Recognizing a good deal: short-term subsidies and the dynamics of public service use

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Abstract

I study the longer-run dynamics of household use of a public service in response to short-term subsidies. I exploit spatial variation in exposure to subsidies that induced households to use a publicly-provided matching platform for sanitation services in Dakar, Senegal. Using platform administrative data, I show that neighborhoods exposed to short-term subsidies are significantly more likely to use the platform after subsidies end, but this effect declines gradually to zero over time. Following a subsequent city-wide subsidy campaign two years later, increased use re-emerges in previously-subsidized neighborhoods before declining again. The pattern of decline and re-emergence shows that short-term subsidies can have persistent effects, but sustaining these effects may require repeated intervention.

Keywords: Technology adoption, environmental quality, urban sanitation, subsidies, development, public services, fecal sludge management
JEL codes: H40, O18, Q53, R22

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1 Introduction

Short-term subsidies are a common tool in environmental and development policy to spark demand for a new technology, internalize externalities, and generate technological innovation. Subsidies could have positive longer-run impacts if they facilitate learning (Dupas, 2014; Bensch and Peters, 2020; Carter et al., 2021; Meriggi et al., 2021) or negative impacts if they anchor households to a reference price (Kőszegi and Rabin, 2006; Fischer et al., 2019). Short-term subsidies could also generate longer-run impacts when network externalities are present in two-sided markets, or when there are increasing health benefits (Jullien et al., 2021; Springel, 2021; Deutschmann et al., 2024b). The longer-run impacts of short-term interventions can have dramatic implications for assessing their cost-effectiveness (Allcott and Rogers, 2014; Baird et al., 2016).

In this paper, I ask whether exposure to short-term subsidies increases longer-run household use of a platform for sanitation services, both after subsidies end and when new subsidies become available. Externalities and features of decentralized markets for sanitation services, including high search costs and supplier market power, impact household sanitation choices (Kresch et al., 2020; Augsburg et al., 2024; Houde et al., 2024). Platforms to centralize the market and match households to service providers have the potential to address frictions and improve safe management of fecal sludge, including reducing search costs and undercutting collusive behavior (Gehrig, 1993; Bakos, 1997; Brown and Goolsbee, 2002; Cramer and Krueger, 2016; Farronato and Fradkin, 2018; Gaineddenova, 2022; Dillon et al., 2024; Houde et al., 2024). Platforms can also be an effective tool for delivering targeted subsidies and internalizing some of the externalities inherent in household sanitation choices (Johnson and Lipscomb, 2021).[1] Governments, social enterprises, and non-profit organizations have launched platforms for sanitation services in at least ten countries (Markandya, 2019; GSMA, 2020; CWIS, 2021; Johnson and Lipscomb, 2021; Pit Vidura, 2021; USAID, 2021).[2]

I study this question in Dakar, Senegal, where in 2014 the government launched a platform and call center to connect households with providers of mechanized desludging services using auctions (Deutschmann et al., 2024a). Management of fecal sludge is a key environmental and public health challenge for many cities in low- and middle-income countries, including Dakar. Rapid urbanization means many households lack access to the sewage network and must instead use on-site sanitation systems, such as septic pits or tanks, which fill and must be emptied. Unsafe management of fecal sludge degrades local water quality, causes substantial economic losses, and increases diarrheal disease and death, particularly among children (Garg et al., 2018; UN Environmental Programme, 2020;

[1]Once established, platforms can also be useful for delivering other services and targeted information (Pakhtigian et al., 2024).

[2]Platforms are also being increasingly used for other public services like solid waste disposal in low-income countries (PROMOGED, 2022; Sowe, 2024).

Cameron et al., 2022). Safe management of on-site sanitation systems typically requires hiring a mechanized truck to "desludge" the pit, but in many neighborhoods of Dakar less than half of households relied on mechanized desludging in 2013 (Houde et al., 2024). Instead, households often turn to cheaper "manual" alternatives which result in sludge being disposed in streets, courtyards, or stormwater canals.

I exploit quasi-random variation in neighborhood-level exposure to short-term subsidies which induced households to use the platform. Starting in mid 2014, Lipscomb and Schechter (2018) conducted an experiment which offered subsidies to about 3700 randomly-selected households in about 400 neighborhoods for the purchase of a mechanized desludging service. These experimental subsidies were available for one year and allowed households to purchase two mechanized desludgings of their septic pit for a fixed price of about $34 USD each, roughly 66% of the average market price. To access the experimental subsidies, households were required to call in and use the matching platform.

To identify the impact of these short-term experimental subsidies on later use of the platform, I rely on a key feature of the sampling strategy of Lipscomb and Schechter (2018), which selected about 800 grid points from a 200 x 200 meter grid in residential areas without sewer access.[3] As a rule, every second grid point was selected for possible inclusion in the experiment. This "checkerboard" sampling results in a set of about 400 non-treated neighborhoods which are tightly interspersed with the roughly 400 treated grid point neighborhoods from the experiment. Households in both types of neighborhoods received a baseline survey, and the two sets of neighborhoods are comparable on average observable characteristics at baseline. Both types of neighborhoods had equal access to the platform. I compare outcomes in the set of neighborhoods selected to receive experimental subsidies with the tightly interspersed set of non-treated neighborhoods who received no such subsidies. I construct take-up at the neighborhood level using usage data from the platform.

I first show that the experimental subsidies were effective at increasing contemporaneous household adoption of the platform in treated neighborhoods relative to non-treated neighborhoods, as measured by the number of unique service requests submitted by households. In the first six months of the subsidized period, which coincided with the public launch and scale-up of the platform, treated neighborhoods had 0.21 more service requests than non-treated neighborhoods, a 23 percent increase. In the subsequent six months, usage in non-treated neighborhoods declined substantially (from 0.93 requests per neighborhood to 0.31), but there are still about 0.3 more requests from treated neighborhoods than from non-treated neighborhoods, a 96 percent increase. This finding uses a distinct empirical strategy and additional sample to Lipscomb and Schechter (2018), who look at the impact of subsidies within treated neighborhoods and find that a randomized offer of a subsidized desludging increased use of mechanized desludging in the following year by 0.03 p.p (8

[3] Throughout the paper, unless otherwise specified I use the term neighborhood to refer to the area defined by a circle with a radius of 100 meters centered at each grid point. 100 meters exactly bisects the distance to the next grid point.

percent) relative to a baseline average of 0.32.

Second, I explore the dynamics of platform use after the experimental subsidies were no longer available. In the six months after subsidy availability ended, previously-treated neighborhoods had 0.59 more requests than non-treated neighborhoods, a 139 percent increase. Although overall requests declined over time, usage of the platform remained significantly higher in previously-treated neighborhoods until 18 months after the end of the experiment, at which point usage became statistically indistinguishable from non-treated neighborhoods. The decline in treatment effects over time suggests that behavior changes may not persist indefinitely absent additional intervention or advertising.

Third, I test whether past experience with subsidies and the platform causes households in previously-treated neighborhoods to respond differently to a new round of subsidies. In 2017, two years after the experimental subsidies ended, the government ran a major city-wide subsidy and advertising campaign in Dakar intended to increase adoption of improved sanitation services.[4] As before, accessing these subsidies required calling the call center and using the platform to find a service provider. I find that previously-treated neighborhoods had 1.1 (73 percent) more requests during the city-wide subsidy campaign relative to non-treated neighborhoods.[5] Following the conclusion of the city-wide subsidy campaign, usage dropped in all neighborhoods but again continued at relatively higher levels in previously-treated neighborhoods for the first six months before fading out.

I explore mechanisms and find suggestive evidence that the effects of the experimental subsidy intervention were not limited to direct recipients. In the twelve months following the conclusion of the experiment, previously-treated neighborhoods had more request volume than non-treated neighborhoods from users who had not used the platform during the period of the experiment. This could suggest that information about the platform is shared within neighborhoods, consistent with findings from Deutschmann et al. (2024b) that referrals are an important channel for sourcing information about sanitation service providers.

I additionally look at desludgings successfully sourced through the platform, which are a subset of household requests. I find that the experimental subsidy intervention leads to more sanitation services successfully sourced through the platform in the subsequent three years, both when the prices were unsubsidized and during the city-wide subsidy campaign, but the effects are generally small in absolute terms. At its peak, during the city-wide subsidy campaign, I estimate that previously-treated neighborhoods saw about one additional desludging sourced through the platform, or a 72 percent increase relative to about 1.4 desludgings per non-treated neighborhood.

A limitation of this paper is that I do not observe household sanitation behavior outside the platform in cases where a household request did not lead to a desludging sourced through

[4]The campaign offered households anywhere in the city mechanized desludgings for a fixed, subsidized price of about $33 USD, one dollar cheaper than the price previously offered during the experiment.

[5]This finding is consistent with recent work showing prior exposure to an intervention implemented by an NGO increases subsequent uptake of a similar intervention (Usmani et al., 2024).

the platform. Household service requests that do not lead to desludgings may still increase welfare if they allow households to negotiate better prices outside the market or discipline service providers to behave more competitively (Deutschmann et al., 2024a; Rudder and Dillon, 2024). There may also be additional benefits over time for households that my data would not capture, if using the platform once allows households to identify a new high-quality service provider with whom they can contract directly in the future.

This paper contributes to the literature on the fading impacts of short-term interventions. This has been documented most clearly in the literature on habit formation, with Caro-Burnett et al. (2021) finding that subsidies induce short-term changes in adoption of improved toilets, but behavior changes decay over time and become statistically indistinguishable from control-group participants. Hussam et al. (2022) similarly find that financial incentives increase handwashing, but the effects decay over time. I demonstrate a similar pattern in the adoption of a platform for sanitation services which, in contrast to toilet use or handwashing, is not something likely to develop a habit given that households typically only need to desludge 1-2 times per year.

I also contribute to the literature studying consumer interactions with platforms in two-sided markets (Rysman, 2009). The platform I study reduces time costs for households seeking to source mechanized desludging services, and during subsidized periods also offered highly discounted prices. Past work has shown that consumers are responsive to both prices and service wait times in ride-sharing platforms (Goldszmidt et al., 2020; List, 2021). Intermediation in decentralized markets with high search costs, like residential solar or urban waste management, can improve welfare for both buyers and sellers (Salz, 2022; Dorsey, 2024). I study a context where services are needed infrequently but regularly by households, in contrast to ride sharing or urban waste markets where users may participate in markets frequently, or residential solar sales where users may only participate once. Short-term subsidies may have different implications when the need for a service is infrequent, if households have sticky reference points that adjust either over time or based on recent market experiences (Kőszegi and Rabin, 2006; Thakral and Tô, 2021), or if price variation causes persistent changes in preferences (Severen and Van Benthem, 2022). I contribute by showing that consumer use of a platform persists after short-term subsidies end but declines over time, and that repeated discounts can provide a spark to re-engage households.

I additionally contribute to a broader literature on short-term subsidies and longer-run effects in environmental quality, health, and sanitation. Households generally exhibit low willingness to pay at market prices for mechanized desludging and latrines (Jenkins et al., 2015; Ben Yishay et al., 2017; Burt et al., 2019; Peletz et al., 2020; Armand et al., 2023). Subsidies can increase adoption and change household behavior, but less is known about the conditions under which these changes persist (Pakhtigian et al., 2022). My results suggest that the longer-run impacts of the subsidies of Lipscomb and Schechter (2018) are potentially several times larger than the direct, short-term effects, and that effects persist but not indefinitely absent additional intervention.

The rest of the paper is organized as follows. In Section 2, I describe the context of the paper and the administrative data I use. In Section 3 I describe the empirical strategy, and present results of that strategy in Section 4. Section 5 concludes.

2 Background

2.1 Policy context and timeline

In this paper, I study the interaction of three policy elements of the Dakar urban sanitation program of the National Sanitation Office of Senegal (ONAS). The first is the launch of a call center and matching platform for sanitation services, which ONAS provided as a public service. The second is an experiment which subsidized desludgings for households and induced them to call the call center and use the platform. The third is a city-wide subsidy campaign which again induced households to call the call center. Figure 1 provides a visual guide to the timeline of these activities.

A primary goal of the ONAS program was to increase the use of mechanized septic pit desludging. In 2013, about 75% of households in Dakar did not have a sewer connection and instead used toilets which drained into septic pits (Sene, 2017). These pits typically need to be emptied 1-2 times per year. Households can either choose to use a mechanized desludging provider, who pumps sludge from the pit into a vacuum truck and disposes of it off-site, or they can perform a manual desludging. Manual desludgings may be done by a family member or a *baay pell* (Wolof for "father shovel") who is paid for the service. In either case manual desludgings typically result in fecal waste being dumped in the street in front of a house or a nearby empty lot, with important health implications for children especially (Kresch et al., 2020; Johnson and Lipscomb, 2021).

In early 2014, ONAS scaled up a call center and platform designed to match households to mechanized desludging service providers (Deutschmann et al., 2024a). The platform was available as a public service for households, and operated continuously from 2014 to 2018, and again in 2019.[6] The platform used just-in-time auctions to match households and service providers (Houde et al., 2024). The mechanized desludging services offered through the platform did not differ in any substantive way from those available elsewhere in the market.[7]

Starting in 2014, researchers conducted a series of experiments in partnership with ONAS to study household demand for sanitation services (Lipscomb and Schechter, 2018;

[6] Due to a change in overall government sanitation strategy, ONAS elected to close the platform in 2018 and transfer its management to a social enterprise. This transition began in April 2018 and the service became operational again in February 2019.

[7] In companion work, Deutschmann et al. (2024a) find that the platform effectively offered lower prices than the wider market in some areas of the city, whereas in others it was offering prices similar to market prices available elsewhere. That variation in prices offered by the system covers areas much larger than the neighborhoods of interest in this paper.

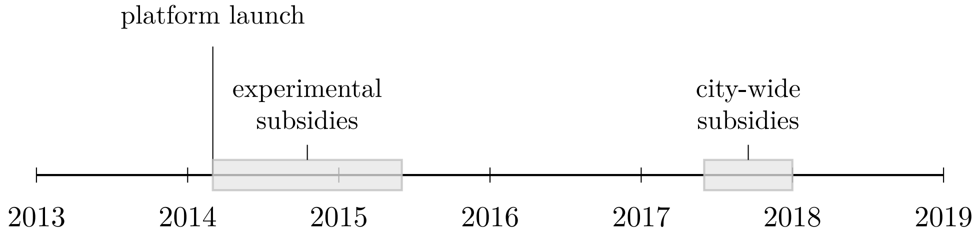


Figure 1: Timeline of key dates

Deutschmann et al., 2024b). These experiments included offering households fixed, subsidized prices to encourage adoption of mechanized desludging. About half of randomly-selected households were offered a subsidized price of about $34 USD, which represented a significant discount over the baseline average market price of about $50 USD.[8] In order to use the subsidies, households needed to call the call center and request a desludging. Each household had two subsidized desludgings available to use in a twelve month period starting from the date they were surveyed. The availability of these experimental subsidies ended by mid-2015.

Two years after the conclusion of the experiments, in 2017, ONAS launched an extensive campaign to boost mechanized desludgings. The compaign offered households a fixed, subsidized price almost exactly equal to the "high" subsidy previously offered during the experimental phase of the project.[9] This new subsidy was available city-wide and advertised extensively, including with billboards, radio spots, and promoted posts on social media platforms (Figure A.1). As with the experiment, to access the subsidy households needed to call the call center to request a desludging.

2.2 Data

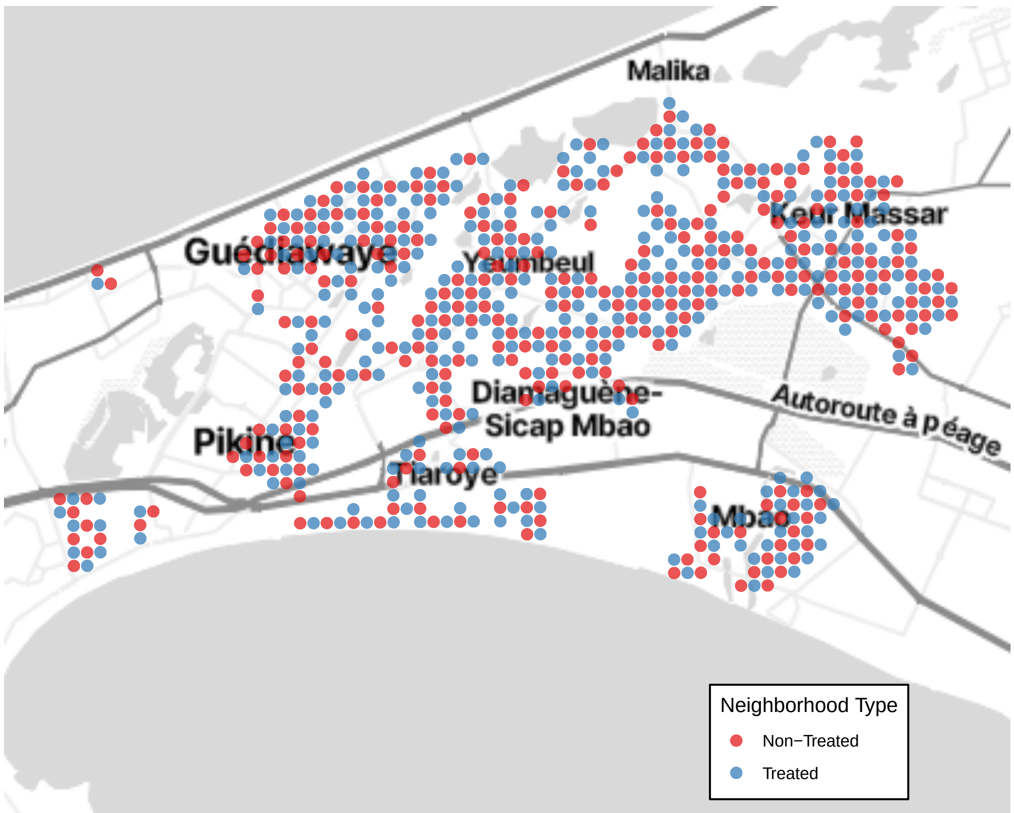
Key for this paper is the sampling strategy used to select subsidized neighborhoods for Lipscomb and Schechter (2018). The field team first mapped a set of grid points across the city, placed 200 meters apart, and assigned every other grid point for possible inclusion in the experimental sample (I refer to these as "treated" grid points) in a checkerboard pattern. The remaining grid points were held out for inclusion in a companion survey without any associated exposure to experimental subsidies. Both surveys used similar criteria to exclude grid point neighborhoods in areas connected to the sewage network, highly flood-prone areas (in which household sanitation behavior is necessarily quite different), non-residential

[8]As Deutschmann et al. (2024b) describe, the other half of sampled households were offered subsidized prices of about $48 USD, representing a small discount over the average price. Take-up at this price was much lower, although some households did call to redeem these discounts. In what follows, I refer simply to the experimental subsidies without distinguishing the prices. On average, in a given neighborhood five households received a "high" subsidy offer and five households received a "low" subsidy offer. In most neighborhoods, the number of high subsidy offers was between four and six.

[9]The "high" subsidy of Lipscomb and Schechter (2018) offered households a price of 17,000 CFA, whereas the 2017 subsidy campaign provided a price of 16,500 CFA.

areas, and one small region of the city in which sampling was conducted differently for a pilot experiment.[10] For my analysis, I additionally drop grid points in areas which were not included in both sets of surveys.[11] Figure 2 illustrates the retained set of neighborhoods. I rely on the locations of these grid points and the spatial extent of the sampling strategy for the experiment to determine whether a particular area of the city was exposed to experimental subsidies.

Figure 2: Location of sampled grid points in Dakar



I use administrative data covering the universe of household service requests made to the platform, and especially the location of those requests. When a household called to use the platform, the operator would input the phone number and search for a matching profile. If the phone number the household used was already in the platform's database, the operator would confirm the caller's details (namely their name and location) before

[10] The sampling criteria for neighborhoods was similar across the two surveys, but the sampling criteria for households within each grid-point neighborhood was slightly different, and surveys were conducted about one year apart. Despite the differences in household sampling and survey timing, grid point neighborhoods appear broadly similar on observable characteristics from the two sets of baseline surveys, with no statistically significant difference in baseline use of mechanized desludging, baseline average price for a mechanized desludging, household size, or education of the household head. There are also no significant differences in population density using WorldPop or Meta estimates (WorldPop and CIESIN, 2020; Facebook Connectivity Lab, 2024)

[11] Results are robust to including these grid points in the analysis (see Tables A.7 and A.8).

launching the service request. If the phone number did not appear, the operator would first ask if an alternative phone number may been given by a member of the household in the past. If no match was found, the operator would create a new profile and collect information about the household, including its location. Dakar does not have a popularly-used address system, so operators used a database of about 2000 landmarks to geo-locate households. These landmarks included "primary" landmarks such as gas stations, schools, and government buildings, as well as "secondary" landmarks such as a corner store. For each household, operators would ask about a primary and secondary landmark. The names of these landmarks were then used by the platform to communicate household locations to sanitation service providers.

Before launching the platform, in 2012 the NGO Water and Sanitation for Africa conducted a baseline census of about 65,000 households in areas of the city not connected to the sewage network. This baseline census formed the initial client database for the platform. About one third of the platform users I observe match directly to this baseline census based on a phone number they provided, meaning I observe precise coordinates that were taken at their doorstep. I also observe landmark information for these users. The remaining two thirds of users only have coordinates associated with at least one nearby landmark. Where possible, I use coordinates of the "secondary landmark" which should be closer to the household (this covers about 10% of users). In remaining cases, I use coordinates of the "primary landmark" if no secondary landmark is available.[12] The median distance from a household with precise coordinates to the nearest landmark is 93 meters. There are no significant differences in the fraction of users I observe with precise coordinates across treated and non-treated grid point neighborhoods.

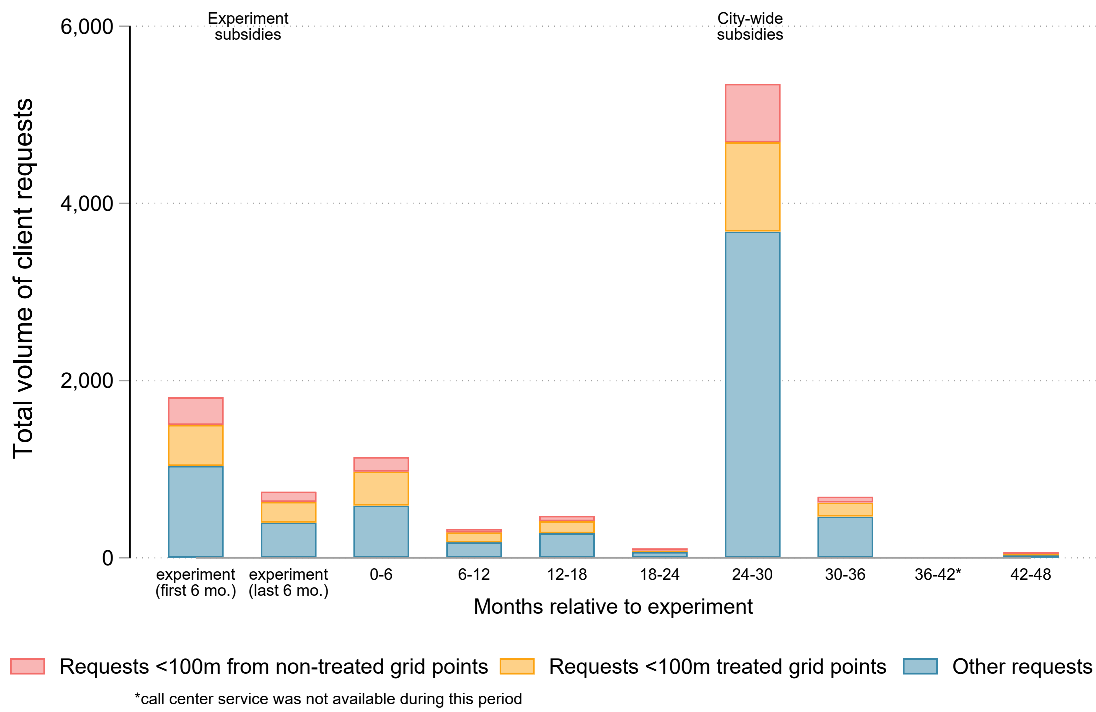
In my primary analysis I include all household requests that fall within 100 meters of a grid point, whether or not the household location is precise or defined by the nearest landmark. This radius exactly bisects the distance to the next grid point.[13] In Appendix A I demonstrate how my results compare if I consider different distance cutoffs. I additionally conduct robustness checks in which I restrict my analysis only to the subset of households with precise coordinates. Figure 3 shows the total volume of requests handled by the platform in each six-month period, and highlights the number of requests that came from treated or non-treated grid point neighborhoods, as well as any other requests from the city that came from households outside these neighborhoods. The figure shows that these neighborhoods represented a substantial fraction of total platform use throughout the period of study. Figure A.3 plots the distribution of the outcome at the neighborhood-period level and shows that a majority of neighborhood-periods have zero household requests.

I additionally conduct analysis in which I look separately at the volume of "post-subsidy new" and "subsidy period repeat" platform users. I define a service request to be from a

[12]In cases when I observe precise coordinates as well as a primary and secondary landmark, the median distance to secondary landmark is about half the median distance to primary landmark.

[13]Figure A.2 demonstrates visually how I attribute households in the data to nearby grid points.

Figure 3: Total volume of household requests per six-month period



This figure shows the total volume of household requests for desludgings made through the matching platform. It includes both successful desludgings and requests where the service was not completed, either because the client declined the offered price or the trucker was unable to complete the job. The red and yellow bars show the number of requests made from households near treated and non-treated grid points. The blue bar shows all other requests that came from other areas of the city.

new user if the associated client ID did not have a recorded service request during the subsidy period. Other requests, from clients who had previously used the platform during the subsidy period, are classified as repeat users. There is some risk that this strategy over-identifies new users, if a different household member calls to use the platform, or if a previous user changes their phone number. Operators were trained to ask about both of these scenarios before creating a new client profile, but nevertheless it is possible that there is some misclassification of repeat users as new users. Because the treated and non-treated areas are so similar and spatially interspersed, I have no reason to believe users in different areas would change phone numbers at different rates, nor that household separations or mobility would occur at different rates in treated or non-treated neighborhoods.

3 Empirical Strategy

In this section, I discuss the empirical strategy and identification assumptions I use to study the dynamics of adoption of the platform. I conduct empirical analysis at the grid point neighborhood level using administrative data on all household requests for mechanized desludgings recorded in the matching platform. I assign household requests to the closest grid point using their location information, as described above in Section 2, using a radius of 100 meters in my primary analysis. I construct a panel at the grid point level for each six-month period, such that time periods t align with the two subsidy campaigns and include four intervening time periods during which use of the platform was not subsidized. Six months also corresponds to the typical interval between desludgings for most households. For each outcome of interest, I estimate the following equation:

$$Y_{jt} = \alpha + \sum_{k=0}^{8} \beta_k (T_j \times \mathbf{1}[k = t]) + \lambda_t + \eta_j + \epsilon_j \tag{1}$$

where Y_{jt} is an outcome for grid point j in six-month period t. The primary outcomes considered below are a dummy variable equal to one if any households near grid point j called to use the platform in time period t, and a count variable with the number of household requests. Additional outcomes include a count variable with the number of desludgings successfully sourced through the platform (a subset of the number of requests) and the prices of all auctions and successful desludgings. I additionally separate the total requests variable and look at the number of requests from users who had never called during the subsidy phase ("post-subsidy new users") and users who used the platform during the experiment ("subsidy-period repeat users").

T_j is a dummy equal to one for treated grid point neighborhoods and zero for non-treated grid point neighborhoods. In my preferred specification, I include grid point fixed effects (η_j) to account for potential neighborhood-level, time-invariant differences in use of mechanized desludging due to location, neighborhood accessibility, and baseline wealth.[14] I additionally include time period fixed effects λ_t. Standard errors ϵ_j are clustered at the grid point level to account for serial autocorrelation.[15] The coefficients of interest, β_k, capture the within-period difference in adoption in the neighborhoods of treated grid-points relative to non-treated grid points after accounting for time invariant neighborhood characteristics.

Identification of the β_k coefficients rests primarily on the assumption that there are not time-varying differential changes in treated and non-treated neighborhoods that are unrelated to the subsidy program I study. This is a plausible assumption given that the neighborhoods I study are quite small, with a radius of 100m. The popular definition of neighborhoods in Dakar is typically much larger, as are the smallest formal administrative units, and these do not correspond in any consistent way to the grid point neighborhoods I study. Additionally, I assume that a given grid point's assignment to participate in the experiment of Lipscomb and Schechter (2018) was as-good-as-random, conditional on the

[14] Results are robust to instead including baseline control variables and a higher-level arrondissement fixed effect.

[15] In Table A.10 I show alternative standard errors arising from a permutation test, discussed below in Section 4.3.

sampling methodology described above to identify study areas.[16] In essence, I assume the fact that one set of grid points was assigned to the experimental subsidies and the other was only used for the companion survey is exogenous to any characteristics of these two sets of grid points.

4 Results

In this section, I present results on demand for the platform over time, as well as exploring heterogeneity and the robustness of my results.

First, in Figures 4a and 4b, I present results showing the per-period intention-to-treat effects of exposure to the experimental subsidies on subsequent use of the platform. Table A.1 presents these same results in a table. In the first six months of the experiment, 45% of non-treated neighborhoods had any residents call to use the platform (baseline mean in Column 1 of Table A.1), and on average about 0.93 households per neighborhood used the platform during that period (baseline mean in Column 2). The treatment increased the probability of any calls from a neighborhood during this period by 18 percent (8 p.p.), and increased the number of requests by 23 percent (0.21 requests). In the second six months of the experiment, the probability and volume of calls in non-treated neighborhoods declined to about 18 percent and 0.31 households per neighborhood. In treated neighborhoods, usage remained persistently higher, with an 88 percent (16 p.p.) greater probability of any usage and 96 percent (0.3) more requests.

Following the experiment, I find that platform use remained persistently high in previously-treated neighborhoods. In the first six months, about 22 percent of non-treated neighborhoods had any platform users, and there were an average of 0.42 calls per neighborhood. This represents a slightly higher volume of calls in non-treated neighborhoods than the previous six months. Households typically desludge 1-2 times per year, and desludging needs exhibit some seasonality aligned with rainy season flooding and key holidays. The experiment treatment increased the probability of usage during this period by 82 percent (18 p.p.) and increased the volume of usage by 139 percent (0.585 requests). By 12-18 months after the experiment, the probability and volume of usage in treated neighborhoods remained persistently higher despite an overall decline in usage, with a 66 percent greater probability of usage (5 p.p compared to an 8 percent probability of any calls from non-treated areas) and 97 percent (0.28 requests) greater volume of usage, relative to 0.17 households per non-treated neighborhood. At 18 months post-experiment, usage of the platform has declined

[16] Note this assumption is distinct from the within-treated-grid-points randomization of Lipscomb and Schechter (2018). The sampling for Lipscomb and Schechter (2018) fixed an every-other-grid-point pattern, so the treatment assignment I consider in this paper is perfectly negatively correlated across neighboring grid points. There are only two possible treatment assignments given that spatial structure. For this paper, I assume that the choice of which set of grid points to include in the experiment was effectively random, and uncorrelated with unobservable neighborhood characteristics that would make one set of areas more likely to use the platform than the other. Below in Section 4.3 I discuss several strategies for assessing the robustness of my inference using permutation tests given this particular spatial structure.

in both treated and non-treated areas, with just 4 percent of neighborhoods with any usage and no detectable treatment effect.

Next, I consider the interaction between the experiment and the city-wide subsidy campaign which launched two years after the end of the experiment. Usage of the platform increases substantially across the city, with 29 percent of non-treated neighborhoods recording any usage and an average of 1.5 households per neighborhood calling. Previously-treated neighborhoods resume using the platform at an increased rate, with 33 percent higher probability of any usage (10 p.p) and 73 percent (1.1) more households calling during the city-wide campaign.

When the city-wide campaign ended, usage of the platform declined again in non-treated areas, with 10 percent of neighborhoods recording any usage and an average of 0.14 households per neighborhood calling in. Usage in treated neighborhoods declined more slowly, with 105 percent greater probability of usage at the extensive margin (11 p.p.) and 201 (0.28) percent greater volume of requests. Six months after the subsidy campaign ended, the call center was temporarily closed due to a change in management, and when it reopened usage was low in both treated and non-treated areas up to the limit of the administrative data I have available.

4.1 Potential mechanisms

In this subsection, I explore some potential mechanisms for the pattern of treatment effects shown in Figure 4.

One channel by which exposure to the platform during the experiment could lead to increased use later is through prices. This could occur if there are increasing returns to scale for service providers in a neighborhood, or if serving a neighborhood allows providers to learn more about accessibility (road quality and width) and offer lower prices in the future in some places. In Table A.4, I find no evidence of a treatment effect on price over time in previously-subsidized areas. Price does not appear to be a primary channel driving continued use of the platform in previously-subsidized areas relative to non-treated areas.

Another potential channel by which treated neighborhoods could continue to exhibit higher demand is spillovers, or within-neighborhood referrals. Neighborhood referrals are an important source of information about mechanized desludging in this context (Deutschmann et al., 2024b). If we observe that there are consistently more new platform users in previously-treated neighborhoods over time than in non-treated neighborhoods, this would be consistent with information about the platform being shared. As described above in Section 2, I define new and repeat users based on whether or not they used the platform at all during the year of the Lipscomb and Schechter (2018) experiment (whether or note the user themselves was eligible for the subsidy, or in a subsidized neighborhood).

Table A.5 first shows summary statistics by period for use of the platform by new and repeat users. Consistently across periods, the raw average number of requests from new users in treated areas exceeds the average requests from new users in non-treated areas. Table A.6 corroborates this in a regression: excluding repeat users who learned about the platform during the subsidy phase, there are still persistent increases in use of the platform by new users after the experiment ends. These effects fade out past 12 months. Treatment effects re-emerge with the city-wide subsidy campaign and again persist for the six months following the end of that campaign. Table A.6 also show that there are persistently more requests from repeat users. The persistent increase in both new and repeat users in previously-treated neighborhoods suggest households that have used the platform find it valuable to continue using, and that they may share information with neighbors. However, absent additional data, these results remain only suggestive that information spillovers could be occuring.

4.2 Potential downstream impacts

Next, I discuss some of the potential downstream impacts of my findings on neighborhood environmental quality and household health. Because this paper relies exclusively on baseline survey data and platform administrative data, I cannot say with certainty how much of the increase in usage in treated areas represents an increase in mechanized desludging use overall relative to what we would expect at baseline.

To give a sense of how the usage of the platform compares to what we might expect to be happening in these neighborhoods overall, I use population density data from WorldPop (WorldPop and CIESIN, 2020) and baseline household sizes from neighborhood surveys to estimate that the median neighborhood in my sample has about 53 households. From the baseline surveys, the median household at baseline desludges about twice per year, but only about a third of households in our data have ever used a mechanized desludging, suggesting the median neighborhood had about 16 households performing a mechanized desludging in a given six month period at baseline.

One way to estimate downstream impacts is to look at the number of desludgings sourced directly through the platform.[17] This gives an estimate of the *direct* impacts of the subsidies on longer-run household behavior and welfare. Figure 5 and Table A.3 show that the increased platform use in previously-treated neighborhoods did lead to an increase in desludgings sourced through the platform, but the increase in most periods is small in magnitude. In the six months after the experiment ended, the average treated neighborhood saw an additional 0.11 desludgings sourced through the platform, or less than 1 percent of baseline mechanized desludgings in the median neighborhood. At its peak, during the

[17]There are multiple reasons why a service request could fail to lead to a completed job. During non-subsidized periods, households may choose not to accept the price offered, or may use the offered price to negotiate a better price with their usual provider. Accessibility is also a salient issue: households are sometimes inaccessible by some or all mechanized desludging trucks, if their street is too narrow or too sandy, or if the pit opening is too far from the street. In general I do not observe the reason a given job was not completed.

city-wide subsidy phase, treated neighborhoods saw an additional 0.98 desludgings sourced through the platform, or about 6 percent of baseline in the median neighborhood. The direct impacts on neighborhood environmental quality and household health are therefore likely to be small.

However, there are additional channels by which the platform could influence the wider market and households' sanitation choices. Households could achieve a better bargaining outcome in the traditional market, if having a quote from the platform provides households with information about current market prices or a credible outside option. Deutschmann et al. (2024a) estimate that, over the short term, each additional auction in a neighborhood in 2014 (induced by random variation in advertising across administrative units) reduced prices in the traditional market and led to 4 p.p. increase in use of mechanized desludging among nearby households. If this relationship were to hold throughout the period I study, a back-of-the-envelope calculation suggests that, following the conclusion of the experiment, on average about five more mechanized desludgings happened in the median treated neighborhood in the subsequent three years than otherwise would have occurred, or about 5 percent of baseline in the median neighborhood. Given the number of neighborhoods, this suggests there were about 1850 more mechanized desludgings total over the three years following the conclusion of the experiment. By comparison, the experiment directly subsidized about 350 desludgings over the course of one year.

Is this change in mechanized desludging meaningful for health? Houde et al. (2024) estimate an elasticity of diarrhea with respect to mechanized desludgings in a neighborhood of -0.58. A 5-6% increase in mechanized desludging use in the median neighborhood could therefore lead to a roughly 2.5-4% decrease in diarrhea incidence among children.

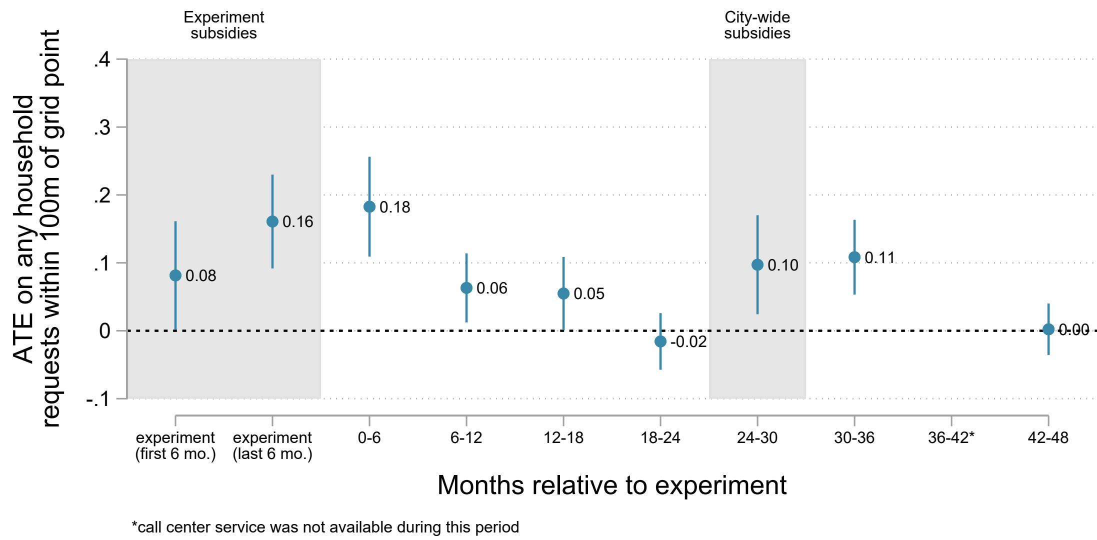
4.3 Robustness

In my preferred specifications, I rely on a radius of 100 meters to define grid point neighborhoods, since this exactly splits the 200 meter distance used to initially define the sampling frame of grid points. Nevertheless, one may wish to verify that the results presented are not driven entirely by this particular neighborhood definition. In Tables A.7 and A.8 I present results using alternative radii (75 meters, 125 meters, and 150 meters). These specifications do produce mild changes in the magnitude of coefficients, but are qualitatively similar and rarely result in any changes in statistical significance.

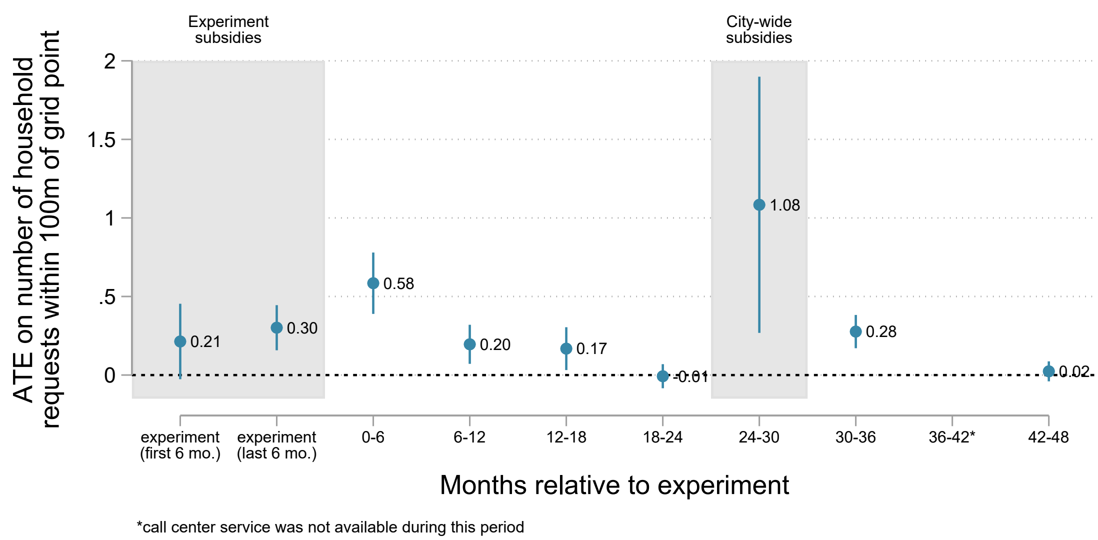
Additionally, as described above in Section 2, in my preferred specifications I include only grid points in areas that were surveyed in both of the relevant baseline surveys. In Columns 4-6 of Tables A.7 and A.8, I present results when I include grid points in areas where only one set of surveys were done (i.e., either the experiment was conducted in an area but the interspersed areas were not surveyed in the other survey, or vice versa). Using this expanded sample, there are mild changes in the magnitude of coefficients but qualitatively similar results.

Figure 4: Average treatment effects of neighborhood experiment subsidy assignment by period

(a) Impact of experimental subsidies on probability of calls from a neighborhood

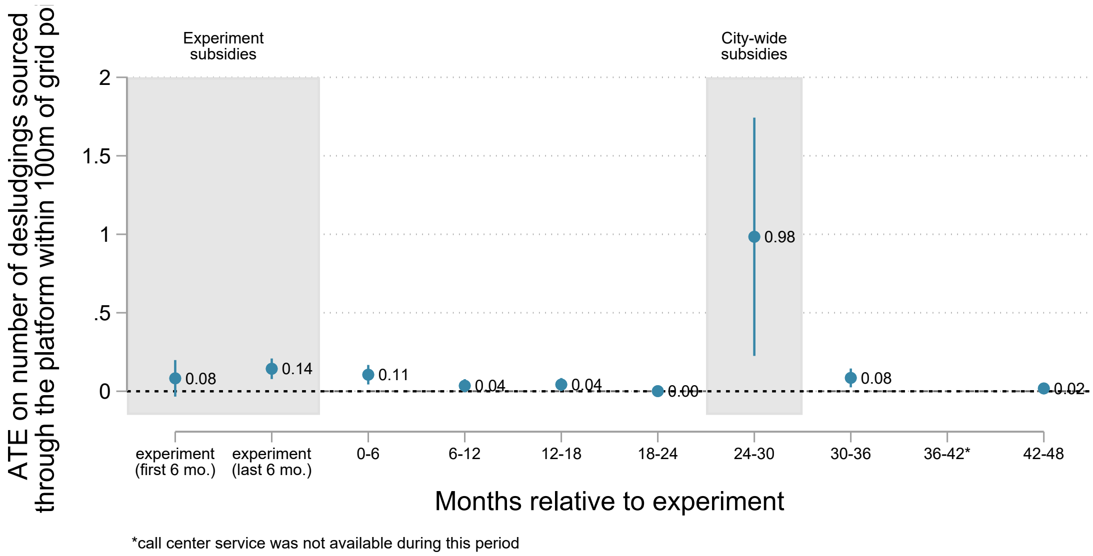


(b) Impact of experimental subsidies on volume of calls from a neighborhood



Results shown in these figures are the per-period treatment effect estimates, shown above in equation (1) as β_k. Figure 4a matches column 1 of Table A.1 and shows estimates in which the outcome is a dummy equal to one if any households from the area around grid point j called the platform in period t. Figure 4b matches column 2 of Table A.1 and shows estimates in which the outcome is the number of service requests received from the area around grid point j in period t. All regressions include grid point and time period fixed effects, with standard errors clustered at the grid point level. Error bars show 95 percent confidence intervals.

Figure 5: Average treatment effects of neighborhood experiment subsidy assignment by period on desludgings sourced through the platform



Results shown in this figure are the per-period treatment effect estimates, shown above in equation (1) as β_k. Table A.3 shows the full regression results. The outcome is a dummy equal to 1 if a desludging was successfully sourced through the platform. All regressions include grid point and time period fixed effects, with standard errors clustered at the grid point level. Error bars show 95 percent confidence intervals.

Following Chen and Roth (2024), I conduct additional analysis for my second outcome of interest, the number of requests from a neighborhood in a period, using Poisson Pseudo-Maximum Likelihood. Results of this exercise are presented in Table A.2. In general, the results are qualitatively consistent with the findings presented in Table A.1, with the exception of an insignificant coefficient ($p = 0.165$) for the treatment effect during the first experiment period. In most cases the implied effects are slightly larger, relative to the OLS results. For example, this alternative specification suggests a 165 percent increase in request volume, relative to a 96 percent increase implied by the OLS regression.

Finally, despite the plausibly as-good-as-random assignment of treatment status, the strategy of assigning every other grid point to treatment status means that there are only two possible treatment assignments conditional on the location of the grid points. This complicates the use of standard randomization inference procedures (Young, 2019). I conduct a permutation test in which I fix the spatial *structure* of treatment assignment, randomly shift the set of grid points by up to 200 meters in any direction, and randomly assign one set of every-other grid points to treatment. For a given counterfactual set of grid points, I repeat the entire procedure of assigning households to neighborhoods (as illustrated in Figure A.2) and create the resulting counterfactual cluster-level panel. The results of this exercise are shown in curly brackets in Table A.10. In general, the results mirror the conventional p-values, with several exceptions where the p-values from this procedure exceed conventional levels of significance, which could suggest the analytical standard errors I estimate clustered at the grid point level are somewhat undersized. Nevertheless, the primary qualitative findings hold: short-term exposure to subsidies causes increased use of the platform when experimental subsidies end, which fades to become indistinguishable from zero over time, and increased platform use in treated neighborhoods re-emerges when the city-wide subsidy campaign begins.

5 Conclusion

In this paper, I explore the dynamics of household adoption of a platform for sanitation services in Dakar, Senegal. I show that short-term subsidies had persistent impacts on household use of the platform years after the subsidies ended, improving their cost-effectiveness and with the potential for downstream effects on neighborhood environmental quality and household effects.

My paper has implications for the design of optimal sanitation and environmental policy in the presence of externalities, and more broadly for our understanding of the role of short-term subsidies in driving longer-run technology adoption. In this case, past exposure to subsidies increased use of the matching platform both when it was and was not subsidized. For policymakers interested in increasing adoption of matching platforms to address market frictions, my results suggest a role for short-term subsidies. Taken together with the results of Johnson and Lipscomb (2021), targeted short-term discounts for the poorest households may be a particularly cost-effective strategy for inducing longer-run behavioral change, perhaps re-occurring periodically to reinforce the longer-run change. Furthermore, my results suggest there may be some spillovers in adoption of platforms within neighborhoods, inducing new households to adopt the platform in addition to sparking persistent changes in usage among direct recipients.

Future work could explore whether and how platforms like the one I study could be further scaled by governments or social enterprises to improve the functioning of public service markets in low-income countries. It would also be of interest for future work to disentangle the role of different types of learning in driving the effects I observe, including learning about the convenience of the platform itself, learning about the service providers who use the platform, and learning about the health and environmental outcomes that increase demand.

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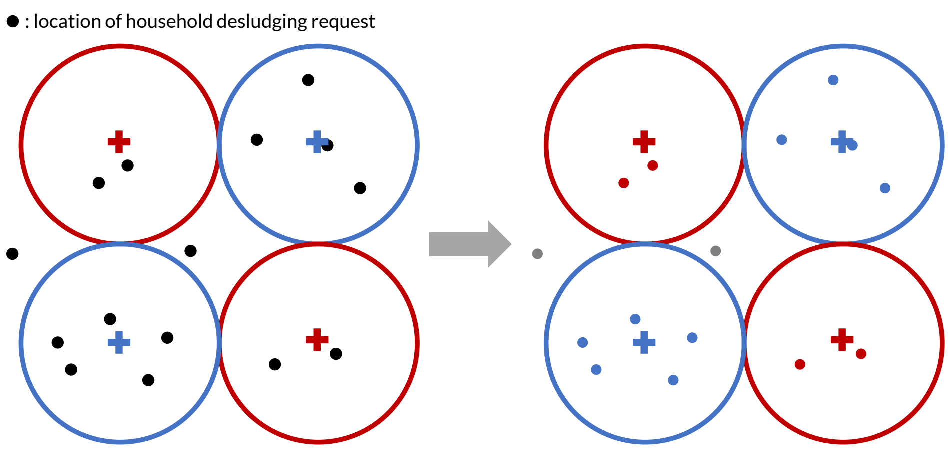
A Additional Tables and Figures

Figure A.1: Advertising for the city-wide subsidy campaign

Figure A.2: Example of household attribution to nearby grid points



Note: This figure shows a visual representation of how I attribute households to treated or non-treated gridpoints. Each black dot in the left pane represents a household request. Service requests from households within each 100m circle are attributed to the nearest grid point. Service requests outside these circles are excluded from the analysis.

Figure A.3: Distribution of household requests from a given neighborhood in a given period

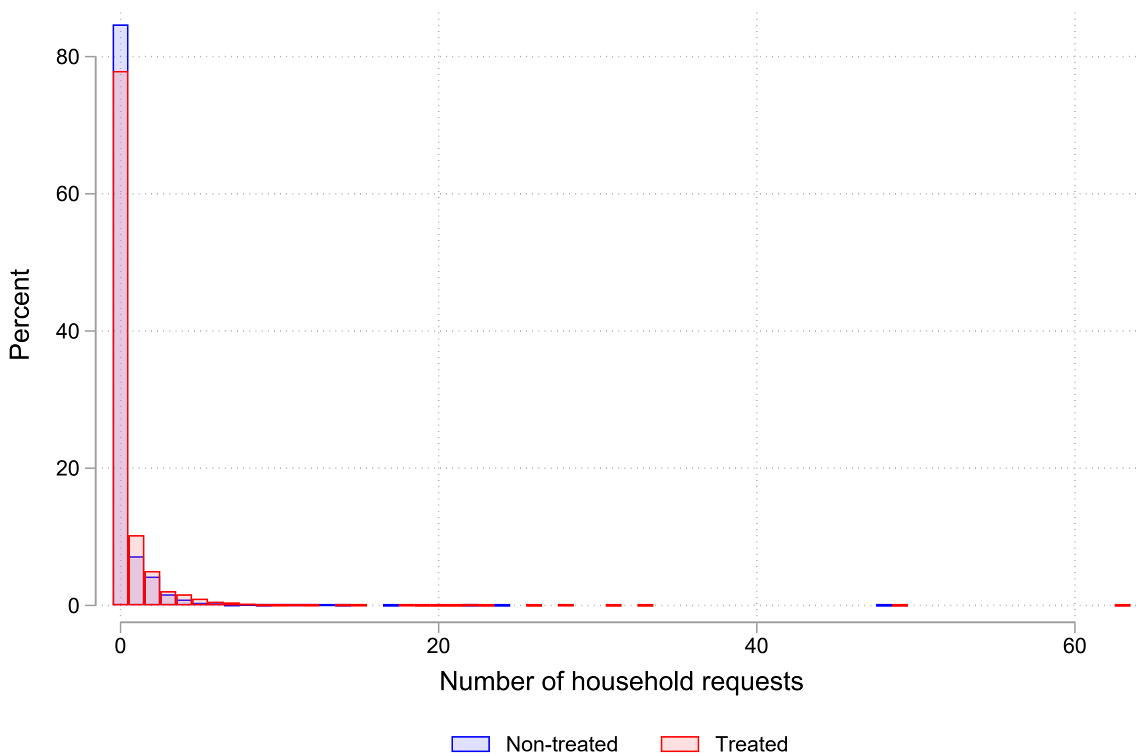


Table A.1: Call center use by period during and after experimental subsidies

	(1) Any Requests	(2) Number of Requests
Treated ×	0.081**	0.214*
experimental subsidies (first 6 mo.)	(0.041)	(0.122)
Treated ×	0.161***	0.301***
experimental subsidies (last 6 mo.)	(0.035)	(0.073)
Treated ×	0.183***	0.585***
0-6 months post-subsidies	(0.037)	(0.099)
Treated ×	0.063**	0.196***
6-12 months post-subsidies	(0.026)	(0.063)
Treated ×	0.055**	0.168**
12-18 months post-subsidies	(0.027)	(0.069)
Treated ×	-0.016	-0.007
18-24 months post-subsidies	(0.021)	(0.039)
Treated ×	0.097***	1.084***
city-wide subsidies	(0.037)	(0.415)
Treated ×	0.108***	0.277***
30-36 months post-subsidies	(0.028)	(0.054)
Treated ×	0.002	0.024
42-48 months post-subsidies	(0.019)	(0.032)
experimental	-0.269***	-0.621***
subsidies (last 6 mo.)	(0.034)	(0.085)
0-6 months	-0.228***	-0.514***
post-subsidies	(0.034)	(0.086)
6-12 months	-0.383***	-0.817***
post-subsidies	(0.032)	(0.084)
12-18 months	-0.369***	-0.762***
post-subsidies	(0.032)	(0.084)
18-24 months	-0.414***	-0.872***
post-subsidies	(0.029)	(0.081)
city-wide subsidies	-0.159***	0.559**
	(0.034)	(0.245)
30-36 months	-0.348***	-0.797***
post-subsidies	(0.032)	(0.082)
42-48 months	-0.421***	-0.890***
post-subsidies	(0.031)	(0.084)
Observations	6540	6540
Number of grid points	654	654
Non-treated baseline mean	0.452	0.934
Fixed effects	Grid Point	Grid Point

Results in this table are from linear regressions of the outcome variables (shown at the top of each column) on the treatment dummy and time period dummies. All regressions include grid point fixed effects. Non-treated baseline mean is the mean of the outcome variable among non-treated grid points in the initial experimental subsidies phase. Standard errors (in parentheses) are clustered at the grid point level. Note that the call center was not in operation in the 36-42 month post-subsidy period.

Table A.2: Call center use by period during and after experimental subsidies, estimated with Poisson

	(1) Number of Requests (Poisson)
Treated ×	0.527
experimental subsidies (first 6 mo.)	(0.380)
Treated ×	0.973**
experimental subsidies (last 6 mo.)	(0.379)
Treated ×	1.189***
0-6 months post-subsidies	(0.375)
Treated ×	1.236***
6-12 months post-subsidies	(0.458)
Treated ×	0.942**
12-18 months post-subsidies	(0.441)
Treated ×	-0.470
18-24 months post-subsidies	(0.702)
Treated ×	0.880**
city-wide subsidies	(0.360)
Treated ×	1.378***
30-36 months post-subsidies	(0.369)
Treated ×	0.261
42-48 months post-subsidies	(0.533)
experimental	-1.091***
subsidies (last 6 mo.)	(0.154)
0-6 months	-0.798***
post-subsidies	(0.137)
6-12 months	-2.076***
post-subsidies	(0.277)
12-18 months	-1.690***
post-subsidies	(0.220)
18-24 months	-2.712***
post-subsidies	(0.320)
city-wide subsidies	0.469***
	(0.169)
30-36 months	-1.913***
post-subsidies	(0.198)
42-48 months	-3.037***
post-subsidies	(0.354)
Observations	4900
Number of grid points	490
Non-treated baseline mean	1.338
Fixed effects	Grid Point

Results in this table are from a poisson pseudo-maximum likelihood regression of the outcome variables (number of requests from a neighborhood in a period) on the treatment dummy and time period dummies. All regressions include grid point fixed effects. Non-treated baseline mean is the mean of the outcome variable among non-treated grid points in the initial experimental subsidies phase. Standard errors (in parentheses) are clustered at the grid point level. Note that the call center was not in operation in the 36-42 month post-subsidy period.

Table A.3: Desludgings sourced through the platform by period during and after experimental subsidies

	(1) Number of Completed Desludgings
Treated × experimental subsidies (first 6 mo.)	0.082
	(0.059)
Treated × experimental subsidies (last 6 mo.)	0.143***
	(0.033)
Treated × 0-6 months post-subsidies	0.105***
	(0.032)
Treated × 6-12 months post-subsidies	0.035
	(0.021)
Treated × 12-18 months post-subsidies	0.043**
	(0.021)
Treated × 18-24 months post-subsidies	0.001
	(0.014)
Treated × city-wide subsidies	0.984**
	(0.386)
Treated × 30-36 months post-subsidies	0.085***
	(0.031)
Treated × 42-48 months post-subsidies	0.018
	(0.014)
experimental subsidies (last 6 mo.)	-0.166***
	(0.039)
0-6 months post-subsidies	-0.141***
	(0.040)
6-12 months post-subsidies	-0.214***
	(0.036)
12-18 months post-subsidies	-0.210***
	(0.037)
18-24 months post-subsidies	-0.234***
	(0.036)
city-wide subsidies	1.117***
	(0.230)
30-36 months post-subsidies	-0.162***
	(0.039)
42-48 months post-subsidies	-0.238***
	(0.036)
Observations	6540
Number of grid points	654
Non-treated baseline mean	0.245
Fixed effects	Grid Point

Results in this table are from a linear regression of the outcome variable (number of desludgings sourced through the platform in a neighborhood in a period) on the treatment dummy and time period dummies. All regressions include grid point fixed effects. Non-treated baseline mean is the mean of the outcome variable among non-treated grid points in the initial experimental subsidies phase. Standard errors (in parentheses) are clustered at the grid point level. Note that the call center was not in operation in the 36-42 month post-subsidy period.

Table A.4: Call center prices by period during and after experimental subsidies

	(1) Price (all requests)	(2) Price (completed desludgings)
Treated ×	-3.834*	
experimental subsidies (first 6 mo.)	(2.050)	
Treated ×	-2.356	
experimental subsidies (last 6 mo.)	(2.122)	
Treated ×	-2.434	-1.890
0-6 months post-subsidies	(1.994)	(3.165)
Treated ×	-3.405	-3.446
6-12 months post-subsidies	(2.550)	(3.905)
Treated ×	-0.824	-2.125
12-18 months post-subsidies	(2.452)	(3.461)
Treated ×	-5.621**	-13.455***
18-24 months post-subsidies	(2.383)	(3.878)
Treated ×	-2.014	0.091
30-36 months post-subsidies	(1.970)	(3.252)
Treated ×	1.007	3.626
42-48 months post-subsidies	(2.276)	(5.706)
experimental	1.013	
subsidies (last 6 mo.)	(0.982)	
0-6 months	2.761***	0.000
post-subsidies	(0.883)	(.)
6-12 months	3.053*	0.722
post-subsidies	(1.561)	(2.035)
12-18 months	0.518	-1.677
post-subsidies	(1.689)	(1.813)
18-24 months	2.067	1.259
post-subsidies	(1.284)	(2.894)
30-36 months	0.103	-1.641
post-subsidies	(0.817)	(1.564)
42-48 months	-2.510*	-7.458
post-subsidies	(1.377)	(4.767)
Observations	1213	228
Number of grid points	355	88
Non-treated baseline mean	46.397	46.886

Results in this table are from linear regressions of the outcome variables (shown at the top of each column) on the treatment dummy and time period dummies. Both outcomes are defined in USD (at a conversion rate of 550 XOF to 1 USD). For column 1, note that subsidy recipients during the experiment did not directly observe the price offered. Column 2 only includes periods when subsidies were not active. All regressions include grid point fixed effects. Non-treated baseline mean is the mean of the outcome variable among non-treated grid points in the initial experimental subsidies phase. Standard errors (in parentheses) are clustered at the grid point level. Note that the call center was not in operation in the 36-42 month post-subsidy period.

Table A.5: Average household requests by period, by treatment status, and by new/repeat status

	All users		Post-subsidy new users		Subsidy-period repeat users	
	Treated	**Non-treated**	**Treated**	**Non-treated**	**Treated**	**Non-treated**
Experiment subsidies	1.121	0.934				
(first 6 mo.)	(1.729)	(1.411)				
Experiment subsidies	0.588	0.314				
(last 6 mo.)	(1.089)	(0.795)				
0-6 months	0.978	0.421	0.808	0.341	0.170	0.079
post subsidies	(1.636)	(0.957)	(1.472)	(0.851)	(0.554)	(0.413)
6-12 months	0.286	0.117	0.203	0.117	0.082	0.000
post subsidies	(0.977)	(0.552)	(0.748)	(0.552)	(0.549)	(0.000)
12-18 months	0.313	0.172	0.201	0.131	0.113	0.041
post subsidies	(1.029)	(0.648)	(0.776)	(0.542)	(0.656)	(0.285)
18-24 months	0.027	0.062	0.011	0.010	0.016	0.052
post subsidies	(0.256)	(0.347)	(0.128)	(0.131)	(0.222)	(0.301)
city-wide	2.549	1.493	2.146	1.279	0.404	0.214
subsidies	(6.505)	(4.316)	(5.646)	(3.791)	(1.447)	(0.982)
30-36 months	0.387	0.138	0.371	0.124	0.016	0.014
post subsidies	(0.937)	(0.457)	(0.904)	(0.438)	(0.148)	(0.143)
42-48 months	0.066	0.045	0.060	0.045	0.005	0.000
post subsidies	(0.342)	(0.266)	(0.335)	(0.266)	(0.074)	(0.000)

Note: This table shows the mean values of the number of household requests from each neighborhood, both overall and separately by new/repeat user status. Standard deviations are shown in parentheses.

Table A.6: Call center use by new and repeat callers

	(1) Post-Subsidy New Users	(2) Subsidy Period Repeat Users
Treated × 0-6 months post-subsidies	0.4852*** (0.0891)	0.0993** (0.0389)
Treated × 6-12 months post-subsidies	0.1049* (0.0538)	0.0907*** (0.0303)
Treated × 12-18 months post-subsidies	0.0884 (0.0559)	0.0796** (0.0393)
Treated × 18-24 months post-subsidies	0.0195 (0.0302)	-0.0269 (0.0227)
Treated × city-wide subsidies	0.8852** (0.3618)	0.1984** (0.0942)
Treated × 30-36 months post-subsidies	0.2656*** (0.0520)	0.0110 (0.0148)
Treated × 42-48 months post-subsidies	0.0125 (0.0304)	0.0110 (0.0089)
6-12 months post-subsidies	-0.2241*** (0.0540)	-0.0793*** (0.0242)
12-18 months post-subsidies	-0.2103*** (0.0484)	-0.0379 (0.0269)
18-24 months post-subsidies	-0.3310*** (0.0501)	-0.0276 (0.0297)
city-wide subsidies	0.9379*** (0.2048)	0.1345** (0.0622)
30-36 months post-subsidies	-0.2172*** (0.0473)	-0.0655*** (0.0239)
42-48 months post-subsidies	-0.2966*** (0.0497)	-0.0793*** (0.0242)
Observations	5232	5232
Number of grid points	654	654
Non-treated baseline mean	0.341	0.079
Fixed effects	Grid Point	Grid Point

Results in this table are from linear regressions of the outcome variables (shown at the top of each column) on the treatment dummy and time period dummies. Post-Subsidy New Users is the number of requests from a given neighborhood from users that did not call to use the platform during the experimental subsidy period. Subsidy Period Repeat Users is the number of requests from a neighborhood by households which had previously used the platform during the experimental subsidy period. All regressions include grid point fixed effects. Non-treated baseline mean is the mean of the outcome variable among non-treated grid points in the initial experimental subsidies phase. Standard errors (in parentheses) are clustered at the grid point level. Note that the call center was not in operation in the 36-42 month post-subsidy period.

Table A.7: Robustness table: Call center use (extensive margin) by period during and after experimental subsidies, with different sample definitions and neighborhood radius thresholds

	Dense grid points			All grid points		
	(1)	(2)	(3)	(4)	(5)	(6)
	75m	125m	150m	75m	125m	150m
Treated ×	0.074**	0.110***	0.113***	0.096***	0.126***	0.136***
experimental subsidies (first 6 mo.)	(0.037)	(0.041)	(0.041)	(0.034)	(0.038)	(0.038)
Treated ×	0.118***	0.213***	0.234***	0.121***	0.207***	0.221***
experimental subsidies (last 6 mo.)	(0.030)	(0.037)	(0.038)	(0.027)	(0.033)	(0.034)
Treated ×	0.165***	0.194***	0.199***	0.160***	0.176***	0.187***
0-6 months post-subsidies	(0.033)	(0.040)	(0.040)	(0.029)	(0.036)	(0.036)
Treated ×	0.069***	0.062**	0.062**	0.059***	0.050*	0.051*
6-12 months post-subsidies	(0.022)	(0.029)	(0.030)	(0.020)	(0.026)	(0.027)
Treated ×	0.059**	0.055*	0.056*	0.050**	0.051*	0.055*
12-18 months post-subsidies	(0.023)	(0.031)	(0.032)	(0.021)	(0.028)	(0.029)
Treated ×	-0.010	-0.005	-0.012	-0.009	-0.007	-0.011
18-24 months post-subsidies	(0.018)	(0.025)	(0.025)	(0.016)	(0.022)	(0.023)
Treated ×	0.094***	0.137***	0.125***	0.099***	0.121***	0.113***
city-wide subsidies	(0.034)	(0.038)	(0.038)	(0.031)	(0.035)	(0.035)
Treated ×	0.092***	0.128***	0.131***	0.083***	0.106***	0.106***
30-36 months post-subsidies	(0.023)	(0.031)	(0.033)	(0.021)	(0.028)	(0.029)
Treated ×	0.006	0.017	0.009	-0.006	-0.001	-0.010
42-48 months post-subsidies	(0.015)	(0.022)	(0.023)	(0.014)	(0.020)	(0.020)
experimental	-0.190***	-0.317***	-0.338***	-0.164***	-0.286***	-0.295***
subsidies (last 6 mo.)	(0.031)	(0.035)	(0.035)	(0.027)	(0.031)	(0.031)
0-6 months	-0.166***	-0.248***	-0.262***	-0.142***	-0.218***	-0.229***
post-subsidies	(0.031)	(0.036)	(0.035)	(0.026)	(0.031)	(0.031)
6-12 months	-0.276***	-0.424***	-0.441***	-0.238***	-0.382***	-0.394***
post-subsidies	(0.029)	(0.033)	(0.033)	(0.025)	(0.029)	(0.030)
12-18 months	-0.266***	-0.407***	-0.424***	-0.229***	-0.368***	-0.382***
post-subsidies	(0.029)	(0.034)	(0.034)	(0.025)	(0.030)	(0.030)
18-24 months	-0.276***	-0.472***	-0.497***	-0.241***	-0.431***	-0.450***
post-subsidies	(0.027)	(0.030)	(0.030)	(0.023)	(0.027)	(0.027)
city-wide subsidies	-0.100***	-0.200***	-0.207***	-0.071***	-0.153***	-0.156***
	(0.032)	(0.033)	(0.034)	(0.027)	(0.030)	(0.031)
30-36 months	-0.252***	-0.386***	-0.400***	-0.221***	-0.343***	-0.348***
post-subsidies	(0.028)	(0.033)	(0.034)	(0.024)	(0.030)	(0.031)
42-48 months	-0.286***	-0.490***	-0.517***	-0.246***	-0.439***	-0.459***
post-subsidies	(0.028)	(0.031)	(0.031)	(0.024)	(0.028)	(0.028)
Observations	6540	6540	6540	7630	7630	7630
Number of grid points	654	654	654	763	763	763
Non-treated baseline mean	0.303	0.524	0.552	0.263	0.473	0.496

Results in this table are from linear regressions of a dummy variable indicating any household calls from that neighborhood on the treatment dummy and time period dummies.Columns 1, 2, and 3 use the main sample of 654 grid points as described in Section 2, whereas columns 4, 5, and 6 use all 763 grid points surveyed, including in areas where only treated or non-treated areas were ultimately surveyed. Columns 1 and 4 use a 75m radius (narrower than 100m as used in my primary analysis), columns 2 and 5 use 125m, and columns 3 and 6 use 150m to define neighborhoods. All regressions include grid point fixed effects. Non-treated baseline mean is the mean of the outcome variable among non-treated grid points in the initial experimental subsidies phase. Standard errors (in parentheses) are clustered at the grid point level. Note that the call center was not in operation in the 36-42 month post-subsidy period.

Table A.8: Robustness table: Call center volume of use by period during and after experimental subsidies, with different sample definitions and neighborhood radius thresholds

	Dense grid points			All grid points		
	(1)	(2)	(3)	(4)	(5)	(6)
	75m	125m	150m	75m	125m	150m
Treated ×	0.243***	0.327**	0.494***	0.274***	0.421***	0.629***
experimental subsidies (first 6 mo.)	(0.090)	(0.140)	(0.160)	(0.081)	(0.130)	(0.149)
Treated ×	0.243***	0.417***	0.489***	0.234***	0.398***	0.478***
experimental subsidies (last 6 mo.)	(0.058)	(0.084)	(0.087)	(0.052)	(0.076)	(0.081)
Treated ×	0.508***	0.718***	0.790***	0.478***	0.651***	0.732***
0-6 months post-subsidies	(0.081)	(0.118)	(0.124)	(0.075)	(0.109)	(0.118)
Treated ×	0.177***	0.241***	0.252***	0.149***	0.203***	0.221***
6-12 months post-subsidies	(0.051)	(0.078)	(0.085)	(0.045)	(0.068)	(0.076)
Treated ×	0.137***	0.239**	0.263***	0.136***	0.256***	0.275***
12-18 months post-subsidies	(0.052)	(0.095)	(0.098)	(0.051)	(0.087)	(0.093)
Treated ×	-0.008	0.042	0.038	-0.009	0.036	0.043
18-24 months post-subsidies	(0.033)	(0.051)	(0.051)	(0.029)	(0.044)	(0.048)
Treated ×	1.008***	1.452**	1.412**	0.918***	1.381***	0.936
city-wide subsidies	(0.313)	(0.587)	(0.674)	(0.280)	(0.532)	(0.748)
Treated ×	0.206***	0.302***	0.320***	0.191***	0.279***	0.325***
30-36 months post-subsidies	(0.045)	(0.068)	(0.072)	(0.041)	(0.063)	(0.071)
Treated ×	0.019	0.081*	0.070	-0.000	0.044	0.041
42-48 months post-subsidies	(0.025)	(0.045)	(0.046)	(0.023)	(0.039)	(0.043)
experimental	-0.334***	-0.852***	-0.934***	-0.295***	-0.748***	-0.807***
subsidies (last 6 mo.)	(0.061)	(0.099)	(0.103)	(0.053)	(0.086)	(0.089)
0-6 months	-0.286***	-0.697***	-0.738***	-0.238***	-0.581***	-0.603***
post-subsidies	(0.062)	(0.103)	(0.107)	(0.056)	(0.092)	(0.099)
6-12 months	-0.455***	-1.066***	-1.131***	-0.399***	-0.943***	-0.994***
post-subsidies	(0.060)	(0.105)	(0.111)	(0.052)	(0.091)	(0.096)
12-18 months	-0.434***	-0.990***	-1.062***	-0.377***	-0.881***	-0.929***
post-subsidies	(0.061)	(0.112)	(0.116)	(0.053)	(0.096)	(0.102)
18-24 months	-0.455***	-1.166***	-1.255***	-0.402***	-1.040***	-1.119***
post-subsidies	(0.055)	(0.102)	(0.106)	(0.048)	(0.089)	(0.092)
city-wide subsidies	0.224*	0.721**	1.107***	0.246**	0.799***	1.584***
	(0.124)	(0.289)	(0.374)	(0.110)	(0.258)	(0.544)
30-36 months	-0.438***	-1.000***	-1.059***	-0.388***	-0.875***	-0.918***
post-subsidies	(0.057)	(0.093)	(0.100)	(0.049)	(0.081)	(0.088)
42-48 months	-0.476***	-1.197***	-1.290***	-0.416***	-1.057***	-1.136***
post-subsidies	(0.058)	(0.105)	(0.109)	(0.050)	(0.091)	(0.094)
Observations	6540	6540	6540	7630	7630	7630
Number of grid points	654	654	654	763	763	763
Non-treated baseline mean	0.503	1.245	1.338	0.442	1.105	1.187

Results in this table are from linear regressions of the number of household calls from a neighborhood on the treatment dummy and time period dummies. Columns 1, 2, and 3 use the main sample of 654 grid points as described in Section 2, whereas columns 4, 5, and 6 use all 763 grid points surveyed, including in areas where only treated or non-treated areas were ultimately surveyed. Columns 1 and 4 use a 75m radius (narrower than 100m as used in my primary analysis), columns 2 and 5 use 125m, and columns 3 and 6 use 150m to define neighborhoods. All regressions include grid point fixed effects. Non-treated baseline mean is the mean of the outcome variable among non-treated grid points in the initial experimental subsidies phase. Standard errors (in parentheses) are clustered at the grid point level. Note that the call center was not in operation in the 36-42 month post-subsidy period.

Table A.9: Call center use by period during and after experimental subsidies, sample restricted to precise coordinates

	(1) Any Requests	(2) Number of Requests
Treated ×	0.060	0.125
experimental subsidies (first 6 mo.)	(0.039)	(0.112)
Treated ×	0.125^{***}	0.178^{***}
experimental subsidies (last 6 mo.)	(0.032)	(0.058)
Treated ×	0.126^{***}	0.345^{***}
0-6 months post-subsidies	(0.034)	(0.071)
Treated ×	0.068^{***}	0.197^{***}
6-12 months post-subsidies	(0.021)	(0.053)
Treated ×	0.025	0.106^{**}
12-18 months post-subsidies	(0.020)	(0.047)
Treated ×	-0.003	0.010
18-24 months post-subsidies	(0.015)	(0.025)
Treated ×	0.052	0.196^{*}
city-wide subsidies	(0.032)	(0.112)
Treated ×	0.021	0.050^{**}
30-36 months post-subsidies	(0.018)	(0.024)
Treated ×	0.003	0.026
42-48 months post-subsidies	(0.014)	(0.023)
experimental	-0.276^{***}	-0.586^{***}
subsidies (last 6 mo.)	(0.033)	(0.080)
0-6 months	-0.231^{***}	-0.528^{***}
post-subsidies	(0.034)	(0.079)
6-12 months	-0.390^{***}	-0.772^{***}
post-subsidies	(0.030)	(0.078)
12-18 months	-0.369^{***}	-0.734^{***}
post-subsidies	(0.030)	(0.078)
18-24 months	-0.393^{***}	-0.776^{***}
post-subsidies	(0.029)	(0.077)
city-wide subsidies	-0.217^{***}	-0.324^{***}
	(0.032)	(0.101)
30-36 months	-0.372^{***}	-0.766^{***}
post-subsidies	(0.030)	(0.076)
42-48 months	-0.393^{***}	-0.783^{***}
post-subsidies	(0.030)	(0.077)
Observations	6540	6540
Number of grid points	654	654
Non-treated baseline mean	0.407	0.800
Fixed effects	Grid Point	Grid Point

Results in this table are from linear regressions of the outcome variables (shown at the top of each column) on the treatment dummy and time period dummies. The sample is restricted to platform users with precise GPS coordinates and excludes households geo-localized only with the nearest landmark. All regressions include grid point fixed effects. Non-treated baseline mean is the mean of the outcome variable among non-treated grid points in the initial experimental subsidies phase. Standard errors (in parentheses) are clustered at the grid point level. Note that the call center was not in operation in the 36-42 month post-subsidy period.

Table A.10: Call center use by period during and after experimental subsidies, with permutation test p-values

	(1) Any Requests	(2) Number of Requests
Treated × experimental subsidies (first 6 mo.)	0.081 (0.041) [0.046] {0.048}	0.214 (0.122) [0.081] {0.068}
Treated × experimental subsidies (last 6 mo.)	0.161 (0.035) [<0.001] {0.031}	0.301 (0.073) [<0.001] {0.111}
Treated × 0-6 months post-subsidies	0.183 (0.037) [<0.001] {0.021}	0.585 (0.099) [<0.001] {0.012}
Treated × 6-12 months post-subsidies	0.063 (0.026) [0.015] {0.167}	0.196 (0.063) [0.002] {0.057}
Treated × 12-18 months post-subsidies	0.055 (0.027) [0.046] {0.188}	0.168 (0.069) [0.016] {0.129}
Treated × 18-24 months post-subsidies	-0.016 (0.021) [0.460] {0.484}	-0.007 (0.039) [0.849] {0.808}
Treated × city-wide subsidies	0.097 (0.037) [0.009] {0.095}	1.084 (0.415) [0.009] {0.001}
Treated × 30-36 months post-subsidies	0.108 (0.028) [<0.001] {0.002}	0.277 (0.054) [<0.001] {<0.001}
Treated × 42-48 months post-subsidies	0.002 (0.019) [0.911] {0.903}	0.024 (0.032) [0.467] {0.486}
Observations	6540	6540
Number of grid points	654	654
Non-treated baseline mean	0.138	0.228
Fixed effects	Grid Point	Grid Point

Results in this table are from linear regressions of the outcome variables (shown at the top of each column) on the treatment dummy and time period dummies. All regressions include grid point fixed effects. Non-treated baseline mean is the mean of the outcome variable among non-treated grid points in the initial experimental subsidies phase. Standard errors (in parentheses) are clustered at the grid point level. Analytical p-values are shown in square brackets. P-values from permutation test (with 1000 iterations) are shown in curly brackets. See Section 4.3 for more on this procedure. Note that the call center was not in operation in the 36-42 month post-subsidy period.