. Several of these studies also suggest that information alone does not get firms to formalize, but that offering individual assistance and help paying the time and monetary costs of formalizing can increase take-up.

Our intervention adds another setting to this strand of the literature, namely, the *cuidacoches* labor market in a Latin American capital city. Second, our experiment is designed to disentangle the pure impact of one-on-one assistance relative to that of other treatments. A third contribution to the existing literature is that we test a more intensive and involved intervention: a *boots-on-the-ground* approach. This intensity and involvement may be appropriate and necessary when working with deeply disadvantaged populations. The

fourth contribution is the description of a new and unique database containing the sociodemographic characteristics of this understudied population of deeply vulnerable street workers.

The rest of this paper is organized as follows. Section II lays out the context of our study with basic background information on the market of *cuidacoches*. Section III describes the details of the experimental design. Section IV presents the main results. In Section V, we discuss the main results and add an analysis of the impact of the information and spillover effects of our experiment on the take-up of other social programs. Section VI concludes the paper.

II. Research setting: *Cuidacoches* labor market

Consumers often provide tips to workers as a way of payment for some services (Natter and Kaufmann 2015). Among those workers commonly tipped, vulnerable workers offer a service associated with an informal right of usufruct over a place. This is the case for informal car washers, street performers, golf caddies and car windshield cleaners at traffic lights.

In the case of vehicles, we find those who unsolicitedly work on the street as parking valets and look after parked cars, expecting a tip in return. Montevideo, the capital city of Uruguay, provides an ideal opportunity to study highly deprived valets in a voluntary payment market. This city has nearly 1,400,000 inhabitants (Uruguayan National Institute of Statistics, Census, 2011) and 540,000 cars (Municipality of Montevideo, Department of Transport, 2015).

The *cuidacoches* market experienced sudden growth in 2002, when the country suffered a severe economic crisis that left a large part of the population below the poverty line. Most of the workers absorbed by the *cuidacoches* market are unskilled, given the precarious conditions that the job entails. This type of informal job has consolidated over the last fifteen years in a setting of sustained growth in number of cars purchased.

Cuidacoches are self-employed and are not constrained to a fixed schedule. They stand in a visible spot in the street, wearing a reflective jacket, so people can identify them and look after the parked cars. Usually, they also assist people in finding a parking space and parking their car. In some cases, there can be more than one *cuidacoches* in the same block, in which case they settle the issue of how to distribute the work themselves.

In 2014, the number of *cuidacoches* in Montevideo—both registered and unregistered—was approximately 3,000 (Cabrera and Cid 2017). Despite the municipality's aim to regulate this voluntary payment market, nearly half of them are unregistered.

Within this deprived population of *cuidacoches*, those who have no permit show even less income, less savings, a greater homelessness rate, a lower rate of health coverage, and worse indicators of external appearance and violent behavior (Table 1). Precisely, in this study, we focus on those *cuidacoches* that have no permit to work on the street.

Municipality support program for *cuidacoches*

Several attempts have been made to ban, regulate and legislate the market of *cuidacoches* in different parts of the world. The municipality of Montevideo—Intendencia de Montevideo (IM)—has a long tradition of issuing regulations for the *cuidacoches* market. Some of these policies date back to 1933 (these regulations can be found in the Digesto Municipal [Intendencia de Montevideo 2018]). Currently, the municipality support program consists of handing out permits that allow *cuidacoches* to work in exclusivity on a certain block and make contributions to their retirement pension. To register themselves, the *cuidacoches* must possess a health certificate, national identity card, and criminal record report (indicating whether they have a criminal record or not). Registered *cuidacoches* receive the property right to a specific area, which means that the municipality will provide protection if another *cuidacoches* wants to work in the same place. Once they receive the municipal permit, it is mandatory to go to the municipality office to sign in on a form once per month (a practical way to foster a closer relationship with the municipal government). The permit allows the *cuidacoches* to work in a certain street block with no expiration date. Nonetheless, this monthly checking-in is useful to guarantee that the health card, the identity card and the crime report are up to date. The municipality can revoke the permit in the case of misbehavior, complaints from drivers, etc. Table 2 shows that in 2014—the year prior to our intervention—the municipality had issued 181 permits, and at the end of 2014, 100 of them had expired.

Many people remain puzzled as to why only half of the *cuidacoches* have a permit, although the monetary costs of the requirements for the permit are low (equivalent to one or two working days as *cuidacoches*), and the benefits of this permit seem to be noteworthy (Table 3 shows the benefits reported by the *cuidacoches* themselves). As discussed in the introduction, *cuidacoches* will apply for the municipal permit if the benefits exceed the costs. But *cuidacoches* may overemphasize the present costs of applying and face extreme difficulty in thinking about the long-run consequences from immediate actions. There is evidence that the tradeoff between immediate outcomes and distant outcomes involves hyperbolic discounting (McClure et al. 2004; Kable and Glimcher 2007), or even, instead of thinking about the long run, individuals rely on rules of thumb or past experiences (Stanovich, West, and Toplak 2012). Another possible explanation for the low rate of take-up of the municipal program is *cuidacoches*' concern about identity, which dominates their general behavior. This means that they may care about the extent to which their behavior deviates from

that of their social group (Fryer et al. 2012). In addition, their little experience in administrative procedures and in dealing with state bureaucracy may be a significant barrier to comply with municipal regulations.

Given these hypotheses (then supported by the pieces of evidence provided in Table 5), one possible strategy to foster *cuidacoches*' take-up of the municipality program is one-on-one assistance. The previous literature in the education field suggests that intense personal help may be beneficial (Carrell and Sacerdote 2017, Bettinger et al. 2012). Lavecchia, Liu and Oreopoulos (2014) develop a general framework for thinking about behavioral barriers: they suggest that one way to cope with these barriers is close personal assistance. By offering help to "get it done now", *cuidacoches* may change the way they make their decisions, reducing their procrastination. Intense personal assistance provides a social component to nudge attempts in the required administrative process and can be tailored to individual circumstances.

III. Methodology

A. Data

We base our analysis on five databases:

(1) During 2013 and 2014, we built two cross-sectional databases with information on 724 *cuidacoches*. We use these data for selecting the blocks of the city in which the majority of *cuidacoches* are concentrated.

(2) A baseline survey at the beginning of the current field experiment (March-May 2015). We use these data to check the pretreatment balance.

(3) Administrative data provided by the municipality, containing the registration information of all *cuidacoches* in Montevideo that obtained a municipal permit from 2002 to 2015. These administrative data from the municipality are key to our analysis because they allow us to build the outcome variable "municipal permit achievement in December 2015". Knowing the actual take-up of the program for each *cuidacoches* prevents misreporting (what the *cuidacoches* declare may not coincide with reality), and it also prevents missing values (*cuidacoches* who were untraceable by our team in the streets at the follow-up survey).

(4) Administrative data about the *cuidacoches*´ take-up of other public benefits, provided by the Ministry of Social Development (MIDES). We use this data to analyze possible spillovers to other social programs.

(5) A brief follow-up survey at the end of the current field experiment (November 2015-April 2016). The sample experienced a high attrition rate, as it is common in this type of settings. *Cuidacoches* in general, and *cuidacoches* without a working permit in particular, are a deeply vulnerable population (Cabrera and Cid, 2017). Many of them are homeless, only very few have mobile phones and change their number or sell it frequently. It was extremely difficult to follow this population: we extended the follow-up survey for six

months (November 2015 to April 2016), devoting extra resources to find the missing *cuidacoches*, but we were not able to achieve a significant improvement in the response rate. Thus, we decided to gather administrative data for our main outcome, where there is no attrition (data source #3). Nonetheless, we include some questions from the follow-up survey in the descriptive tables 3, 4, 7 and 9.
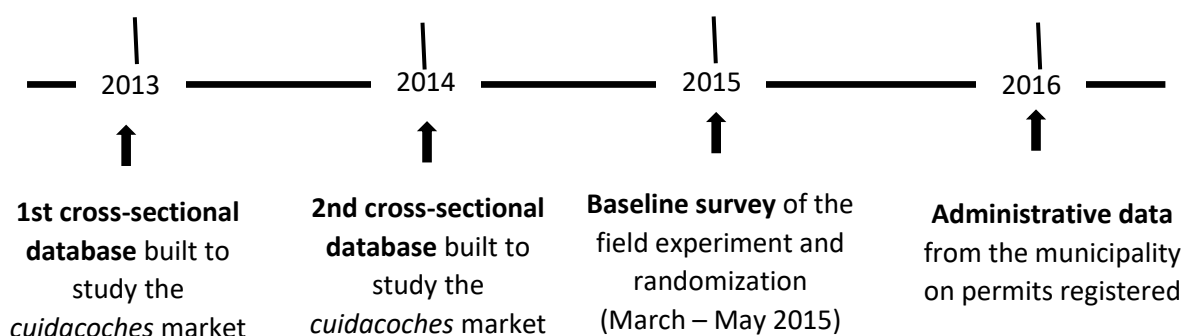
B. Summary statistics

In Table 4, we present a set of descriptive statistics from the data collected via a survey administered to *cuidacoches* who did not hold a municipal permit at the baseline of the experiment (data source #2). Ninety-one percent of them are male. The average *cuidacoches* is 43 years old and has been working on the same block for 5.73 years. They work 9.6 hours per day. Although we work with *cuidacoches* who do not have a valid permit, 27 percent of them had previously received a municipal permit, which had expired by the time of the survey. The baseline survey also includes questions regarding the external appearance of the *cuidacoches* (according to the subjective assessment of the interviewer). Table 4 shows that this index takes the value of only 0.76 (out of 4), but it hides that only one out of four *cuidacoches* seems tidy, seven percent seem influenced by drugs or alcohol, on average they receive regular dental care, and the average quality of language employed by a *cuidacoches* is between poor and normal.

C. Experimental design

We have been studying the *cuidacoche*s market since 2013 (Cabrera and Cid 2017; Blanco, Cabrera, and Cid 2016). These previous descriptive studies have helped us to explore hypotheses and mechanisms to design the present field experiment.

Timeline:

| 2013 | 2014 | 2015 | 2016 |

1st cross-sectional database built to study the *cuidacoches* market

2nd cross-sectional database built to study the *cuidacoches* market

Baseline survey of the field experiment and randomization (March – May 2015)

Administrative data from the municipality on permits registered

The database built from the surveys applied in 2013 and 2014 (data source #1) helped us identify the blocks of Montevideo with a greater chance of finding *cuidacoches* who did not have a permit. Each cell (group of street blocks) was randomly assigned to one of the following three groups (Treatment 1, Treatment 2, or Control).

Treatment 1 (T1). Each *cuidacoches* in the *one-on-one assistance treatment* group received the following in March-May 2015:

1. a two-page brochure (see Figure A.1 in the Appendix section) with the basic information to take-up the municipality support program and
2. the assistance of a social worker to personally help him or her comply with the municipality's requirements to receive the permit.

Treatment 2 (T2). The second treatment arm consists of *one-on-one assistance plus cost coverage treatment*. These *cuidacoches* received the following in March-May 2015:

1. the abovementioned two-page brochure,
2. the assistance of a social worker to help him or her in the procedures to achieve the documentation demanded by the municipality to receive the permit, and
3. coverage of the costs of achieving the required documentation.

This treatment arm includes the coverage of costs in the treatment because we identified in the baseline of the experiment that low monetary resources seem to be a barrier to obtaining the municipal permit. Table 5 shows that 31 percent of male *cuidacoches* and 33 percent of female *cuidacoches* report that they do not have enough money to pay for the documents required by the municipal program. Thus, this second treatment also shows a real potential impact.

The cost coverage is not a future reimbursement: the social worker takes the *cuidacoches* to different clinical and public offices to obtain a national identity card, document of criminal records, and health card, and in each step, the social worker pays for him or her at the clinic or office.

Control (C). *Cuidacoches* in the *control group* did not receive one-on-one assistance or cost coverage of their expenses. They only received the two-page brochure with the basic information to take-up the municipality support program. We chose to treat with information the control group because our prior work (Cabrera and Cid 2017) showed that the lack of information was reported as a major reason for not having a work permit. Remarkably, this prior knowledge was confirmed in the baseline survey at the beginning of this intervention (Table 5: "I am not well informed about the procedures for obtaining the work permit"). Guided by the Ethics Committee, since the cost of the brochure was negligible in the budget of the experiment and we could help hundreds of disadvantaged individuals, we decided to deliver the information to all participants. Thus, *information* alone is not a treatment in our experimental setting. Nonetheless, in section V.c, we will measure the impact of information using a nonexperimental approach (a synthetic control method).

The comparison of the *one-on-one assistance treatment* (T1) and the *control group* (C) allows us to measure the impact of providing personal assistance on the take-up of the municipal program. The comparison of the two treatments allows us to explore the role of financial restrictions for obtaining the municipal permit (the monetary costs to fulfill the requirements are equivalent to two work days as *cuidacoches*).

D. Randomization

Randomization was performed at the cell level (each cell is a group of street blocks). The reason for implementing the randomization at the cell level, rather than at the individual level, is to reduce contamination. We did not want to have two adjacent *cuidacoches* in different groups (i.e., one assigned to the control group and the other to one of the treatments), thus introducing contamination and possible biases in the experiment. Moreover, we exclude a buffer of one block on all sides of the grid cell. The *cuidacoches* working in buffer areas were not invited to join the program. Figure 1 shows a global view of the city with the 88 cells selected for the experiment. Figure 2 is a closeup of the downtown city area, where we can more clearly see the buffer areas between treated areas. Even if we had not included these precautions, we think that the probability of spillover would be very low. Indeed, we have data from a survey of *cuidacoches* (data source #1), collected eighteen months before the current experiment. These data show that the average *cuidacoches* reported very low levels of connection with nearby *cuidacoches*.

To implement the design, we exploit two previous surveys (data source #1) that we conducted in our prior research (Cabrera and Cid 2017; Blanco, Cabrera and Cid 2016). In these surveys, we collected the distribution of *cuidacoches* across Montevideo. Now, for this field experiment, we impose a grid to divide the city into similar areas in terms of the number of *cuidacoches*. We balance on a vector of three variables at the cell level: a) the number of *cuidacoches* in each cell (obtained from data source #1); b) the size of the area in the cell, which is associated with the number of street blocks; and c) the number of cars in the cell (Uruguayan National Institute of Statistics, 2014). Randomization is implemented via stratification in this vector of variables. We create groups of four cells that are similar in those strata and then randomly assign two of them to control, one to T1 (*one-on-one assistance treatment*) and one to T2 (*one-on-one assistance plus cost coverage treatment)*. Cells that are not assigned in the first round of the procedure are balanced using the number of *cuidacoches* and the size of the area in the cell. Of the 88 cells included in the randomization, 42 are assigned to *C,* 23 to *T1*, and 23 to *T2*. The social workers were able to interview 339 *cuidacoches,* in those cells, who did not have the municipal permit (the *cuidacoches* who already had the working permit are neither interviewed nor included in the experiment).

Table 6 presents the mean and standard deviations to check the balance condition for the variables used in the randomization procedure and other cell level variables obtained from household surveys.

Interestingly, even though randomization was performed at the cell level, the balance condition was also achieved at the individual level (Table 7). The pairwise differences illustrate that both treatments are well balanced with respect to control and to one another at the cell and individual levels.

E. Intervention

The social workers were hired and trained by members of the research team. They received a package with printed materials for the intervention and an identification card from the university. The package contained the manual of procedures, copies of the information brochure, copies of the survey, and a map. To avoid mistakes, each map identified the cells of the control and treatment groups only for the specific part of the city where that social worker would administer the survey (see Figure 1). Each social worker went over all of the blocks in their corresponding cells, and every time they found *cuidacoches* without a permit, they carried out the survey (each survey took approximately 30 minutes). If the *cuidacoches* belonged to treatment 1 (T1) or treatment 2 (T2) cells, the social worker encouraged the *cuidacoches* to obtain the documents required by the municipality support program and tried to schedule a date to personally help him or her through the process. The social worker took the *cuidacoches* through the entire process that ends in the offices of the municipality, where the *cuidacoches* finally registers himself or herself and obtains his or her municipal permit. This part of the process itself might only take 10 hours (7 hours at the different offices and 3 hours of travel), but the total process takes more than a day because it is necessary to have an appointment at each of the offices. The appointments are made on the phone or online, so the social worker has to assist the *cuidacoches* in setting up the appointments. For T2, the social worker, in addition to accompanying the *cuidacoches* through the entire process, also pays the entire cost. Since a possible concern of field experiments is that the evaluation itself may cause the treatment or comparison group to change its behavior because it is conscious of being observed (Duflo, Glennerster and Kremer 2007), we personally trained the social workers to avoid any commentary to *cuidacoches* that might have induced them to think that they were part of an experiment.

Only one *cuidacoches* refused to be surveyed. The field supervisor closely monitored social workers to help them in case they encountered any difficulty with the *cuidacoches* or with the procedure.

F. Identification strategy of the field experiment

Given that our research design is a randomized control trial, the identification strategy is straightforward. To evaluate the impact of the intervention, we start by considering both treatments together:

$$y_{ic} = \alpha + \delta_0 treat_c + X_i`\beta_i + u_{ic} \qquad (1)$$

where y_{ic} takes the value 1 if *cuidacoches* i located in area c receives the municipal permit and 0 otherwise. We do not need to rely on what the *cuidacoches* declare about the permit possession. Our measure of the

possession of permits comes from administrative data provided by the municipality that registers every permit expedited. $treat_c$ takes the value 1 if the *cuidacoches* is assigned to a cell (group of street blocks) selected to receive *one-on-one assistance* (i.e., T1 or T2), regardless of whether the *cuidacoches* also receives cost coverage. X_i is a vector of *cuidacoches'* characteristics. The standard errors of the estimates for this and all subsequent models are clustered by cells. The coefficient of $treat_c$ in this specification is a consistent estimate of the average percentage change in the take-up of the municipal permit from assignment to the treatments.

To evaluate the effect of each treatment on the permit possession, we estimate the following:

$$y_{ic} = \alpha + \sum_{j=1}^{2} \delta_{0j} treat_c^j + X_i'\beta_i + u_{ic} \qquad (2)$$

where $treat_c^j$ denotes the treatment groups, and δ_{0j} captures the causal effect of treatment j on obtaining the permit under the identifying assumption that $treat_c^j$ is orthogonal to u_{ic}.

IV. Results

Table 8 reports the results from the OLS estimator. Columns 1 to 4 display the results from equation (1) and show that a worker who receives one-on-one assistance is three times more likely to receive the municipal permit than a worker that does not receive such support. Columns 5 to 8 display the results of the OLS estimator of δ_{01} and δ_{02} in equation (2). These coefficients are also significantly different from zero, and this fact holds when including different controls, confirming that one-on-one assistance is effective in increasing the compliance rate with the permit requirements. The likelihood of fulfilling the requirements to achieve a municipal permit is 14 percentage points higher for *cuidacoches* in the *one-on-one assistance treatment* (T1) than in the control group; this represents a threefold increase over the mean of the *control group*. Columns 5 to 8 also show that one-on-one assistance plus cost coverage (T2) is effective in increasing the rate of take-up of the municipal program. *Cuidacoches* in the *one-on-one assistance plus cost coverage treatment* show a 23-percentage-point increase in the likelihood of taking-up the municipal program in comparison to the *control group*. This represents a likelihood that is four times that of the *control group*. Interestingly, the estimate of the rate of take-up of the control group (8 percent) seems to be the upper bound of the real rate because of a) the possible contamination effect from individuals in the treatments groups that work a few blocks away and may share positive experiences regarding taking-up the municipal program, and b) probable general equilibrium effects: a *cuidacoches* in the control group may observe that many other *cuidacoches* are obtaining their permits and think that this could end in an equilibrium, where only the *cuidacoches* with municipal permits may keep their street segments.

Notwithstanding the notorious difference between $\hat{\delta}_{01}$ and $\hat{\delta}_{02}$, we cannot reject the null hypothesis that δ_{01} and δ_{02} are equal (the t value of the difference is 1.36). We are not able to rule out that the lack of statistically significant difference between the two main treatments (assistance vs. assistance + cost coverage) may be largely due to lack of statistical power. We think that a 9 percentage point difference in take-up is a big difference, but unfortunately we cannot distinguish it from chance.

Table 9 presents the results of a mean comparison by obtaining a municipal permit within the treatment group. This allows us to examine which characteristics of *cuidacoches* might be correlated with greater take-up of the municipal program. We find statistical evidence of the demand for such take-up being higher among older workers (p=0.05), which might be because vulnerable elders are more prone to want to secure their jobs (the municipality and the police protect the regularized *cuidacoches* if someone tries to take them out of their assigned block). *Cuidacoches* that have an expired work permit seem to be more likely to obtain a new one in comparison to those who never obtained one (p=0.05), perhaps because they are already familiar with the procedures required to obtain the permit and simply need a little nudge from the social worker to accomplish such requirements.

We found other differences, though not significant, among the treatment groups. As women may be more vulnerable in the street, they seem to be more likely to take-up the municipal program (to receive the protection of municipal authorities). In addition, those who seem to be making a living by guarding cars (work more hours per day as *cuidacoches* and have been doing the job for more years) are also more open to obtaining the municipal permit. There is no significant difference in the take-up of the municipal program by the number of minors under care, years of education, and type index, although the standard errors are relatively large for some of the dimensions of heterogeneity. Overall, we view the results as indicating that those *cuidacoches* who are older and have held a work permit are more likely to obtain the municipal permit.

V. Discussion

Growing concerns about the low take-up rates for government support programs have spurred calls to simplify the application process and enhance visibility. If take-up is low, then the targeted programs may fail to reach their main goal of providing a minimum bundle of goods for the target group. Some theorize that millions of deeply vulnerable adults around the globe, who are eligible for welfare programs, are overwhelmed by the complexity of the public aid process. Employing a unique database, we present the results from a randomized field experiment in which disadvantaged individuals were offered one-on-one assistance to cope with the behavioral and bureaucratic barriers to achieve a social benefit. The municipality of Montevideo—a city with 1,400,000 inhabitants in Latin America—has established a social program that provides an official permit for daily work in a specific segment of street and making contributions to pension plans. We find that a worker

who receives one-on-one assistance is three times more likely to receive the permit (those who receive both one-on-one assistance and cost coverage of the documentation are four times more likely to receive the permit). Our results suggest that one-on-one assistance is a promising intervention for vulnerable individuals. Most of the workers absorbed by this disadvantaged setting are unskilled and suffer from the precarious conditions that the job entails (they have to cope with adverse weather conditions, and many of them are homeless, with poor or no health coverage or pension insurance, suffering a permanent deterioration of their human capital).

To our knowledge, we are the first to identify a successful intervention focused on helping disadvantaged individuals that work in street markets where goods and services are sold in informally assigned areas. This type of market is of paramount importance in the understanding of contemporary phenomena such as those found in blocks where vehicles are washed by informal workers, streets where garbage is picked up in exchange for voluntary financial compensation or in markets where goods are sold in squares and at traffic lights.

Several attempts have been made in different countries to regulate these practices, but governments have to address a large barrier: the behavioral obstacles faced by deeply vulnerable populations such as *cuidacoches*. Some of the behavioral barriers may be procrastination, poor long-run decisions, overemphasizing of the present, perceived negative social identity, perplexity of the procedure to take-up the support program, and too much reliance on routine. One-on-one assistance from a professional social worker have helped the *cuidacoches* to cope with these barriers to receive the social benefits from holding a municipal permit.

We will perform three additional analyses to provide a broader picture of our field experiment. We will first discuss a back-of-the-envelope cost-benefit analysis of expanding our intervention to the whole population. Our second extension will be to merge our experimental data with an administrative registry of people involved in other welfare programs to determine whether the one-on-one intervention had spillover effects. Finally, we will study the effect of providing information alone, providing a brochure to the *cuidacoches,* but no personal assistance or cost coverage.

A. Cost analysis

One possible concern about one-on-one assistance is the cost of the intervention: an exploration of the costs seems to be mandatory, though most previous studies do not include it. We offer the following study of the budget of our intervention.

We calculated the total cost of the *one-on-one assistance plus cost coverage treatment* at USD 123 per *cuidacoches* (Section A1 of the Appendix shows the components of the cost in detail). This includes the

payment of the assistant (USD 79), the coverage of the costs of the required documents (USD 34 at most), and travel allowances for both the assistant—social worker—and *cuidacoches* (USD 10). Although the assistant receives the travel allowances in advance, he or she receives his or her fees only if the *cuidacoches* achieve the municipal permit from the municipality. The cost of the *one-on-one assistance treatment* is USD 89 (USD 79 assistant fee plus USD 10 for travel allowances).

It is estimated that the population of *cuidacoches* in Montevideo is approximately 3,000, and only half of them possess the municipal permit required by the municipality (Blanco, Cabrera and Cid 2016). If no intervention is applied, it is expected that, at most, 8 percent of the *cuidacoches* will end up obtaining a municipal permit (1,500 x 0.08 = 120 *cuidacoches*). With a program design such as the *one-on-one assistance plus cost coverage treatment*, we expect a 23-percent increase in the likelihood of receiving the municipal permit, that is, a final figure of 465 *cuidacoches*, and a total cost of USD 57,195.

At this point, we cannot perform a traditional cost-benefit analysis because it is not possible to precisely estimate the monetary benefits to society from having *cuidacoches* in the streets holding municipal permits instead of unregulated *cuidacoches*, nor can we estimate the benefits for *cuidacoches* from obtaining a municipal permit (i.e., less use of violence to protect their place in the street—as suggested by Blanco, Cabrera and Cid 2016—or better access to public health services due to having a health card—as suggested by Martínez and Barreiro 2015). The aim of this cost analysis section is to convey that the cost of these interventions is affordable: at most, the intervention may cost USD 123 per *cuidacoches*. In terms of the average income of *cuidacoches*, this cost is approximately 8 workdays. In terms of the minimum wage in Uruguay, the 123 USD cost of the intervention equals one-third of the monthly minimum wage. Thus, the cost of the intervention is affordable both in terms of *cuidacoches*' daily income and in comparison with the minimum wage per day in the country.

Finkelstein and Notowidigdo (2019) develop a model that allows to formally assess the welfare implications of interventions that inform individuals about their likely eligibility ("information interventions") or reduce the private costs of applying ("assistance interventions"). In particular, authors include in the model the fact that interventions may decrease targeting, thus affecting private and public welfare outcomes. Seemingly contrary to the "behavioral" hypothesis that information barriers and transaction costs deter the neediest eligible individuals, marginal applicants and enrollees may be less needy than average applicants or enrollees. A key feature of the model of Finkelstein and Notowidigdo (2019) is a fiscal externality on the government from the program, which creates the standard wedge between private and socially optimal application choices; they model this as the public costs of processing each application and paying benefits for the marginal enrollees. A sufficient condition for interventions that increase targeting to be more likely to increase private welfare is that the under-estimation of expected benefits is greater in percentage terms for

the targeted individuals. However, even in this case, the social welfare impacts of increased targeting remain ambiguous. Indeed, the model shows that the targeting properties of the intervention have no general relationship to its social welfare impact; analysis of social welfare requires information not only on how misperceptions vary across individuals, but also how the size of the fiscal externality varies across these individuals.

B. Spillover effects on the take-up of other public programs

The documents required by the municipality to receive the work permit (identity card, crime record report, and health card) are the same as those demanded by other government support programs. We explore the possible spillover effects of our intervention on the take-up of other social benefits.

We identify more than 10 public support programs where *cuidacoches* may apply. We merge our database (339 *cuidacoches)* with the administrative registry from the Ministry of Social Development (MIDES). We study the spillover effects of our intervention on those programs not targeted by our professional social workers. In Appendix A2, we provide all the details of this analysis. We are not able to reject the null hypothesis that there were no impacts of the experiment on the take-up of other national welfare programs. This finding suggests that deeply vulnerable populations require intensive and professionally provided one-on-one assistance to access other social programs that would benefit them. A one-shot intervention targeted at one specific program is not enough.

C. The impact of providing information

Finally, we explore the possible impact of providing information to increase the take-up of the municipality support program. Guided by the Ethics Committee (see Section III. C. iii.), we provide a brochure containing information to both the control and treatment groups. Thus, we have no means to disentangle the impact of this information using only the field experiment.

To assess the impact of this information brochure provided to the *cuidacoches* in the experiment, we exploit the fact that many blocks of Montevideo are not part of the experiment but have *cuidacoches* anyway. Those blocks are not treated because of budget constraints. Many parts of the city are distant or have low concentrations of *cuidacoches:* including these regions in the field experiment would have significantly increased the cost of the surveys. We employ a synthetic control strategy with the aim of comparing the municipal permits obtained in the cells that have only received the information brochure (the control group in the field experiment) with the cells that were not part of the experiment at all.

With the administrative municipality records, we are able to build a balanced panel database for the period 2008-2015. The observations are at the cell level and contain the number of *cuidacoches* permits

issued. Thus, we have seven years of preintervention data to build *synthetic control.* The full description of the analysis is presented in Appendix A3.

Our estimates suggest that providing information, even if delivered *directly* to each targeted individual, is not enough to promote the take-up of support programs within this deeply vulnerable population. This finding highlights the importance of a *boots-on-the-ground* approach that helps disadvantaged individuals by removing the barriers in each of the steps with the hope of reaching the goal of them entering a support program.

VI. Conclusions

The results of the *one-on-one assistance* experiment demonstrate strong effects from offering intense and involved personal help to *cuidacoches*. We find that this intervention is effective in increasing the number of *cuidacoches* who enter the support program (receive a municipal permit), both as an isolated treatment or combined with cost coverage. The impact is economically relevant: while the control group experiences a rate of take-up of 8 percent, the *one-on-one assistance treatment* increases the rate of take-up by 14 percentage points (this represents a threefold increase in the likelihood of compliance in comparison to the control group), and the *one-on-one assistance plus cost coverage treatment* increases the rate of take-up by 23 percentage points (this represents an fourfold increase in the likelihood of receiving a municipal permit in comparison to the control group). We find that this achievement does not translate into greater take-up of other government support programs to which the *cuidacoches* would qualify. This finding reinforces the key role of one-on-one assistance: in the absence of one-on-one help, *cuidacoches* do not take-up other public benefits. Our results also suggest that providing only information to the *cuidacoches* has no impact on their compliance with the requirements of the municipal program.

Further research may explore the effects of *one-on-one assistance* on other areas such as labor outcomes, financial inclusion (opening a bank account), or access to the health and pension system. Moreover, the findings of this study may foster further research aimed at developing strategies to help deeply vulnerable populations in other countries.

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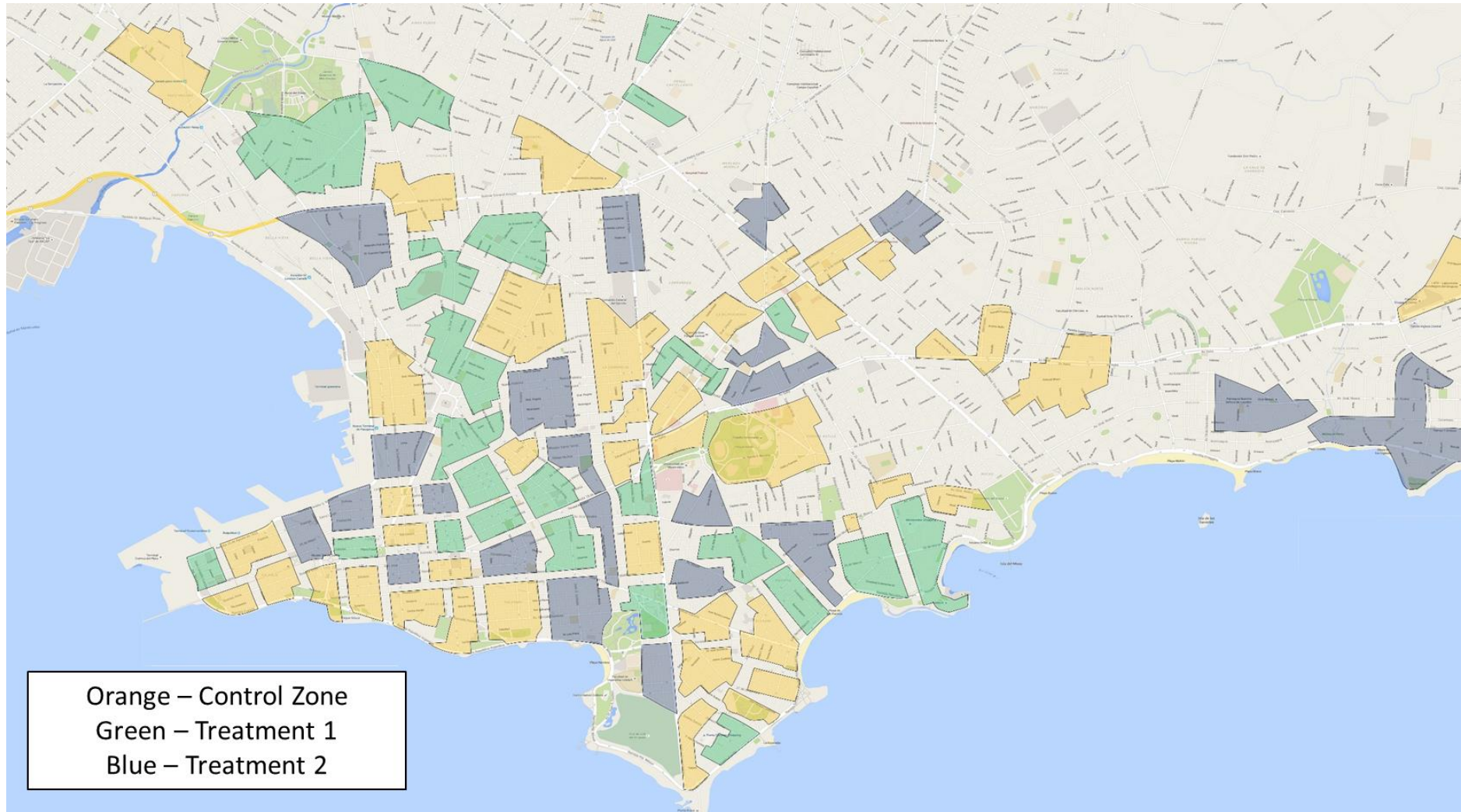
Figure 1. The highlighted zones identify the cells (containing blocks) selected to be divided into three groups (Control, Treatment 1, and Treatment 2), in order to implement the randomization. These zones are the ones with the greatest density of *cuidacoches* (Cabrera & Cid, 2017).

Figure 2. Example of map cells randomized into treatment groups. Between the experimental cells there are buffer zones of one block which were not included in the experiment in order to avoid contamination effects.

Table 1 - Well-being associated with municipal work permit

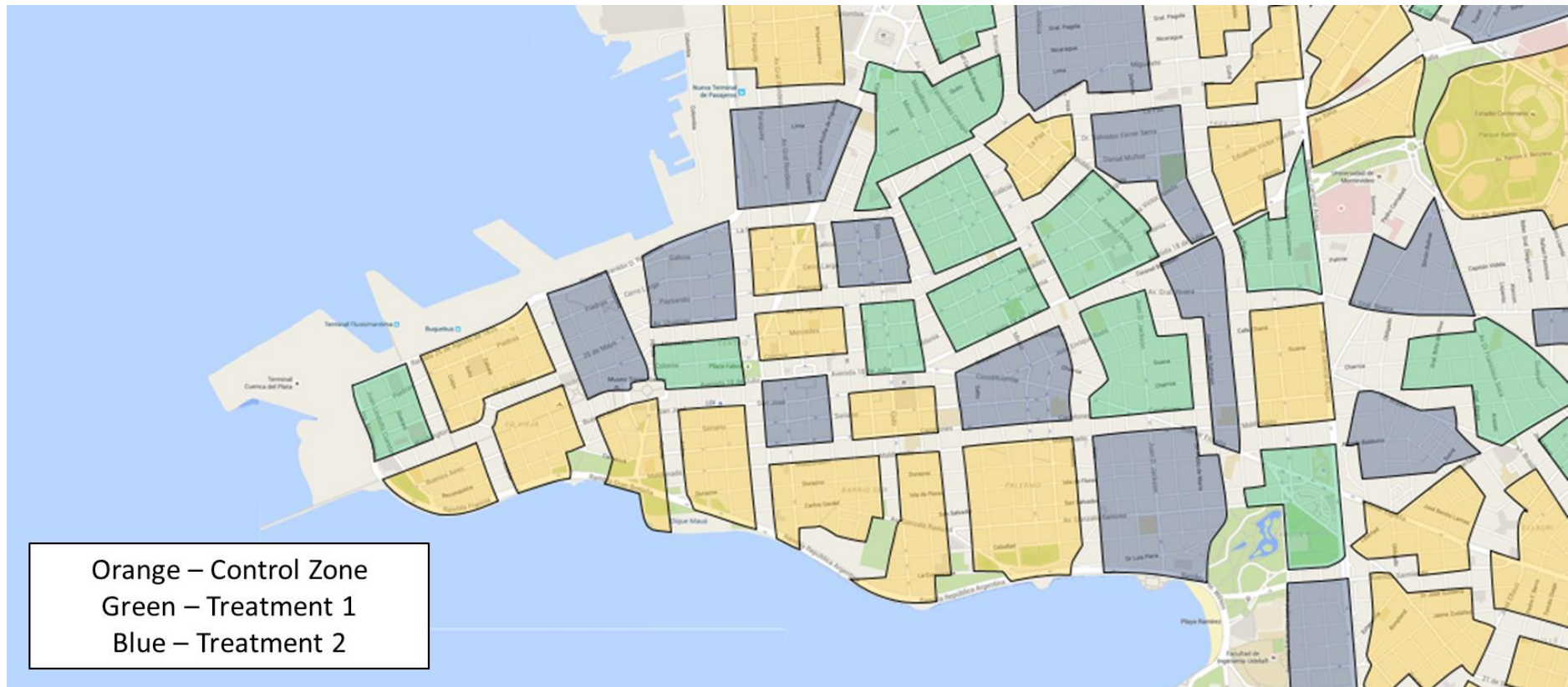| Outcome:	(1) Monthly payment in logs	(2) Savings	(3) Homeless	(4) Health Care	(5) Type Index	(6) Violence Index
	(Earnings from guarding cars in the block.)	(= 1 if the *cuidacoches* has spare money at the end of the month, 0 if he has nothing left to save.)	(= 1 if the *cuidacoches* is homeless.)	(= 1 if the *cuidacoches* has his health covered either by himself or through his couple, 0 if he doesn´t have health coverage.)	(Indicator of external appearance: language, substance abuse, dental care and tidiness; the higher the index [from 0 to 4], the poorer the condition.)	(Indicator of usage of violence for protecting the workplace; the higher the index [from 0 to 2], the more violent the person.)
Having the work permit	0,150***	0,100***	-0.11***	0.18***	-0.49***	-0,200***
Controls:						
Age	Yes	Yes	Yes	Yes	Yes	Yes
Female	Yes	Yes	Yes	Yes	Yes	Yes
Years of education	Yes	Yes	Yes	Yes	Yes	Yes
Observations	532	434	538	498	511	503

Note: OLS estimates (each estimate includes a constant, but it is not shown in the table).
Source: survey 2013.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 2 – Permits issued by the Municipality in 2014

Variable	Description of variables	Total	Percentage
Total	Number of permits provided to cuidacoches.	181	-
"Active"	Cuidacoches with an unexpired work permit at the end of 2014.	81	45%
"Inactive"	Cuidacoches with an expired work permit at the end of 2014.	100	55%
Women	Number of permits provided to female cuidacoches.	30	16%
Men	Number of permits provided to male cuidacoches.	151	84%
Age	Average age in years.	52	-
Women´s Age	Average women´s age in years.	52	-
Men´s Age	Average men´s age in years.	52	-

Note: Source Municipal Authorities database. Data corresponding to the inflow for year 2014.

Table 3 - Benefits of having the municipal work permit

	Mean	S.D.	Min	Max	#Obs.
Benefits of having the municipal work permit, reported by cuidacoches <u>with</u> a valid permit (*)					
I own the block, no one can take me out from it.	0.75	0.43	0.00	1.00	327
The police protects me if someone tries to take me out of the block.	0.24	0.43	0.00	1.00	327
I get better tips.	0.15	0.36	0.00	1.00	327
The Municipality gives me a vest and I find it useful.	0.13	0.34	0.00	1.00	327
I want to pay the BPS monotax (a contribution to receive a future pension for the elderly.)	0.04	0.20	0.00	1.00	327
Estimated benefits of having the municipal work permit, reported by cuidacoches <u>without</u> a valid permit (**)					
There would be no benefit.	0.42	0.49	0.00	1.00	200
It would give me confidence/I would feel more secure.	0.30	0.45	0.00	1.00	200
I would own the block, no one could take me out from it.	0.24	0.43	0.00	1.00	200
The police would protect me if someone tried to take me out of the block.	0.13	0.33	0.00	1.00	200
I would get better tips.	0.07	0.25	0.00	1.00	200
I find useful the vest the IMM would give me.	0.06	0.24	0.00	1.00	200

Note: (*) Source survey 2013.
(**) Survey November 2015 - April 2016.

Table 4 - Definition and Description of Variables

Variable	Description of variables	Mean	S.D	Min	Max	#Obs.
Female	1 if the person is female, 0 otherwise.	0.09	0.28	0.00	1.00	339
Years working as a cuidacoche	Number of years the person has worked in the block as a *cuidacoches.*	5.73	6.77	0.04	39.00	339
Age	Age in years.	42.80	14.02	17.00	82.00	332
Hours per day working as a *cuidacoches*	Hours worked on an average weekly day.	9.56	2.74	3.00	16.00	339
The *cuidacoches* had a municipal permit but expired	1 if the person has got a work permit but it has expired and 0 if the person has never got it.	0.27	0.45	0.00	1.00	339
Type Index	Index composed of four dummy variables: physical appearance, denture condition, substance abuse and language of the *cuidacoches* observed by the interviewer. The higher the index (from 0 to 4), the poorer the condition.	0.76	0.94	0.00	4.00	258
Years of education (*)	Years of completed education.	5.89	2.89	0.00	16.00	226
Minor children (*)	Number of minor children under their care.	0.55	1.07	0.00	8.00	246

Note: Source baseline survey, March-May 2015.
(*) Source follow up survey, November 2015 - April 2016.

	Men	Women	Difference	S.E.	p-value	#Obs.
Table 5 - Reasons for not having a municipal work permit: Mean Comparison by Gender						
It is complicated getting the health card.	0.29	0.47	0.18**	0.09	0.05	338
I cannot lose working hours on procedures.	0.27	0.13	-0.14*	0.09	0.10	338
Having the permit is not necessary for working here.	0.32	0.17	-0.15*	0.09	0.09	338
The procedures for getting the work permit are complicated (®).	0.13	0.00	-0.13	0.08	0.11	246
I have never had my judicial records (®).	0.13	0.06	-0.07	0.08	0.42	246
The Municipality is far away.	0.07	0.13	0.06	0.06	0.23	338
I do not have enough money to pay for/renew the work permit.	0.31	0.33	0.02	0.09	0.78	338
I have no desire to get the work permit.	0.21	0.20	-0.01	0.08	0.92	338
I am not well informed about the procedures for getting the work permit (®).	0.35	0.35	0.00	0.12	0.99	246

Note: This table includes the reasons why those who never got a work permit do not have one and the reasons why those who have an expired work permit do not renew it.

(®) Answer options only available for those who never got their work permit.

Source baseline survey, March-May 2015.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

*Significant at the 10 percent level.

Table 6 - Mean Comparison of Baseline Characteristics (data at cell level)

	T1	C	Diff.	S.E.	p-value	#Obs.	T2	C	Diff.	S.E.	p-value	#Obs.	T2	T1	Diff.	S.E.	p-value	#Obs.
Cuidacoches (number)	5.70	5.00	-0.70	-1.03	0.50	65	5.09	5.00	-0.09	-1.02	0.93	65	5.09	5.70	0.61	-1.36	0.66	46
Area	307.02	322.02	15.00	-54.60	0.78	65	346.96	322.02	-24.94	-58.17	0.67	65	346.96	307.02	-39.94	-63.67	0.53	46
Cars (average by Household)	0.42	0.45	0.03	-0.06	0.70	65	0.47	0.45	-0.02	-0.07	0.74	65	0.47	0.42	-0.05	-0.08	0.56	46
Residential dwellings (number)	567.73	570.81	3.08	-42.97	0.94	65	570.94	570.81	-0.13	-44.82	1.00	65	570.94	567.73	-3.21	-42.59	0.94	46
Households (number)	530.35	518.60	-11.75	-38.51	0.76	65	533.46	518.60	-14.86	-40.95	0.72	65	533.46	530.35	-3.11	-40.03	0.94	46
Apartments (pct)	0.71	0.68	-0.03	-0.05	0.51	65	0.68	0.68	-0.00	-0.05	0.91	65	0.68	0.71	0.03	-0.06	0.62	46
Rooms (avg number)	3.21	3.20	-0.01	-0.12	0.95	65	3.29	3.20	-0.09	-0.13	0.53	65	3.29	3.21	-0.08	-0.16	0.63	46
Habitants (avg by hhold)	2.28	2.31	0.03	-0.07	0.62	65	2.32	2.31	-0.01	-0.07	0.90	65	2.32	2.28	-0.04	-0.08	0.60	46
Owner (pct)	0.50	0.53	0.03	-0.03	0.30	65	0.52	0.53	0.01	-0.03	0.75	65	0.52	0.50	-0.02	-0.03	0.57	46
Age	40.76	41.12	0.36	-0.58	0.54	65	40.51	41.12	0.61	-0.55	0.27	65	40.51	40.76	0.25	-0.72	0.73	46
Primary education (avg)	0.14	0.15	0.01	-0.01	0.48	65	0.14	0.15	0.01	-0.01	0.39	65	0.14	0.14	0.00	-0.02	0.92	46
Employed (pct)	0.55	0.55	0.00	-0.01	0.74	65	0.56	0.55	-0.01	-0.01	0.32	65	0.56	0.55	-0.01	-0.01	0.51	46
Retiree (pct)	0.17	0.17	0.00	-0.01	0.72	65	0.16	0.17	0.01	-0.01	0.16	65	0.16	0.17	0.01	-0.01	0.39	46

Note: The number of *cuidacoches* in each cell come from Blanco, Cabrera and Cid (2016). The number of cells is 88 (42 correspond to "Control" (C), 23 to "Treatment 1" (T1) and 23 to "Treatment 2" (T2)).
The data come from the Uruguayan National Institute of Statistics (2014).
Randomization was performed using the first 3 variables of the table to stratify.
*Significant at the 1 percent level.
*Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 7 - Mean Comparison of Baseline Characteristics (data at individual level)

	Treatment 1	Control	Diff.	S.E.	p-value	#Obs.	Treatment 2	Control	Diff.	S.E.	p-value	#Obs.	Treatment 2	Treatment 1	Diff.	S.E.	p-value	#Obs.
Female	0.08	0.09	0.01	0.04	0.82	263	0.08	0.09	0.01	0.04	0.70	268	0.08	0.09	0.01	0.05	0.90	147
Years working as a *cuidacoches*	5.90	5.45	-0.45	0.88	0.61	263	6.29	5.45	-0.84	0.90	0.36	268	6.29	5.90	-0.39	1.27	0.76	147
Age	43.00	42.70	-0.30	2.02	0.74	257	42.58	42.70	0.12	1.94	0.95	263	43.00	43.33	0.33	2.23	0.71	144
Hours per day working as a *cuidacoches*	9.10	9.70	0.60	0.37	0.10	263	9.60	9.71	0.11	0.36	0.74	268	9.60	9.10	-0.50	0.50	0.31	147
The *cuidacoches* had a municpal permit but expired	0.27	0.27	0.00	0.06	0.96	263	0.29	0.27	-0.02	0.06	0.69	268	0.29	0.27	-0.02	0.08	0.77	147
Type Index	0.76	0.75	-0.01	0.13	0.94	263	0.79	0.75	-0.04	0.12	0.75	268	0.79	0.76	-0.03	0.17	0.86	147
Years of education (*)	5.92	5.86	-0.06	0.53	0.91	173	5.96	5.86	-0.1	0.45	0.82	179	5.96	5.92	-0.04	0.57	0.93	100
Minor children (*)	0.39	0.66	0.27	0.18	0.13	189	0.46	0.66	0.2	0.18	0.27	194	0.46	0.39	-0.07	0.15	0.64	109

Note: Source baseline survey, March-May 2015.
(*) Source follow up survey, November 2015 - April 2016.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 8 – Treatment effect over obtaining the municipal work permit

Outcome: Obtaining the work permit	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(Mean of the control group: 0.0833)								
T=T1+T2: One-on-one assistance for all *cuidacoches*, mixed with cost coverage for some *cuidacoches*	0.189*** (0.054)	0.186*** (0.053)	0.190*** (0.052)	0.182*** (0.043)				
T1: One-on-one assistance					0.142*** (0.068)	0.145*** (0.068)	0.150*** (0.070)	0.154*** (0.060)
T2: One-on-one assistance plus cost coverage					0.232*** (0.074)	0.223*** (0.070)	0.225*** (0.070)	0.209*** (0.056)
Controls:								
Age	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Female	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Years working as a *cuidacoches*	No	No	Yes	Yes	No	No	Yes	Yes
Hours per day working as a *cuidacoches*	No	No	Yes	Yes	No	No	Yes	Yes
The *cuidacoches* had a municipal permit but expired	No	No	Yes	Yes	No	No	Yes	Yes
Type Index (Indicator of external appearance: language, substance abuse, dental care and tidiness)	No	No	Yes	Yes	No	No	Yes	Yes
Fixed Effects: Pollster	No	No	No	Yes	No	No	No	Yes
Observations	339	332	332	332	339	332	332	332

Note: OLS estimates (each estimate includes a constant, but it is not shown); robust standard errors in parentheses.
We cluster standard errors at cell level (group of blocks) in all models.
Source: Municipal Authorities database (December 2015) and baseline survey (March-May 2015).
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 9 – Treatment Characteristics: Mean Comparison by Municipal Work Permit

	Treatment *cuidacoches* that ended with permit	Treatment *cuidacoches* that ended without permit	Difference	S.E.	p-value	#Obs.
Age	46.49	41.62	-4.87**	2.47	0.05	144
The *cuidacoches* had had a municipal work permit that expired before the start of the experiment	0.40	0.23	-0.17**	0.08	0.05	147
Female	0.13	0.07	-0.06	0.05	0.24	147
Minor children (+)	0.29	0.48	0.19	0.16	0.25	109
Type Index	0.90	0.73	-0.17	0.19	0.36	147
Hours per day working as a *cuidacoches*	9.62	9.26	-0.36	0.54	0.51	147
Years of education (+)	5.79	6.02	0.23	0.61	0.71	100
Years working as a *cuidacoches*	6.42	5.98	-0.44	1.42	0.76	147

Note: Source baseline survey, March-May 2015.
(+) Source follow up survey, November 2015 - April 2016.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
*Significant at the 10 percent level.

Appendix

A1. Components of the costs of the required documents to obtain a *cuidacoches* permit

A1.1 Identity card

One of the requirements is a national identity card. Its cost is relatively low (approximately USD 8)—the average daily income of a *cuidacoches* is USD 14 (Cabrera and Cid 2017)—but the procedure may take several days: an appointment for several weeks in the future is set up through a phone call or online. On the appointed date, the procedure may take about an hour. An express procedure costs USD 14. Additionally, it is possible to obtain an identity card for free if a person demonstrates that he or she is below the poverty line.

A1.2 Criminal record report

Once the *cuidacoche*s have obtained the identity card, he or she should obtain his or her criminal record report. This document is standard and may be required for any employee. It reports if a person has a criminal record or not. The cost of the standard procedure is USD 2.5, and the express procedure costs USD 5.

A1.3 Health card

Finally, the *cuidacoches* must obtain a standard health certificate that is mandatory for every worker in the country. This card implies clinical studies and a check-up by a doctor and a dentist. These clinical studies and check-ups are basic, and in an hour, an individual may obtain a health card. If the blood test or the check-up show anything wrong, the clinic may deny the issue of the health certificate or may issue a provisional one. It is mandatory to renew the health card every two years, but if an individual receives a provisional card, he or she may have a job but are obliged to renew it in a few months. There is no shortage of clinics that offer these health certificates, scattered across many neighborhoods, for a low price (approximately USD 15). Additionally, a person who demonstrates that he or she is below the poverty line may obtain a health card for free in some public health facilities. An important feature of the health card is that it demands more previous clinical tests for women. Even though the cost is the same (USD 15), a woman must have a mammogram and a Papanicolaou test. She may receive these tests for free at public health facilities, but she may cope with a waiting list of several days.

A2. Spillover effects on the take-up of other public programs

A2.1 Descriptive statistics

Table A1 reports the descriptive statistics about the *cuidacoches'* take-up of welfare benefits at baseline. We were able to find the administrative data of 308 out 339 *cuidacoches* from the Ministry of Social Development

(MIDES). Only 11 percent of the *cuidacoches*' households take-up the *TUS* Food Stamp program (*Tarjeta Uruguay Social*: a government program that helps with money for basic consumer goods for vulnerable households). Less than 18 percent of the *cuidacoches* take-up the AFAM benefit (*Asignaciones Familiares*: a government program that helps with money to vulnerable households with children attending school). This may be evidence that they have no permanent household (approximately 20 percent spend the night at shelters sponsored by the Ministry of Development, and many reported living in the street, as Cabrera and Cid [2017] show). FONASA (*Fondo Nacional de Salud*) is a national fund that receives the mandatory contributions of formal workers and is devoted to covering the health of the worker, his or her spouse and their children. One out of four *cuidacoches* contributed at least one month to this public fund. At first sight, it seems to be an important proportion for vulnerable individuals, but we should bear in mind that, for instance, an unskilled construction laborer that worked only one month will be registered as a FONASA payer. The ASSE (*Administración de los Servicios de Salud del Estado*) is a national program that offers health services for formal workers and pensioners. Three out of four *cuidacoches* seem to have had at least one month of the right to access the health service. They could have attained this right due to working only a month as a laborer or because they are pensioners or spouses of a formal worker. Regardless, the right to access health services does not mean that the *cuidacoches* truly used these health services. Finally, we can observe that the *cuidacoches* at the baseline present a low welfare take-up of programs such as retirement pensions, disability benefits, pensions due to old age and critical disadvantaged household conditions, housing subsidies from the public institutions ANV (Agencia Nacional de Viviendas) or BPS (Banco de Previsión Social), training or jobs for long-term unemployed individuals living in deeply vulnerable households.

A2.2 The impact on the take-up of other support programs

The documents required by the municipality support program (identity card, health card, and criminal record report) coincide with those required by most national support programs. We assess whether those *cuidacoches* nudged into the municipality support program take-up the benefits of other programs of the national welfare system. The social workers who were employed to apply our intervention did not help the *cuidacoches* with applying to programs other than the municipal one.

Since the *cuidacoches* do not randomly decide to take-up the municipal permit, we use the indicator of random selection into treatment (ITT, intention to treat) as the relevant explanatory variable. We run equation (1), where y_{ic} takes the value 1 if *cuidacoches i* located in area *c* take-up national welfare programs and 0 otherwise, and $treat_c$ takes the value 1 if the *cuidacoches* are assigned to a group selected to receive *one-on-one assistance* (regardless of whether the *cuidacoches* also received cost coverage). X_i is a vector of *cuidacoches*' characteristics. We also run instrumental variable regressions using the ITT indicator as an instrument for effective participation.

Table A2 reports –in the follow up- the average of take up by treatment. All in all, we are not able to reject the null hypothesis that there is no impact on welfare entries. We also run OLS regressions adding control variables (age, gender, type index, ever having a municipal permit, months working as *cuidacoches*, working hours a day at the street), and instrumental variables regressions using the random assignation to the treatment as an instrument: there is no change in the results (estimates are available upon request).

Thus, though one-on-one assistance (both with and without cost coverage) is effective to make the *cuidacoches* take-up the municipality support program, this achievement does not translate into take up of national welfare benefits where the personal assistance is not present.

A3 Assessing the impact of providing information

A3.1 Identification strategy

In order to assess the impact of the information brochure provided to the *cuidacoches*, we exploit the fact that many blocks of Montevideo had not been selected to participate in the experiment just because they were distant or had low concentration of *cuidacoches.* Including these zones would have increased significantly the cost of the surveys. We employ a synthetic control strategy with the aim of comparing the municipal permits obtained in the zones that have received only the information brochure with the zones that were not part of the experiment.

We use annual cell-level (group of street blocks) panel data for the period 2008-2015. During 2015, the social workers distributed to the *cuidacoches* of the experiment the brochure containing the information to obtain a municipal permit, giving us seven years of pre-intervention data. Our sample period begins in 2008 because it is the first year for which data on permits issued by the municipality are available. Due to the fact that the municipal authorities reported that the administrative data of 2012 includes grave inaccuracies and serious errors of systematization, we discard from the panel data the observations of 2012. Our results are robust, however, to the inclusion of the discarded year.

The synthetic control is constructed as a weighted average of potential control cells (those cells of Montevideo city that receive no information brochure), with weights chosen so that the resulting synthetic control best reproduces the values of a set of predictors of average permits issued in the city before 2015. Our predictors of permits issued are the average by cell of: number of inhabitants that have a job, number of inhabitants with at least completed Primary Education, number of permits issued in the past. The source of this data is the Continuous Household Survey (Uruguayan National Institute of Statistics). We construct a synthetic control that mirrors -before the intervention- the values of the predictors of permits issued in the cells that received our information brochure. We estimate the effect of the information on average permits

issued as the difference in permits issued between the cells treated with sole information and its synthetic control. We then perform a series of robustness and placebo studies.

A3.2 Assessing the effect of providing information

Figure A2 gives an idea of the treated and control zones. Figure A3 plots the trends in permits issued in (i) the cells treated with the sole information brochure, and (ii) the cells that did not receive any intervention. As this figure suggests, the cells that did not received any intervention may not provide a suitable comparison group for the cells that received the information brochure. Even before the intervention, the time series of permits issued in the cells that received the sole information brochure and in the cells that received no intervention differed notably. To evaluate the effect of information on permits issued, the central question is how permits issued would have evolved in the cells that receive the intervention in the absence of the exposure to the information. The synthetic control method provides a systematic way to estimate this counterfactual.

We construct the synthetic control as the convex combination of cells in the donor pool that most closely resemble the cells treated with the sole information brochure in terms of pre-intervention values of permits issued. The results are displayed in Table A3, which compares the pretreatment characteristics of the actual treated cells with that of the synthetic control, as well as with the average of the 63 cells in the donor pool. We see that the average of cells that did not received the information brochure (the donor pool) does not seem to provide a suitable control group to the cells that received the brochure information. In particular, prior to the intervention, the average permits issued, the number of people employed and the number of people with at least Primary Education were lower in the average of the 63 control cells than in the cells that received the intervention. In contrast, the synthetic control accurately reproduces the values of the predictor variables had in the treated cells prior to the intervention. Table A4 displays the weights of each control cell in the synthetic control.

Figure A4 displays average permits issued for the cells treated with the sole information brochure and its synthetic counterpart during the period 2008-2015. Notice that, in contrast to permits issued in the donor pool (the Control group in Figure A3), permits issued in the synthetic control very closely track the trajectory of this variable in the treated cells for the entire pre-intervention period. Combined with the high degree of balance on all predictors permits issued (Table A3), this suggests that the synthetic control provides a sensible approximation to the number of permits that would have been issued in the cells treated with the sole information in 2008-2014 in the absence of the brochure intervention.

Our estimate of the effect of the information brochure is the difference between permits issued in the treated cells and in its synthetic version after the intervention. Figure A4 shows that, immediately after the intervention, the two lines begin to diverge but only slightly: providing information on how to obtain the

municipality permit seems to have an impact of 0.04 (that is, on average, a cell – group of blocks- that was treated only with information shows 0.04 more permits than the cells that receive no information).

To assess the significance of our estimates, we conduct a placebo study by iteratively applying the synthetic control method used to estimate the effect of information in the treated cells to every other cell in the donor pool. In each iteration we reassign in our data the information intervention to one of the 63 control states, shifting treated cells to the donor pool. That is, we proceed as if one of the cells in the donor pool would have received the brochure containing the information to receive the municipality permit in 2014, instead of treated cells. We then compute the estimated effect associated with each placebo run. This iterative procedure provides us with a distribution of estimated gaps for the cells where no intervention took place.

Figure A5 displays the results for the placebo test. The gray lines represent the gap associated with each of the runs of the test. That is, the gray lines show the difference in the number of municipality permits between each state in the donor pool and its respective synthetic version. The superimposed bold line denotes the gap estimated for the cells treated with information. As the figure makes apparent, the estimated gap for the treated cells after 2014 is nearly null. Table A5 reports that the associated p-value of the permutation test is around 0.6. That is, 60 percent of the placebo iterations show a greater effect than the treated cells. Thus, we are not able to reject the hypothesis that the average of permits issued in the treated cells is equal to the average in the donor pool.

As a robustness check, we repeat the iteration process discarding the zones where the pre-intervention RMSPE is 5, 10 and 20 times greater than in the treated cells. Table A5 shows that, even excluding the cells that might be not good controls, we are not able to reject the hypothesis that the average of permits in the treated cells is equal to the average in the donor pool.

The null or limited impact of information is in line with most previous literature (e.g. Carrell and Sacerdote 2017, Bettinger et al. 2012). Liebman and Luttmer (2015) offer some mechanisms that may explain the limited impact of information. "In some cases, the necessary information may be straightforward to understand, but expensive (in either monetary or psychic terms) to acquire. In other cases, the information about program rules may be readily available, but the calculation necessary to determine an individual's own incentives may be very complicated. In still other cases, cognitive biases may cause people to misperceive even relatively simple incentive schedules. Finally, powerful social cues may point people toward a suboptimal decision, even when the correct information is also readily available" (Liebman and Luttmer 2015, 275-276). Finkelstein and Notowidigdo (2019) offer an exception of these mainstream findings. They run a randomized field experiment in which elderly individuals not enrolled in – but likely eligible for – the Supplemental Nutrition Assistance Program (SNAP). One intervention arm received a mailing - and a follow-up reminder

postcard - from the Secretary of Pennsylvania's Department of Human Services (DHS), informing them of their likely eligibility for SNAP. The authors find that information increases enrollment in the program. However, they suggest to be cautious in the interpretation of the results because they find that their intervention to improve information target *less* needy individuals. They also find that a sub-treatment of the Information Only intervention, which omits the reminder postcard, reduces its impact by about 20 percent: this suggests a role for inattention in explaining at least some of the impact of the Information Only intervention.

In order to assess the robustness of our results, we included additional predictors of permits issued among the variables used to construct the synthetic control. Our results stayed virtually unaffected regardless of which and how many predictor variables we included. The list of predictors used for robustness checks includes the number of households in the group of blocks that own at least one car, housing ownership, households' income, age of the inhabitants, number of bedrooms per household, number of apartments.

Table A1 - Mean differences by Treatment and Control groups (pre-intervention)

Program	Differences	S.E.	Treatment	Control	p-value	#Obs.
AFAM	0.003	0.044	0.171	0.173	0.952	308
TUS Food Stamp cardholder	0.011	0.009	0.000	0.011	0.230	308
TUS Food Stamp household	-0.010	0.036	0.116	0.106	0.780	308
Retirement pension	0.007	0.016	0.016	0.022	0.669	308
Disability retirement pension	0.003	0.011	0.008	0.011	0.764	308
Death pension	0.006	0.007	0.000	0.006	0.397	308
Disability Pension	-0.009	0.033	0.093	0.084	0.778	308
Old-age pension 70+	-0.004	0.013	0.016	0.011	0.741	308
Inscribed to health coverage (FONASA)	0.007	0.051	0.256	0.263	0.894	308
Active health coverage through the Social Security*	0.029	0.049	0.217	0.246	0.558	308
ASSE health coverage	-0.012	0.048	0.783	0.771	0.804	308
Housing ANV	-0.010	0.011	0.016	0.006	0.384	308
Housing BPS	-0.016*	0.009	0.016	0.000	0.095	308
Working program (inscribed)	0.008	0.021	0.031	0.039	0.707	308
Working program (participant)	0.001	0.014	0.015	0.016	0.931	308
Attended public shelters at night	-0.068	0.048	0.264	0.196	0.159	308
Days spent in an attention center	-2.586	7.065	23.217	20.631	0.715	308

Source: Own elaboration based on MIDES data.
Notes: This information corresponds to the pre-intervention period (2014).
*** significant at 1% level; ** significant at 5%; * significant at 10%.

Table A2 - Mean differences by Treatment and Control groups (post-intervention)

Program	Differences	S.E.	Treatment	Control	p-value	#Obs.
AFAM	0.003	0.044	0.171	0.173	0.952	308
TUS Food Stamp cardholder	0.003	0.011	0.008	0.011	0.764	308
TUS Food Stamp household	0.007	0.038	0.116	0.123	0.860	308
Retirement pension	0.012	0.017	0.016	0.028	0.472	308
Disability retirement pension	0.009	0.013	0.008	0.017	0.492	308
Death pension	0.006	0.007	0.000	0.006	0.397	308
Disability Pension	0.002	0.034	0.093	0.095	0.954	308
Old-age pension 70+	-0.006	0.016	0.023	0.017	0.685	308
Inscribed to health coverage (FONASA)	0.011	0.050	0.240	0.251	0.824	308
Active health coverage through the Social Security*	0.009	0.048	0.209	0.218	0.857	308
ASSE health coverage	0.009	0.049	0.767	0.777	0.852	308
Housing ANV	-0.010	0.011	0.016	0.006	0.384	308
Housing BPS	-0.016*	0.009	0.016	0.000	0.095	308
Working program (inscribed)	0.005	0.018	0.023	0.028	0.800	308
Working program (participant)	-0.009	0.011	0.015	0.005	0.385	308
Attended public shelters at night	-0.066	0.048	0.256	0.190	0.168	308
Days spent in an attention center	7.202	7.883	20.558	27.760	0.362	308
Double Food Stamp household	0.014	0.031	0.070	0.084	0.652	308

Source: Own elaboration based on MIDES data.
Notes: This information corresponds to the post-intervention period (2015), except from Double Food Stamp household which corresponds to 2016.
*** significant at 1% level; ** significant at 5%; * significant at 10%.

Table A3 - Adjustment of the Synthetic Model

	Treatment cells*	Synthetic Control cells	Donor Pool Average cell
Number of inhabitants with at least completed Primary Education in the period 2009-2014	1050	1050	1031
Number of inhabitants that have a job in the period 2009-2014	674	674	657
Avg. Permits issued in 2009	0.414	0.413	0.190
Avg. Permits issued in 2010	0.397	0.397	0.127
Avg. Permits issued in 2011	0.241	0.239	0.238
Avg. Permits issued in 2012	0.190	0.190	0.143
Avg. Permits issued in 2013	0.690	0.688	0.413
Avg. Permits issued in 2014	0.724	0.726	0.587
Avg. Permits issued in 2015	1.031	0.971	0.603

Source: Own elaboration based on Unidad de Registro de Cuidadores de Vehículos de la Municipalidad de Montevideo (URCV-IM), Household Surveys (INE) and Cabrera and Cid (2017).
Notes: A cell is defined as a group of street blocks

Table A4 - Donor Pool Ponderators

Zone Code	Weight	Zone Code	Weight	Zone Code	Weight
1	0.009	22	0.011	43	0.005
2	0.008	23	0.023	44	0.008
3	0.009	24	0.01	45	0.008
4	0.006	25	0.045	46	0.006
5	0.008	26	0.111	47	0.026
6	0.005	27	0.005	48	0.008
7	0.005	28	0.006	49	0.006
8	0.005	29	0.005	50	0.005
9	0.006	30	0.005	51	0.004
10	0.01	31	0.008	52	0.17
11	0.003	32	0.011	53	0.006
12	0.008	33	0.008	54	0.007
13	0.004	34	0.07	55	0.008
14	0.007	35	0.009	56	0.065
15	0.046	36	0.006	57	0.076
16	0.006	37	0.005	58	0.008
17	0.011	38	0.009	59	0.009
18	0.005	39	0.008	60	0.005
19	0.005	40	0.004	61	0.005
20	0.005	41	0.005	62	0.009
21	0.006	42	0.008	63	0.006

Source: Own elaboration based on Unidad de Registro de Cuidadores de Vehículos de la Municipalidad de Montevideo (URCV-IM), Household Surveys (INE) and Cabrera and Cid (2017).

Table A5 – Sensitivity test		
Criterion for inclusion of Donor Pool zones	Number of placebo tests	p-value
All	63	0.596
RMSPE > #5	22	0.318
RMSPE > #10	24	0.291
RMSPE > #20	31	0.419

Source: Own elaboration based on Unidad de Registro de Cuidadores de Vehículos de la Municipalidad de Montevideo (URCV-IM), Household Surveys (INE) and Cabrera and Cid (2017).

SOLICITUD DE PERMISO DE CUIDA COCHE

Dirigirse a: Intendencia de Montevideo.
Entrada por Santiago de Chile 1275
Horario: 8:00 a 14:00 hrs
Teléfono: 1950 19 41

REQUISITOS

1. Elegir calle desocupada

2. Certificado de buena conducta

3. Cédula de identidad (+ focotopia)

4. Carnet de salud (+ fotocopia)

5. 3 foto carnet

→ **Cédula de identidad**

Si desea renovarla:
Presentar nombre, apellido y fecha de nacimiento en Dirección Nacional de Identificación (Geant o Rincón 665)

Si nunca obtuvo la cédula:
1) Solicitar en Indendencia (18 de julio 1360) y se otorga certificado de nacimiento. Luego dirigirse a Dirección Nacional de Identificación (Geant o Rincón 665).
2) Si está en situación de calle, debe presentarse en el MIDES a las 8hrs de lunes a jueves (18 de Julio 1453)

Requisitos diferentes (consultar)

→ **Carnet de salud**

3 opciones para obtenerlo:
1) Departamento de Clínicas Preventivas (Durazno 1242).
Horario: 8:00 a 12:00

*Si tiene carnet de asistencia el trámite no tiene costo

2) Intendencia de Montevideo (lunes a viernes de 8 a 10:30)
3) Cualquier mutualista o clínica privada

→ **Certificado de buena conducta**

Se solicita en la Dirección Nacional de Policía Técnica (Guadalupe 1513. de lunes a viernes de 7 a 17:30 hrs).
La persona se presenta con Cédula y se informa que es para presentar en la Unidad de Cuidadores de la Intendencia de Montevideo.

*Tener antecedentes no genera complicaciones para sacar el certificado

Figure A.1. Brochure of information page 1

Figure A.1 (cont). Brochure of information, page 2

Cédula de identidad

Para renovar cédula de identidad, se puede realizar una reserva común o de urgencia.

- **Reserva común:** Se da una fecha y hora en un plazo de 20 días con un costo de 209$. Para pedir hora se hace en los mismos locales o en un local Abitab, Redpagos, correobank o al 09002101 (Ciudad vieja)/ 09002227 (Geant).
- **Reserva de urgencia:** Se da una fecha y hora en un día con un costo de 418$. Para pedir hora se hace en los mismos locales o en un local Abitab, Redpagos, correobank o al 09002102 (Ciudad vieja)/ 09002228 (Geant).

Carnet de salud

1) Se puede retirar en el Departamento de Clínicas Preventivas en la calle Durazno 1242 (a 5 cuadras de la Intendencia). Es gratis presentando el carnet de asistencia y la cédula, o con un costo de 0,4 UR (322$ el 1/3/2015) presentando solo la cédula. El horario de atención es de 8:00 a 10:30 de lunes a viernes. Para contactar con el centro: 29002951 y para pedir hora: 08008610 o en el mismo centro.
2) Se puede solicitar en la Intendencia de Montevideo de lunes a viernes entre 8:00 a 14:00 horas. Se requiere de cédula, el costo es de 414$ o 207$ presentando el carnet de asistencia. Por información contactar al 19503000 opción 4.
3) En cualquier mutualista privada, costos varían.

En general se pide Carnet de vacuna antitetánica, muestra de orina en frasco, 12 horas de ayuno, Certificado médico en caso de enfermedad crónica o bajo medicamentos, llevar lentes si utiliza. Para las mujeres entre 21 y 65 años se requiere Papanicolaou y para las mujeres entre 40 y 59 años se requiere una mamografía. Por más información contactar a IMM o Departamento de Clínicas Preventivas.

Certificado de buena conducta

El costo es de 80$ el trámite común (15 días hábiles) o 160$ el trámite urgente (2 días hábiles). Para contactar llamar al 22091612 interno 28.

Carnet de asistencia (opcional)

Se puede obtener en la oficina de ASSE en Cerro Largo 1816 esquina Fernández Crespo. El centro opera de 8:00 a 17:00 de lunes a viernes, el trámite tardará media hora y el certificado tiene una vigencia de 3 años.

Se requiere de los siguientes puntos y completar un formulario:

- Fotocopia de la cédula de identidad (se puede hacer en el mismo centro).
- Fotocopia de constancia de ingresos (se puede solicitar en el MIDES).
- Fotocopia de constancia de domicilio.

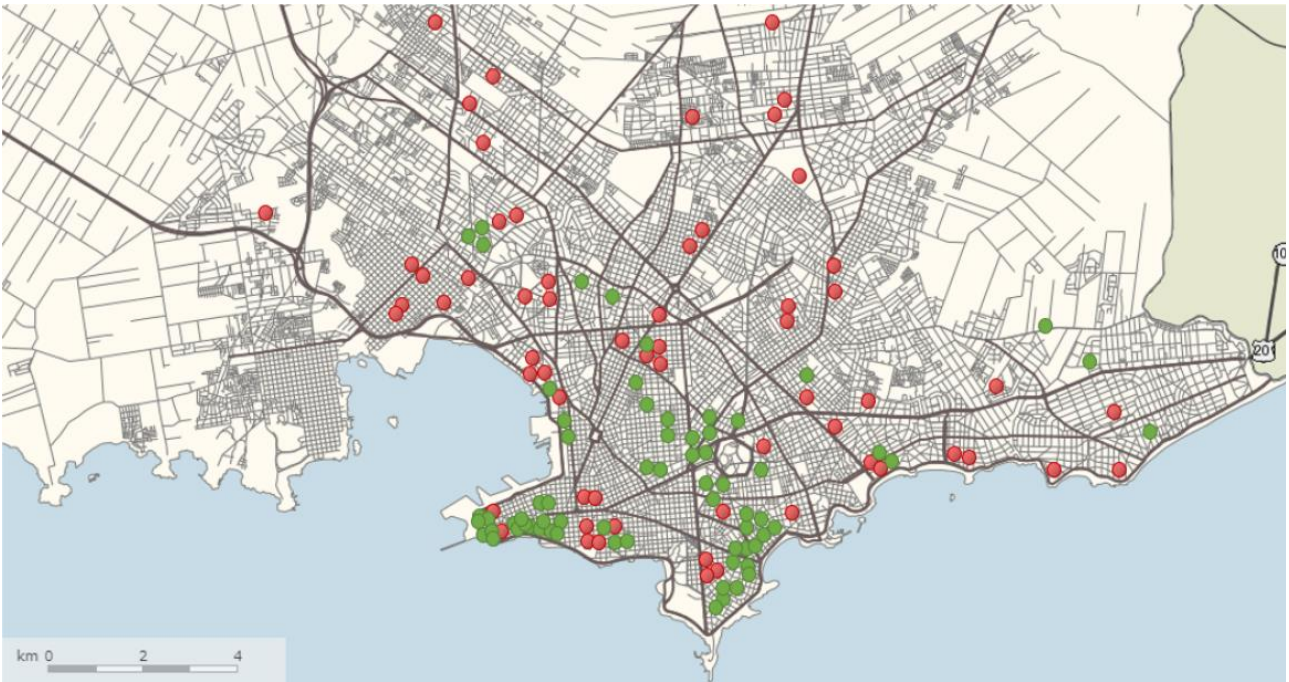*Los costos son del 10/03/2015, podrían aumentar en el correr del año.

Figure A2. Zones in Montevideo to evaluate the impact of the information-only intervention in the synthetic control approach. We gathered information on the number of permits issued in the pre-intervention years (2008-2014). Green dots: zones treated with information. Red dots: control zones.
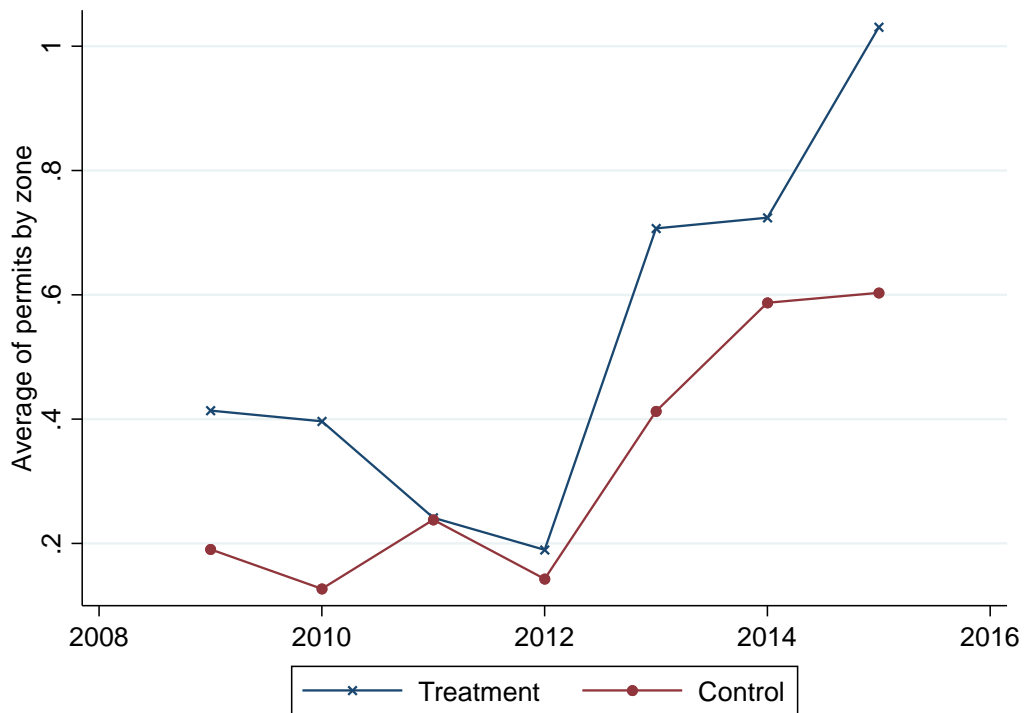Source: Unidad de Registro de Cuidadores de Vehículos de la municipalidad de Montevideo (URCV-IM).

Figure A3: Yearly average of permits issued by zone. Source: Unidad de Registro de Cuidadores de Vehículos de la Municipalidad de Montevideo (URCV-IM).
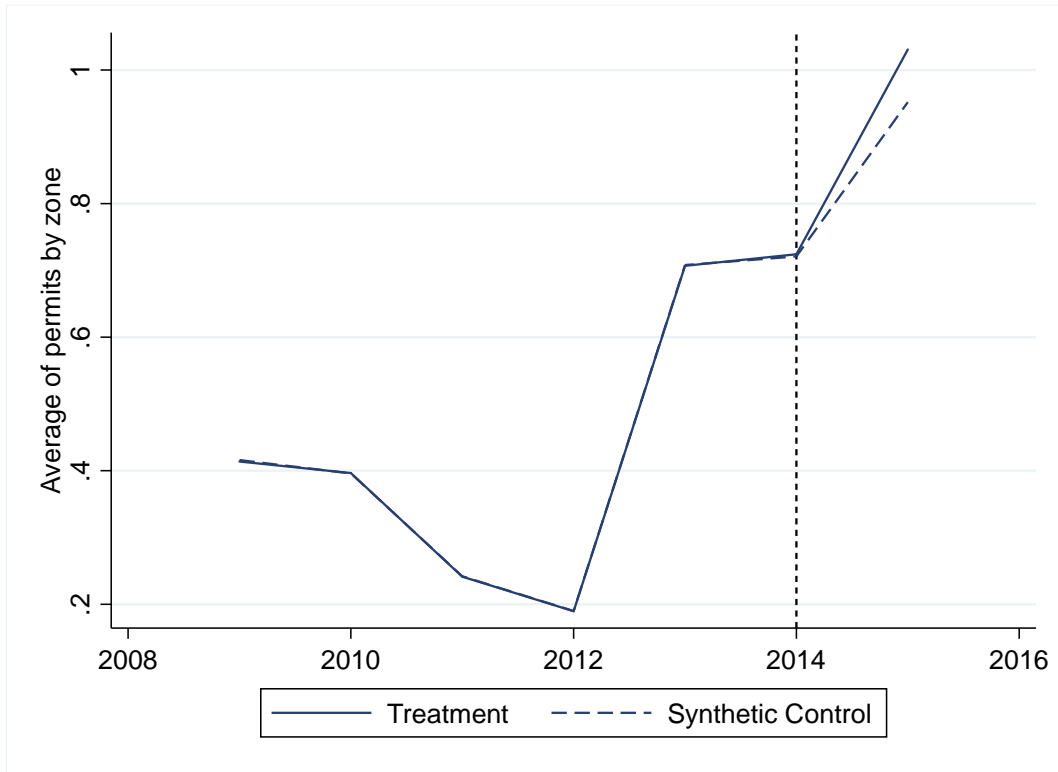
Figure A4: Yearly average of permits issued in the treatment zones and its synthetic control. Source: Own elaboration based on *Unidad de Registro de Cuidadores de Vehículos de la Municipalidad de Montevideo (URCV-IM)*, Household Surveys (INE) and Cabrera et al. (2016).
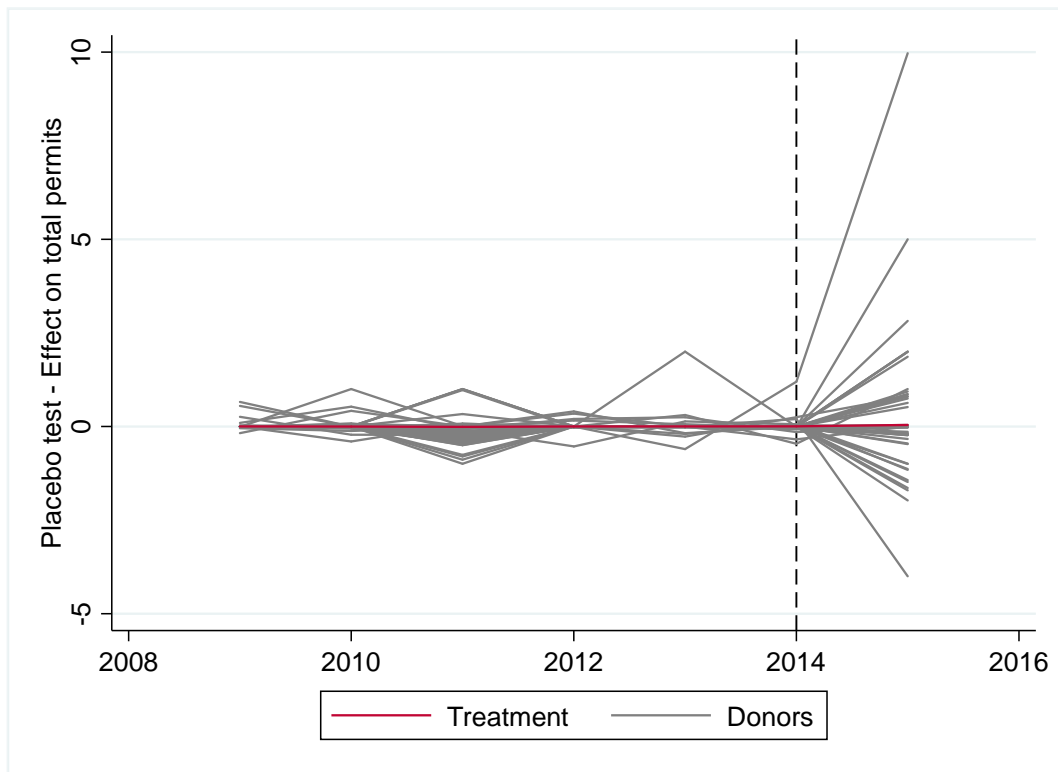
Figure A5. Permutation tests. The gray lines represent the gap associated with each of the runs of the test. That is, the gray lines show the difference in the number of municipality permits between each state in the donor pool and its respective synthetic version. The superimposed bold line denotes the gap estimated for the cells treated with information.
Source: Own elaboration based on *Unidad de Registro de Cuidadores de Vehículos de la Municipalidad de Montevideo (URCV-IM)*