

Connecting Informal Workers to the State: A Field Experiment in the Streets of Montevideo*

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Abstract

Informal workers often must navigate public offices without an employer to guide them. Can personal assistance help them complete a formalization process, and do the initial gains endure? We study a field experiment with 339 street parking attendants in Montevideo, randomly assigning geographic cells to practical information alone, one-to-one assistance from a social worker, or the same assistance with required document costs covered. Personal assistance substantially increased municipal permit acquisition during implementation relative to information alone. Roughly 18 months after assignment, estimated cumulative acquisition remained higher, while the difference in active registry status was smaller and imprecisely estimated. Among participants linked to national records, we find no detectable increase across five indicators spanning transfers, health coverage, and public employment. Personal navigation can accelerate entry into the process it directly targets; maintaining active status and connecting workers to other programs are distinct challenges.

JEL codes: C93, H53, J46, O17.

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1 Introduction

For informal workers without an employer to guide them, obtaining a permit can require documents, appointments, and visits to several offices. Personal assistance may help them complete that process. But a permit acquired during assistance may not remain active once workers must meet recurring requirements on their own. We ask whether one-to-one support increases permit acquisition and what later administrative records reveal about active status.

Street-based own-account work is a common feature of urban labor markets rather than a peculiarity of Montevideo. Across countries, vendors, market traders, vehicle attendants, waste pickers, and other workers earn their livelihoods in public space, often informally and without an employer to navigate registration or social-protection systems on their behalf (Boels, 2014; Ramírez Reynoso and Vanek, 2024). Montevideo’s street parking attendants, known locally as *cuidacoches*, are one example. In our baseline, workers had spent about six years in their current block, and roughly one in three without a permit reported that they could not afford the process.

We study these workers in a field experiment. They help drivers find and watch parking spaces in Montevideo, Uruguay, and receive voluntary payments. A municipal permit was associated with a designated work area and a route into pension contributions. Obtaining it required several documents and interactions with public offices. To keep the experimental conditions geographically separated, the study grouped neighboring street blocks into cells and assigned each cell—rather than each worker—to one of three conditions. All groups received a brochure explaining the process. The first active arm also received intensive, one-to-one assistance from a social worker. The second active arm received the same assistance and direct coverage of the costs of obtaining the required documents.

The experimental estimates therefore compare offers of high-touch navigation, with or without cost coverage, against a comparison group that received practical information and study contact. This design identifies the incremental effect of assistance, not the effect of the brochure on its own.

The original randomization assigned 88 cells: 42 to information only, 23 to personal assistance (T1), and 23 to assistance plus costs (T2). Of these, 67 contain 339 observed participants. The main analysis follows intention to treat, using the original cell assignment.

Our permit outcomes distinguish two concepts at two cuts: whether a worker had ever obtained a permit by the cut, and whether the worker was active in the registry at that cut. At the first administrative cut, assignment to either assistance package increased permit acquisition by 17.1 percentage points, relative to an information-group mean of 9.4 percent, and increased active status by 14.4 points. At the final cut, roughly 18 months after assignment, the acquisition difference was 12.0 points, while the smaller 5.5-point active-status difference was imprecisely estimated.

Because the offers were assigned by cell, our uncertainty calculations follow that design. The evidence is strongest for early entry, while later active status and the incremental contribution of cost coverage remain less precisely estimated.

We also ask whether assistance focused on one municipal process helped participants engage with other parts of the social protection system. Documents obtained and experience with public offices might carry over to later applications. Linked administrative records show no detectable increase across five national indicators spanning transfers, health coverage, and public employment. The municipal permit outcomes remain the paper’s central evidence; these linked records ask how far the response extended.

The paper contributes to two connected literatures. Work on administrative burden and benefit take-up shows that application assistance can matter beyond information (Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019). In France, an in-person meeting that assessed eligibility and helped with applications raised new benefit take-up, whereas an online eligibility simulator did not (Castell et al., 2025). In India, basic and intensive application assistance raised applications beyond information alone, especially among more vulnerable applicants (Gupta, 2026). Conversely, six large EITC field experiments found no detectable take-up response to varied low-touch messages (Linos et al., 2022). A recent meta-analysis similarly finds larger effects on applications than on final receipt and larger effects from reducing compliance demands than from reducing learning demands (Bendtsen, 2026).

The closest formalization evidence documents both strong entry responses and the challenge of sustaining formal status. Reviews find heterogeneous and often modest responses across firm and worker formalization interventions (Bruhn and McKenzie, 2014; Floridi et al., 2020; Jessen and Kluge, 2021). Personalized meetings in Bogotá raised formal entry, but the effect disappeared after a year as firms did not renew their licenses (Galiani et al., 2017). Among informal microentrepreneurs in Rio de Janeiro, in-person assistance increased formalization and subsequent tax compliance, while similar information and assistance delivered virtually did not (Zucco et al., 2020). In Malawi, intensive assistance generated high take-up of business registration, yet registration alone did not improve formal market access or firm performance; gains appeared when assistance was combined with a bank-information session (Campos et al., 2023). Related evidence shows that formalization depends on the institutional package and need not be irreversible (de Mel et al., 2013; Benhassine et al., 2018; Díaz et al., 2018).

To our knowledge, this is the first randomized evaluation of an intervention designed to increase formalization among street-based own-account workers operating in public space. Our setting connects the two literatures through an occupational formalization process rather than firm registration or take-up among existing benefit recipients. Within the same randomized study and municipal record system, we observe both whether a worker ever obtained a permit and whether the worker was active at two administrative cuts. The paper thus adds evidence on the distinction between completing a one-time process and sustaining an administrative relationship with the state among a hard-to-reach population. The experiment is designed to estimate the package effects, rather than distinguish whether continued requirements, low realized benefits, mobility out of the occupation, or other shocks explain the later pattern. The outcome data likewise do not measure the welfare

value of holding the permit. Policies designed to complete initial paperwork should therefore not be evaluated solely by registrations generated during implementation.

The rest of the paper describes the institution and intervention, explains the experimental design and estimands, presents the municipal and secondary social-program results, and discusses their interpretation. The follow-up survey remains descriptive because of response selection and timing.

2 Institutional setting and intervention

2.1 Street parking attendants and the municipal permit

These attendants occupy street locations, signal available spaces, and watch parked vehicles in exchange for voluntary payments from drivers. The occupation has low entry barriers and includes workers with weak attachment to formal employment and public institutions. Social workers leading the fieldwork recruited workers who were understood not to hold a valid municipal permit. They also administered the study’s baseline survey.

The baseline data portray an occupation that was informal but not necessarily transitory. Participants were about 43 years old on average, reported working roughly nine and a half hours per day, and had spent about six years in their current block. Fewer than one in ten were women. A municipal permit could therefore formalize a long-standing activity, yet obtaining it remained an individual administrative task rather than an employer-managed process.

The permit assigned a designated work area, offering municipal protection against competing use of that location, and enabling pension contributions. Applicants were required to obtain a national identity card, a health certificate, and a criminal-record report. The process involved appointments and visits to multiple offices. The direct monetary cost was roughly one or two days of earnings, while the process itself required about ten hours plus waiting between appointments. Field manuals state that the permit itself had no fixed expiration date, but remaining current required permit holders to sign in at the municipality each month and keep their health certificate valid. Municipal records used a last-signature field and a status field; delayed signatures were flagged, and removal was generally recorded after roughly two months without a signature, although practice allowed some flexibility. We therefore distinguish cumulative permit acquisition from active registry status at each administrative snapshot. Acquisition records completion of the initial process. Active status also reflects continued compliance, administrative enforcement, and possible exit from the occupation; it is a registry classification rather than a mechanical measure of monthly compliance.

The baseline responses help explain why even a modest cash cost could matter in this population. Roughly one in three workers without a permit reported that they could not afford the process, while 35 percent of workers who had never held a permit said they were not well informed about how to obtain one. These responses motivated the contrast between practical information, personal navigation, and coverage of document costs; the recurring-signature requirement motivates

measuring active status separately from acquisition.

2.2 Treatments

All participants received a two-page brochure with information about the permit and application process. The brochure was a practical guide that listed the offices, hours, costs, documents, and appointment procedures required at each step. The research team provided it to all participants partly for ethical reasons: once the step-by-step guide had been prepared, another copy cost very little, and the team sought not to withhold potentially useful information from a vulnerable comparison group. The university Ethics Committee approved the protocol, including its treatment of the comparison group.

Participants in T1 were additionally offered personal assistance from a trained social worker. The social worker helped schedule appointments, accompanied the participant across offices, and assisted through municipal registration. Participants in T2 received the same services, while the project directly paid the costs of obtaining required documents. Cost coverage was not a later reimbursement; the social worker paid during the process.

The social workers leading the fieldwork were assigned maps covering designated parts of the city, with cells explicitly labeled C, T1, or T2. They were instructed to survey all eligible workers found in their assigned cells and to adhere strictly to cell assignment. Their contracts combined payment for completed baseline surveys with a success-contingent amount for each T1 or T2 participant who obtained a permit. The research team trained the social workers and monitored implementation during the field period.

These features define two main experimental comparisons: T1 versus information estimates the effect of offering high-touch navigation, while T2 versus information estimates the effect of the bundled package of navigation and cost coverage. The secondary T2–T1 comparison estimates the incremental effect of cost coverage only under the maintained assumption that assistance was otherwise delivered comparably.

Assistance remained available for some pending cases into early 2016. The first municipal cut records permit acquisition through December 2015; the municipality issued no new permits in January or February 2016. We therefore interpret this cut as an interim measure of entry during the broader implementation period, rather than as a common endpoint after all assistance had ended.

3 Experimental design and data

3.1 Geographic assignment and observed sample

The experiment built on fieldwork that the research team had conducted in this market since 2013. Earlier cross-sectional surveys described the workers and mapped their distribution across

Montevideo. This information identified areas with high concentrations of street parking attendants and informed both the geographic design and the hypotheses behind the intervention (Cabrera and Cid, 2017).

The research team used those mappings to construct geographic cells and randomized at the cell rather than the individual level. This design kept attendants sharing a block or working nearby in the same experimental condition, thereby reducing opportunities for the intervention to diffuse across study arms. One-block buffers between cells provided an additional layer of geographic separation. Earlier survey data collected by the team also showed that attendants reported few connections with nearby attendants, further limiting the expected scope for cross-cell diffusion. Randomization was stratified using pretreatment cell characteristics, including planned attendant counts, cell area, and a 2011 census measure of cars per household, and assigned 88 cells to the three study arms.

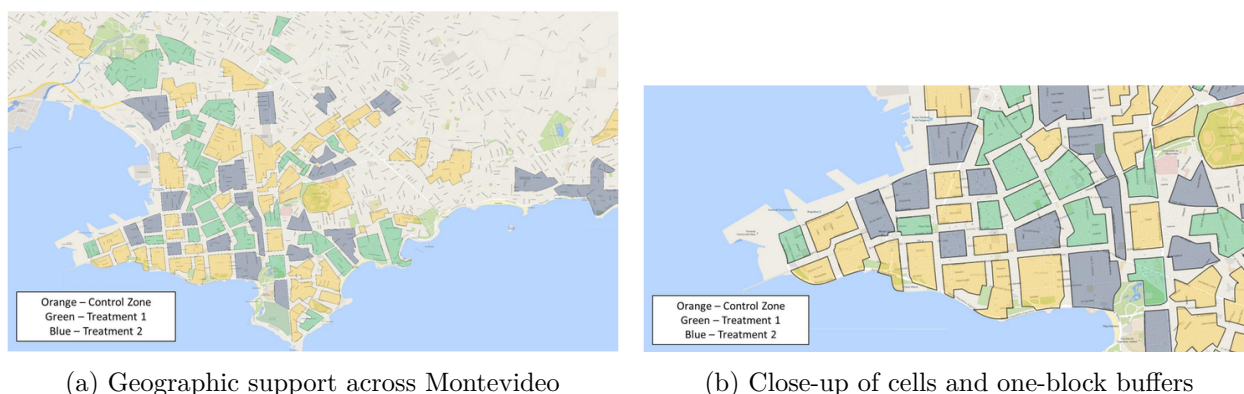
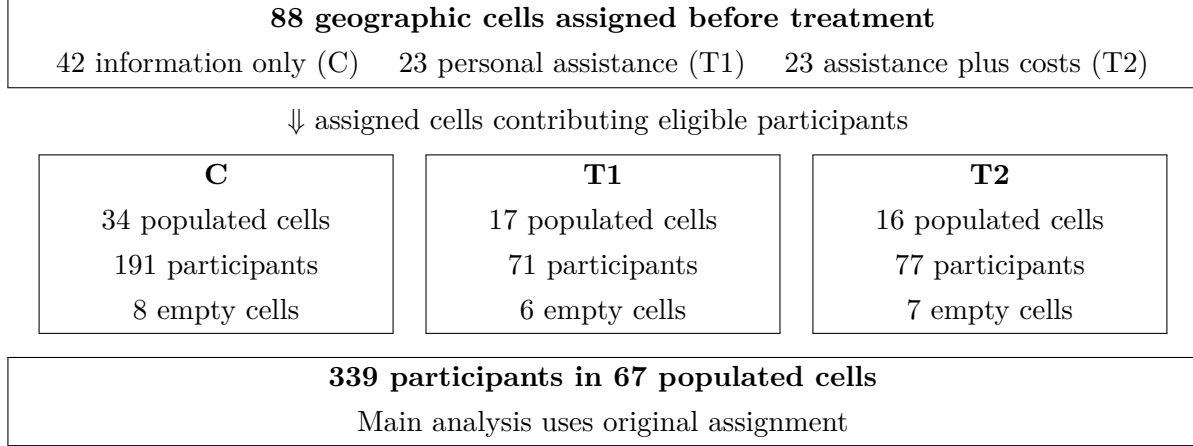


Figure 1: Geographic cells and buffer-zone design

Notes: Panel (a) shows the experimental cells in the areas selected from earlier field mappings; panel (b) illustrates the one-block spaces left between cells to strengthen geographic separation across study arms. The selected areas contained high concentrations of street parking attendants in the earlier surveys (Cabrera and Cid, 2017).

The study files contain no enrolled participant from 21 assigned cells. In total, 67 of the 88 cells contributed 339 workers to the study. The participant-level estimands refer to these observed workers.

Figure 2: Design and observed analysis flow



Notes: Counts come from the original randomization records and participant files. Twenty-one assigned cells have no enrolled participant, but remain part of the original assignment universe. For 13 participants, the operational-arm record differs from original assignment; the analysis uses the original assignment. Participant-level estimands refer to the 339 observed workers.

Table 1 reports a compact baseline summary by original assignment. We show a fixed set of interpretable pretreatment variables rather than selecting controls according to balance tests. Appendix Table 4 reports pairwise standardized differences and randomization p -values. These diagnostics do not determine covariate selection; the primary adjustment uses only the original randomization strata.

Table 1: Baseline characteristics by randomized group

	Information (C)	Assistance (T1)	Assistance + costs (T2)
Female	0.105 (0.307)	0.070 (0.258)	0.065 (0.248)
Age (years)	42.861 (14.537)	43.188 (13.684)	42.316 (13.174)
Years working in current block	5.523 (6.126)	5.646 (7.003)	6.333 (8.038)
Hours worked per day	9.696 (2.624)	9.042 (2.769)	9.688 (2.965)
Previously held a permit	0.267 (0.444)	0.282 (0.453)	0.286 (0.455)
Baseline type index (0–4)	0.764 (0.901)	0.746 (1.105)	0.766 (0.887)
Participants	191	71	77
Randomized cells	42	23	23
Populated cells	34	17	16

Means with standard deviations in parentheses. Age is observed for 332 participants; all other listed variables are observed for 339. The type index is the baseline fieldworker assessment; it is descriptive and not a primary regression control.

3.2 Administrative outcomes, national records, and follow-up survey

We measure four binary permit outcomes for each participant. “Permit obtained by the first cut” records whether the worker had completed permit acquisition by December 2015. “Active at the first cut” records registry status in the corresponding municipal extract. The two corresponding final-cut outcomes, measured roughly 18 months after assignment, capture cumulative acquisition and active status at the later cut. These outcome fields are available for all 339 participant records, avoiding survey attrition for the main analysis. Appendix B reports a complementary family constructed by applying one municipal-register matching rule in every arm among participants with a usable national identification number.

Acquisition is cumulative and can remain equal to one after the permit becomes inactive. Active status is a stock recorded in the municipal system at a date. We use later active registry status to study persistence, while recognizing that it is not a direct measure of individual renewal or retention: it can combine signing behavior, administrative practice, and exit from the occupation.

We also use administrative records from the Ministry of Social Development (MIDES) for five national social-protection indicators. Two concern means-tested transfers: the AFAM–PE child allowance, received by households, and the TUS basic-goods card, which records the worker as a

holder or member of a recipient household. Two concern health coverage: registration with the public provider ASSE and coverage through FONASA, the contributory health insurance scheme associated with formal employment. The fifth is registration in Uruguay Trabaja, a temporary public-employment program; we do not use its participation field. The transfer indicators describe status at the end of 2016; the health indicators record enrollment for at least one month of that year, not service use. For each indicator, we study its 2016 value and control for its 2014 value. We can link these records for 308 of the 339 participants, distributed across 65 populated randomized cells. We present this evidence after the municipal results to examine whether assistance reached beyond the permit process. Appendix C reports linkage and sensitivity to incomplete linkage.

As secondary evidence, a follow-up survey reached 248 of 339 participants. Response rates were 70.7 percent in C, 76.1 percent in T1, and 76.6 percent in T2. Interviews stretched from November 2015 through early April 2016, and item availability varies. We therefore use the survey to describe respondents and potential mechanisms, not as evidence equivalent to the complete municipal outcomes.

4 Estimands and inference

Let $A_c \in \{0, 1, 2\}$ denote the original assignment of cell c to information, assistance, or assistance plus costs, and let $D_c = \mathbf{1}\{A_c \in \{1, 2\}\}$. Our primary specification pools the two assistance packages:

$$Y_{ic} = \alpha + \delta D_c + \lambda_{s(c)} + \varepsilon_{ic}, \quad (1)$$

where $\lambda_{s(c)}$ are indicators for the original randomization strata. The coefficient δ compares assignment to either package offering personal assistance with assignment to information alone. We then separate the two packages using

$$Y_{ic} = \alpha + \beta_1 \mathbf{1}\{A_c = 1\} + \beta_2 \mathbf{1}\{A_c = 2\} + \lambda_{s(c)} + \varepsilon_{ic}. \quad (2)$$

Here β_1 and β_2 are the package-specific ITT effects, while $\beta_2 - \beta_1$ is the secondary comparison of assistance with and without direct cost coverage. All main models include the original randomization-strata indicators and no participant covariates. Standard errors are clustered by cell.

The inference follows the unit of assignment. In addition to cell-clustered standard errors, the replication files report randomization inference based on 5,000 assignments drawn from the original stratified design. All 88 cells remain in that assignment universe, including the 21 without enrolled participants. For the primary pooled contrast, we control the familywise error rate across the four municipal outcomes by applying Holm’s step-down adjustment to the randomization-inference p -values.

For the MIDES outcomes, we retain the pooled original-assignment contrast and add the

program-specific 2014 indicator to the corresponding 2016 regression. We summarize the five outcomes with a standardized index that orients higher values toward greater recorded access. The table reports design-based randomization-inference p -values and Romano–Wolf adjusted p -values within three conceptual domains: means-tested transfers, health coverage, and public employment. Minimum detectable effects are reported to show the precision available in this linked sample. These estimates ask whether help focused on one administrative process also changed recorded access to other programs. Appendix C reports linkage counts and sensitivity to incomplete linkage.

5 Municipal permit results

We present the results in three steps. We first compare assignment to either assistance package with information alone, then show the two packages separately, and finally examine what the experiment can say about persistence and the incremental contribution of covering documentation costs. Table 2 reports one common specification throughout; Figure 3 visualizes the pooled and arm-specific comparisons.

5.1 Administrative entry and active status

At the first cut, the information-group mean for permit acquisition is 9.4 percent. Assignment to either assistance package raised acquisition by 17.1 percentage points. The corresponding estimates are 14.4 points for T1 and 19.3 points for T2. Early active status shows the same pattern for a different outcome: relative to an information-group mean of 6.3 percent, the pooled assistance effect on active registration was 14.4 points. These estimates indicate that the assistance packages substantially accelerated both initial completion and early active status for the observed cohort.

Table 2: Intention-to-treat effects on municipal permit outcomes

	Information mean	Any assistance-C effect	T1-C effect	T2-C effect
Permit obtained by first cut	0.094	0.171*** (0.050)	0.144** (0.068)	0.193*** (0.061)
Active in registry at first cut	0.063	0.144*** (0.039)	0.129** (0.053)	0.156*** (0.048)
Permit obtained by final cut (about 18 months)	0.147	0.120** (0.051)	0.089 (0.071)	0.145** (0.061)
Active in registry at final cut (about 18 months)	0.079	0.055 (0.034)	0.074 (0.053)	0.039 (0.037)

$N = 339$ participants in 67 populated randomized cells. Models use original assignment and original randomization-strata indicators, with no participant-level controls. Parentheses contain standard errors clustered by randomized cell. “Any assistance” pools T1 and T2. The final cut is September 2016, roughly 18 months after assignment. The first cut records permits through December 2015 and is interpreted as an interim measure during the broader implementation period. Conventional significance stars are based on two-sided tests using the reported cell-clustered standard errors and 66 cluster degrees of freedom: *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$. They are not adjusted for multiple outcomes. Across the four pooled outcomes, Holm-adjusted randomization-inference p -values are 0.016, 0.010, 0.084, and 0.194, in table order, based on 5,000 draws from the original stratified assignment.

At the final cut, roughly 18 months after assignment, cumulative acquisition in the information group had risen from 9.4 to 14.7 percent. This change is descriptive: that group received the information brochure and study contact. Assignment to either assistance package increased acquisition by 12.0 points; the package-specific estimates are 8.9 points for T1 and 14.5 points for T2. Active status presents a different picture. Its information-group mean is 7.9 percent, and the pooled effect is a smaller and imprecisely estimated 5.5 points. The package-specific estimates, 7.4 points for T1 and 3.9 points for T2, are also imprecise.

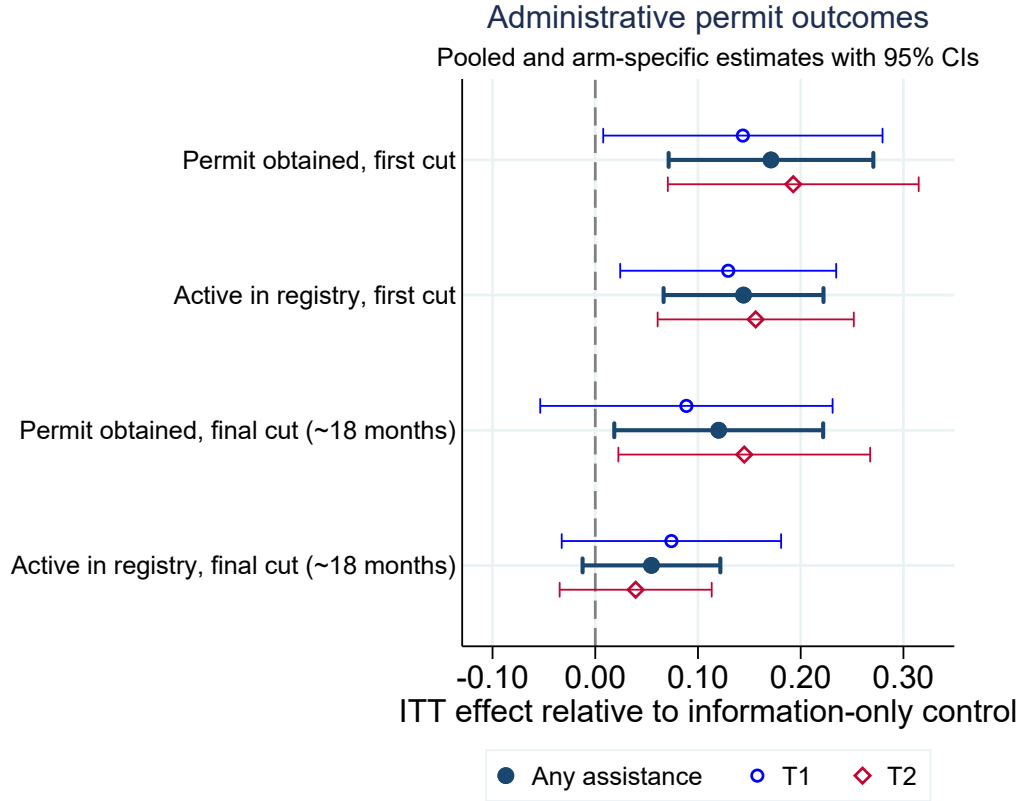


Figure 3: Pooled and arm-specific effects on municipal permit outcomes

Notes: The dark circles show the pooled effect of assignment to either assistance package relative to information alone. The hollow circles and diamonds show T1 and T2 separately relative to information alone; the figure does not display the secondary T2–T1 contrast. Estimates adjust for the original randomization strata and use original assignment. Lines are 95 percent confidence intervals based on standard errors clustered by randomized cell. The final cut is September 2016, roughly 18 months after assignment.

Taken together, the results distinguish generating formal entry from sustaining active status. Assistance produced a large early increase in permit acquisition, while the final difference in active registration was smaller and less precisely estimated. We describe this as an entry–persistence distinction rather than causal “fade-out,” because acquisition and active status are different outcomes.

5.2 What can be learned from T2 versus T1?

The pooled comparison captures the shared offer of personal assistance. Separating T1 and T2 asks the more demanding question of whether direct cost coverage added to that assistance.

The point estimates for T2–T1 are 4.9 points for first-cut acquisition, 2.7 points for first-cut active status, 5.6 points for final-cut acquisition, and –3.5 points for final-cut active status. These differences are not estimated precisely enough to separate modest incremental effects from econom-

ically meaningful ones. We therefore treat T2–T1 as secondary and do not interpret the estimates as showing that monetary costs are unimportant.

5.3 Robustness and precision

The entry–persistence pattern does not depend on a single populated cell. Estimates that remove one cell at a time remain in relatively narrow ranges, and giving each populated cell equal weight produces the same qualitative contrast between early entry and later active status. Inference that follows the original assignment also supports the early pooled effects. After Holm adjustment across the four pooled municipal outcomes, the evidence is strongest for early acquisition and early active status; final acquisition is marginal, and final active status remains imprecise.

Appendix Table 5 applies the same municipal-register matching rule in every arm to the 315 participants with usable identification numbers. The initial acquisition effect is 15.0 percentage points, close to the 17.1-point main estimate; the corresponding estimates for final acquisition and final active status are somewhat larger, at 14.3 and 7.0 points. The conclusion that assistance accelerated entry is therefore robust, while some later magnitudes depend on the measurement rule and sample.

6 Potential spillovers to other public programs

Assistance with a municipal permit may also ease other dealings with the state. Obtaining identity documents and navigating public offices could make later applications easier. Related US evidence documents associations between participation in SNAP and child Medicaid and state policies intended to reduce administrative burden (Schmidt et al., 2024). We use linked national records to examine whether assignment to assistance changed five later indicators spanning transfers, health coverage, and public employment. These outcomes are secondary to the municipal permit that the intervention directly targeted.

Table 3 does not show detectable increases across the five indicators in 2016. The pooled effect on the standardized global index is -0.095 standard deviations, with a 95 percent confidence interval from -0.256 to 0.065 . The individual point estimates are small or negative except for registration in Uruguay Trabaja. The largest estimate in absolute value is a 6.1-percentage-point reduction in ASSE coverage, but it does not survive familywise adjustment. Under the linked-sample specification, the upper confidence limits rule out increases larger than about 2.4 points for the AFAM–PE child allowance, 3.4 points for the TUS basic-goods card, and 4.3 points for FONASA coverage. Sensitivity to incomplete linkage is discussed in Appendix C; the corresponding bounds are wide enough that this precision statement should not be read as attrition-robust.

Table 3: Secondary estimates for national social-protection indicators in 2016

	Information mean	Any assistance–C effect (SE)	RI <i>p</i> -value	Romano–Wolf <i>p</i> -value	MDE
Child allowance (AFAM–PE)	0.169	−0.027 (0.026)	0.359	0.547	0.073
Basic-goods card (TUS)	0.124	−0.021 (0.028)	0.514	0.547	0.079
Public-provider coverage (ASSE)	0.774	−0.061 (0.031)	0.078	0.165	0.089
Contributory coverage (FONASA)	0.339	−0.046 (0.045)	0.368	0.368	0.127
Uruguay Trabaja (registered)	0.051	0.014 (0.024)	0.618	0.618	0.069
Global index (standard deviations)	—	−0.095 (0.080)	0.293	—	0.228

$N = 308$ linked participants in 65 populated randomized cells. Each row compares original assignment to either assistance package with assignment to information alone. Program-specific models control for the corresponding 2014 outcome and the original randomization strata; the index model controls for its 2014 value. Parentheses contain standard errors clustered by randomized cell. RI denotes a two-sided randomization-inference p -value based on 5,000 assignments from the original 88-cell, 17-stratum design. Romano–Wolf p -values adjust within three conceptual domains: AFAM–PE and TUS as means-tested transfers, and ASSE and FONASA as health coverage. Uruguay Trabaja is a public employment program and forms the third, singleton domain. The index is a summary outcome outside those domain adjustments. MDEs are minimum detectable effects at 80 percent power and a 5 percent two-sided significance level, in outcome units. These outcomes and their hierarchy were developed in later analysis and are reported as secondary evidence.

The clear response in permit acquisition did not coincide with detectable increases in these linked national indicators. Direct permit entry and broader engagement with social protection are therefore distinct outcomes for policy evaluation.

7 Discussion and conclusion

The pattern is consistent with personalized assistance reducing navigation, coordination, and follow-through costs that information alone does not remove. Social workers clarified requirements, scheduled appointments, and accompanied participants across offices; these activities could reduce learning, compliance, and psychological costs. T2 additionally reduced direct financial costs. The experiment identifies the package effects, not the contribution of each channel.

The comparison between early entry and later active status is consistent with at least three explanations. First, high-touch assistance may have been temporary: once it ended, participants again faced recurring administrative burdens. Second, participants may have learned that the permit’s realized private value was insufficient to justify continued compliance. Third, occupational mobility, health, relocation, enforcement, or data-record rules may have changed active status independently of administrative burden. The experiment is not designed to separate these mechanisms; doing so would require additional treatment variation and more complete longitudinal records.

These possible mechanisms reinforce the policy distinction between entry and later active status. A program designed to complete initial paperwork may generate many permits during implementation. If policy value depends on continued formal status, evaluation must also measure whether

participants remain active after intensive help recedes. This mirrors evidence from benefit recertification that repeated procedures can generate coverage losses ([Homonoff and Somerville, 2021](#); [Myerson et al., 2025](#)), the temporary formal-entry response to personalized assistance in Bogotá ([Galiani et al., 2017](#)), and broader evidence that formalization can be gradual and reversible ([Díaz et al., 2018](#)).

The estimates apply directly to 339 mostly male street parking attendants in Montevideo and measure permit acquisition and active registry status. The geographic cells and buffers were designed to keep nearby attendants in the same study arm and reduce contact across arms, although some cross-cell contact may have remained.

Assignment to high-touch personal assistance substantially increased early municipal permit acquisition relative to providing information alone. The two assistance packages produced similarly large early effects, although the experiment does not precisely isolate the incremental contribution of covering document costs. Roughly 18 months after assignment, the difference in active registry status was smaller and imprecisely estimated. Linked records provide little evidence of an increase in five other national social-protection indicators among the participants who could be linked; incomplete linkage limits that secondary conclusion. Personal navigation can accelerate a targeted entry process, while maintaining active status and building a broader relationship with the state remain distinct tasks for policy design and evaluation.

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A Baseline balance

Table 4: Pairwise baseline balance: standardized differences and randomization p -values

	T1-C	T2-C	T2-T1
<i>Panel A. Randomized-cell characteristics</i>			
Planned attendants in cell	0.168 (0.230)	0.021 (0.879)	−0.132 (0.411)
Cell area	−0.072 (0.696)	0.110 (0.548)	0.185 (0.311)
Cars per household	−0.101 (0.643)	0.083 (0.709)	0.174 (0.490)
Joint balance p -value	0.714	0.891	0.545
<i>Panel B. Observed-participant characteristics</i>			
Female	−0.121 (0.394)	−0.143 (0.322)	−0.022 (0.887)
Age (years)	0.023 (0.868)	−0.039 (0.781)	−0.065 (0.692)
Years working in current block	0.019 (0.894)	0.113 (0.419)	0.091 (0.578)
Hours worked per day	−0.243 (0.131)	−0.003 (0.986)	0.225 (0.223)
Previously held a permit	0.033 (0.819)	0.042 (0.772)	0.009 (0.957)
Baseline type index (0–4)	−0.018 (0.903)	0.002 (0.989)	0.020 (0.908)
Joint balance p -value	0.711	0.942	0.871

Each non-joint entry reports a standardized mean difference followed in parentheses by a two-sided randomization-inference p -value. The randomization draws preserve the original arm totals within each of the 17 strata and retain all 88 cells; 5,000 assignments are used. The joint test statistic is the sum of squared standardized differences within each panel. Panel A uses all randomized cells and the three pretreatment cell characteristics, which did not all enter the stratification of every cell. Panel B uses the participant variables and source sample in Table 1. The diagnostics describe the realized allocation and do not determine covariate selection.

B Uniform municipal-register measurement

The main analysis uses four participant-level permit outcomes available for all 339 participants. As a complementary measurement exercise, this appendix constructs four parallel outcomes directly from municipal registers, using the same matching rule in every experimental arm. The exercise requires a usable national identification number. Twenty-four participants lack one—16 in C, seven in T1, and one in T2—leaving 315 participants in 65 populated cells. Identification-number availability is 3.5 percentage points higher in the pooled assistance arms, although this difference is imprecise (clustered $p = 0.320$). The alternative estimates thus differ from the main ones in both measurement and sample. A full-sample version of the uniform rule would code these 24 unmatched participants as zero; Table 5 instead excludes them and does not recode the historical main outcomes.

For initial acquisition, the uniform rule counts a worker as newly obtaining a permit if the identification number appears in the November 2015 municipal list but not as a valid permit in the March 2015 register, where PROVISORIO counts as valid. At the November cut, a record marked

BAJA in either ESTADO or UF is inactive; AF alone records a delayed signature, not a BAJA. The corresponding final-cut measure uses the September 2016 register, with active status recorded as ESTADO=ALTA. Social workers recruited participants who were understood not to hold a valid permit, but 20 participants appear with a valid permit in the March register. The information available during field recruitment may not have matched the later record linkage; we therefore do not interpret these cases as protocol violations. The uniform rule does not count their later appearance as new acquisition.

Table 5 shows a similar initial-entry effect under the uniform register rule. Assignment to either assistance package increases initial acquisition by 15.0 percentage points and initial active status by 14.4 points. The alternative estimates for final acquisition and final active status are 14.3 and 7.0 points, respectively. Because the uniform family excludes participants without usable identification numbers and changes both the measurement rule and the sample, it is reported as complementary evidence rather than replacing the full-sample family.

Table 5: Alternative outcomes under a uniform municipal-register rule

	Information mean	Any assistance–C effect	RI <i>p</i> -value	Holm-adjusted <i>p</i> -value
Permit obtained by first cut	0.074	0.150 (0.049)	0.009	0.028
Active in registry at first cut	0.046	0.144 (0.039)	0.001	0.005
Permit obtained by final cut (about 18 months)	0.091	0.143 (0.053)	0.016	0.032
Active in registry at final cut (about 18 months)	0.040	0.070 (0.029)	0.031	0.032

$N = 315$ participants with usable national identification numbers in 65 populated randomized cells. Models use original assignment and original randomization-strata indicators, with no participant-level controls. Parentheses contain standard errors clustered by randomized cell. “Any assistance” pools T1 and T2. Randomization-inference *p*-values use the absolute pooled-treatment coefficient as the statistic and 5,000 assignments drawn from the original stratified 88-cell design, including empty cells. The final column applies Holm’s step-down adjustment across the four outcomes.

C Linkage to national records and sensitivity

Among 339 participants, 308 could be linked to annual national-program records: 177 of 191 in the information arm, 62 of 71 in T1, and 69 of 77 in T2. The remaining 31 have no linked national-program outcomes. An assignment-adjusted comparison estimates that non-linkage was

4.5 percentage points higher in the pooled assistance arms; this difference is imprecise (cell-clustered $p = 0.226$). The municipal permit effect remains strong in the linked sample: assistance increased first-cut acquisition by 18.1 percentage points from an information-group mean of 10.2 percent ($p = 0.005$). The social-program result therefore is not explained by an absent permit response in this subsample.

The estimates in Table 3 assume that linkage does not select participants differentially by their potential social-program outcomes once assignment and the specified baseline outcome are taken into account. To assess sensitivity, we also apply Lee-type trimming under a monotonic-selection assumption. For the 2016 global index, the resulting point-estimate bounds are $[-0.137, 0.057]$ standard deviations. A randomized-cell bootstrap that incorporates sampling uncertainty produces the much wider interval $[-0.398, 0.341]$. Program-specific bootstrap intervals are similarly wide. These calculations do not overturn the absence of a positive point estimate, but they show that incomplete linkage materially weakens claims about small effects. We therefore use the linked records to ask whether there is evidence of a broader administrative bridge, not to establish that all spillovers are zero.