Subsidies and the sustainability of technology adoption: Evidence from the sanitation services market in Dakar *

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Abstract

New technologies may reduce costs for consumers, but costs to use these technologies may slow down adoption. In this paper, I study consumer adoption of an intermediation platform for sanitation services in Dakar, Senegal. The platform, run primarily as a public service, was designed to match households to sanitation service providers. I exploit spatial variation in exposure to short-term subsidies for the purchase of a septic pit desludging through the platform. Using a neighborhood-level panel constructed from platform administrative data, I show that neighborhoods exposed to short-term subsidies continue using the platform at significantly higher levels. Using additional data from a city-wide subsidy campaign, I show that past exposure to short-term subsidies increases future adoption at both subsidized and unsubsidized prices. I discuss possible mechanisms behind these results, and address the implications for social enterprises and governments seeking to operate intermediation platforms in decentralized service markets characterized by health externalities.

Keywords: Technology adoption, urban sanitation, intermediation, subsidies

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1 Introduction

Subsidizing the adoption of new or under-utilized technologies is a common strategy in development policy. These subsidies are often implemented by donors for a fixed period of time, with the intention to spark demand for products that persists after subsidies end. In some cases, such as for anti-malarial bed nets, improved cook-stoves, fertilizer, or solar lamps, a short-run subsidy may be sufficient to increase longer-run adoption (Dupas, 2014b; Bensch and Peters, 2020; Carter et al., 2021; Meriggi et al., 2021). In other cases, subsidized distribution may lower future adoption (Fischer et al., 2019). Understanding how subsidies affect longer-run use of a particular technology is key to designing efficient policy and using limited resources effectively.

In this paper, I use administrative data from an intermediation platform for sanitation services in Dakar, Senegal to evaluate the longer-run consequences of exposure to short-run subsidies for use of the platform. Intermediation platforms, such as ride-sharing software or auction websites, have the potential to transform markets by reducing consumer search costs and matching users to lower-cost service providers (Gehrig, 1993; Bakos, 1997; Brown and Goolsbee, 2002; Cramer and Krueger, 2016; Farronato and Fradkin, 2018; Salz, 2020). The platform I study first launched in 2013 and was operated primarily by the government as a public service during the period studied. To use the platform, households contact a call center and request a mechanized desludging of their septic pit. Improving management of faecal waste by increasing the use of mechanized desludging is a key challenge in many dense cities like Dakar (Kresch et al., 2020). The government's stated goal for the platform was to make it easier and cheaper for households to choose a mechanized desludging over a less-sanitary manual option. The market for sanitation services in Dakar has traditionally been characterized by high search costs for consumers and spatially dispersed service providers who may collude to keep prices high.[1] Existing work suggests intermediation in this market has been effective at lowering prices in some areas of the city (Deutschmann et al., 2021a).

To explore the impact of subsidies on longer-run technology adoption, I rely on data from two distinct but similar subsidy programs which induced households to use the intermediation platform to source a mechanized desludging. First, I exploit variation in neighborhood-level exposure to experimental subsidies offered to randomly selected households. In 2014, Lipscomb and Schechter (2018) provided households in randomly-selected areas of Dakar subsidies which were administered through the platform. These subsidies allowed households to purchase a mechanized desludging of their septic pit for about \$31 USD, roughly 66% of the average market price. I show that in the six months after subsidy availability ended, neighborhoods previously treated with the subsidies were 82% more likely to have any households use the platform to source a mechanized desludging. At eighteen months post-

[1] For more on the nature of the sanitation services market in Senegal, including interviews discussing collusion on prices, see https://www.npr.org/sections/money/2018/07/25/632444815/episode-855-the-poop-cartel.

experiment, treated neighborhoods remained 66% more likely to have any households using the platform. Treated neighborhoods had 139% more service requests than non-treated neighborhoods in the first six months post-experiment, and 97% more service requests eighteen months post-experiment. However, beyond eighteen months post-experiment, increased adoption in treated areas faded to be statistically indistinguishable from zero, suggesting that gains in adoption may not persist indefinitely absent additional intervention.

Second, I explore how past exposure to subsidized prices for sanitation services affects future take-up of similar subsidies. In 2017, two years after the subsidies offered in Lipscomb and Schechter (2018) ended, the government ran a major city-wide campaign in Dakar intended to increase adoption of improved sanitation services. The campaign offered households anywhere in the city desludgings for a fixed, subsidized price of about $30 USD, nearly the same price previously offered during the experiment. I show that neighborhoods previously offered the experimental subsidies were 33% more likely to have any households take advantage of the new round of subsidies. At the intensive margin, treated neighborhoods made 73% more service requests than non-treated neighborhoods. Furthermore, this increase in call center use similarly persists in treated neighborhoods at higher levels than non-treated neighborhoods after the conclusion of the city-wide subsidy campaign. It may be that households in treated areas formed reference points for prices and recognized that the subsidized price was a good deal (Kőszegi and Rabin, 2006), or it could be that the city-wide advertising campaign reminded households of the availability of the service and they were more comfortable using it given past experience.

Third, I demonstrate that the persistent increase in platform use in previously-treated neighborhoods is driven by both new and repeat users of the platform. If the effects were driven only by repeat customers, we might conclude that short-run subsidies are only successful at shifting long-run demand among direct recipients. If instead there are persistent increases in new users in previously-treated neighborhood, this suggests that knowledge and adoption of the platform may spill over within the community, and that subsidies may shift community-level demand beyond direct recipients. I show that in the six months after the Lipscomb and Schechter (2018) subsidies ended, about half of the increased demand in previously-treated areas was from new platform users. This fraction declines over time, but at eighteen months post-subsidy, about one quarter of the increased demand is still being driven by new platform users.

I additionally provide evidence of heterogeneity across space in the persistence of call center adoption. Existing evidence suggests that the health effects of adopting improved sanitation are a function of both a household's decision and the aggregate decisions in the nearby community (Andrés et al., 2017; Kresch et al., 2020). The decision to adopt improved sanitation may exhibit increasing returns to scale, in that it becomes more valuable for a household to adopt the more sanitary option if more of its neighbors have done so (Deutschmann et al., 2021b). Past research has demonstrated the potential for decision spillovers to increase community-level adoption of improved sanitation (Guiteras et al., 2015, 2019; Kresch et al., 2020). I show that use of the platform declines faster at the periphery of treated neighborhoods compared to the center of these neighborhoods. This may suggest that short-run decision spillovers in a concentrated geographical area are insufficient to sustain long-run adoption of improved sanitation when prices are relatively high and neighboring areas do not adopt at the same levels.

This paper builds on previous work demonstrating the role of price reductions in encouraging adoption of improved sanitation. High prices for mechanized desludging are a common feature of urban areas, and households generally exhibit low willingness to pay at market prices (Jenkins et al., 2015; Burt et al., 2019; Peletz et al., 2020). Lipscomb and Schechter (2018) establish that subsidies are effective at increasing adoption of mechanized desludging in the short term, and Johnson and Lipscomb (2021) further demonstrate the potential for targeting subsidies through an intermediation platform to increase mechanized adoption in a highly cost-effective way. Deutschmann et al. (2021b) find that behavioral channels like social pressure and reciprocity do not seem to cause sanitation decision spillovers in urban areas. Instead, households may tend to adopt improved sanitation as more of their neighbors adopt and the returns to their own adoption increase. The choice of how to desludge a pit is a regular choice with visible consequences for households and neighborhoods. Understanding the potential for learning and past experience to drive long term, persistent behavioral change is key to designing optimal sanitation policy. I contribute by showing that adoption of a related service, the intermediation platform, is persistent in the longer run following a subsidized period.

Finally, I contribute to a long literature on subsidies in health and sanitation. The question of whether and how to subsidize these technologies, particularly in the presence of health externalities and peer effects, has long been a question of both academic and policy interest (Kremer and Miguel, 2007; Hoffmann et al., 2009; Banerjee et al., 2010; Cohen and Dupas, 2010; Oster and Thornton, 2012; Dupas, 2014a; Tarozzi et al., 2014; Cohen et al., 2015; Baird et al., 2016). On the one hand, Dupas (2014b) highlights the potential for short-run subsidies to increase long-run demand for experience goods.[2] On the other hand, Fischer et al. (2019) find lower demand for health products following free distribution. In keeping with Dupas (2014b), I find evidence suggestive of long-run demand increases following past experiences with subsidies. In this context, however, I find that demand fades with time absent further intervention,[3] and that long-run demand increases may be driven in part by spillovers to neighbors of past subsidy recipients.

The rest of the paper is organized as follows. In Section 2, I describe the context of

[2]Carter et al. (2021) similarly identify long-run changes in fertilizer adoption from short-run subsidies, and further demonstrate that decision spillovers account for a large portion of subsidy-induced gains.

[3]This result is consistent with findings in the literature on habit formation. Caro-Burnett et al. (2021) find 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. (2021) similarly find that financial incentives increase handwashing, but the effects decay over time.

the sanitation services market in Dakar, including the establishment of the intermediation platform and the subsidy programs. In Section 3 I describe the empirical strategy, and present results of that strategy in Section 4. Section 5 concludes.

2 Context and Data

In 2011, the National Sanitation Office of Senegal (ONAS) launched an ambitious urban sanitation program supported by the Bill and Melinda Gates Foundation. The program supported a wide range of activities intended to reform and modernize the sanitation services sector in Dakar. In this paper, I explore the interaction of three key activities: the establishment of a sanitation call center, experimental subsidies offered as part of a large-scale randomized trial (Lipscomb and Schechter, 2018), and a large city-wide subsidy campaign conducted several years after the randomized trial ended.

First, the program supported the establishment of a call center and intermediation platform to match households to sanitation service providers using an innovative auction system. This system was run as a public service by ONAS with support from Water and Sanitation for Africa and Innovations for Poverty Action. The stated purpose of the call center system was to increase competition among service providers, reduce search costs for households, and facilitate regulation of the sector. The majority of trucks active in the sector were registered in the call center system, although a minority chose to participate on a regular basis. The platform conducted just-in-time auctions with sanitation service providers whenever a client called to request service, and the resulting auction ending price was offered to that client as the price available through the platform. As Deutschmann et al. (2021a) discuss in detail, 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.

Second, the program supported a set of experiments in 2013-2014 studying household demand for mechanized desludging services. 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 \$31 USD, which represented a significant discount over the baseline average market price of about \$45 USD.[4] In order to use the subsidies, households needed to call the call center and request a desludging using the intermediation platform. Each household had two subsidized desludgings available to use in a twelve month period. Key for this paper is the sampling strategy used to identify those households. The field team mapped a set of approximately 820 grid points across

[4] As Deutschmann et al. (2021b) describe, the other half of sampled households were offered subsidized prices of about \$43 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.

the city, placed 200 meters apart, and assigned every other grid point to be included in the experimental sample (the "treated" grid points). The remaining grid points were held out for a companion survey without any associated experiment. Figure 1 illustrates the sampling strategy and resulting set of grid points.

At both treated and non-treated grid points, the field team conducted household surveys to learn about past sanitation behavior and health outcomes.[5] To sample households for both surveys, enumerators would start at the grid point and follow a pre-determined pattern to spiral outwards. In the area around each grid point, 10-12 households were surveyed in each round, with a median distance of 50 meters from households to the grid point. For this paper, I combine both sets of grid points and calculate average characteristics at the grid point area level using the baseline surveys. The final "full" set of 763 grid points excludes some areas connected to the sewage network, highly flood-prone areas (in which household sanitation behavior is necessarily quite different), non-residential areas, and one small region of the city in which sampling was conducted differently for a different pilot experiment. To ensure close comparability, for the purposes of the main analysis in this paper I further restrict to the set of grid points which are entirely adjacent, leaving a final sample of 654 grid points.[6]

Two years after the conclusion of the experiments, in 2017, ONAS launched an extensive campaign to boost mechanized desludging use. They offered households a fixed, subsidized price almost exactly equal to the "high" subsidy previously offered during the experimental phase of the project.[7] This new subsidy was available city-wide and advertised extensively, including with billboards, radio spots, and promoted posts on Facebook (Figure A.1). As with the experiment, to access the subsidy households needed to call the call center to request a desludging using the intermediation platform.

In this paper, I rely primarily on administrative data covering the universe of desludgings conducted through the call center system. The platform operated almost continuously from 2013 to 2018.[8] Households in the data are geo-located using one of two strategies. About one third of households match directly to a baseline census conducted in Dakar in 2012, meaning I observe precise coordinates for their location.[9] The remaining two-thirds are

[5]Note: surveys of treated and non-treated areas were distinct, although similar in their collection of sanitation information. Non-treated areas were first surveyed in 2012 and again in 2014, whereas treated areas were first surveyed in 2013 and again in 2015.

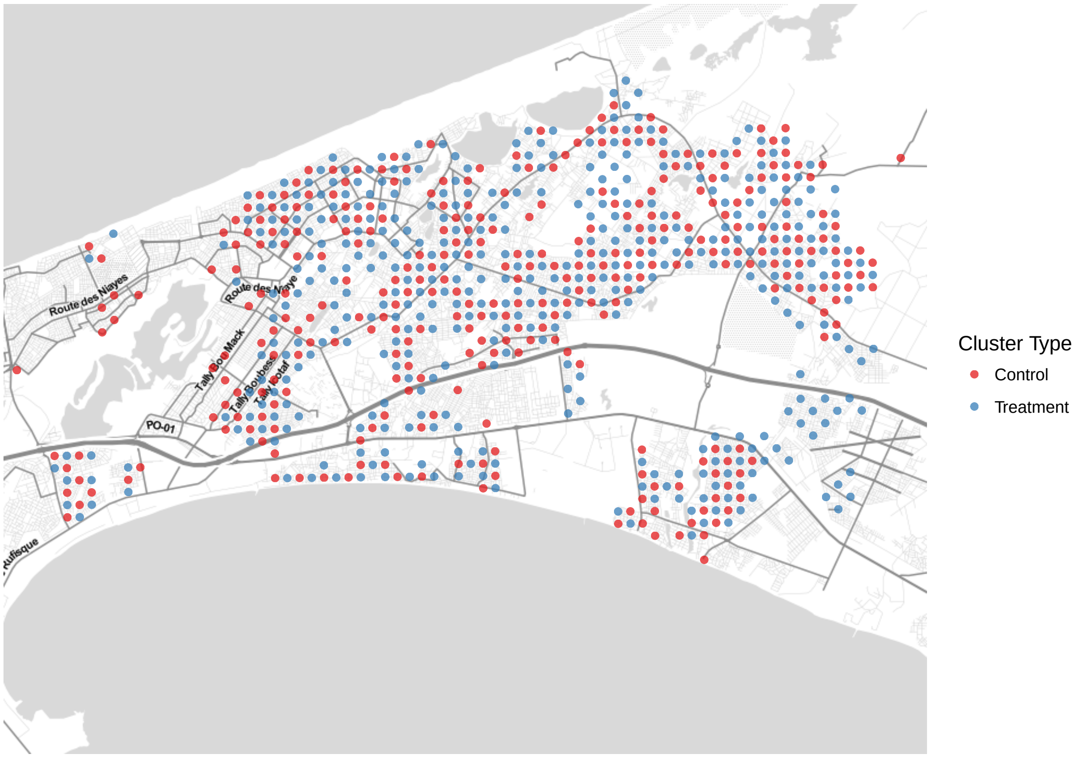
[6]As Figure 1 shows, in some areas of the city only treated or control grid points were ultimately surveyed, whereas the areas in between were not surveyed in the companion survey. In the primary analysis, I exclude these since I am not able to include any direct neighbors of the other status. In Appendix A I present results using the "full" set of grid points. These results are ultimately quite similar to those using the more restricted set of "dense" grid points.

[7]The "high" subsidy of Lipscomb and Schechter (2018) was 17,000 CFA, whereas the 2017 subsidy campaign advertised a fixed price of 16,500 CFA.

[8]Due to a change in overall government sanitation strategy, ONAS elected to close the call center service 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.

[9]This baseline census was conducted by Water and Sanitation for Africa. It involved a simple mapping exercise to establish areas of the city not connected to the sewage network and develop a database of names,

Figure 1: Location of sampled grid points in Dakar



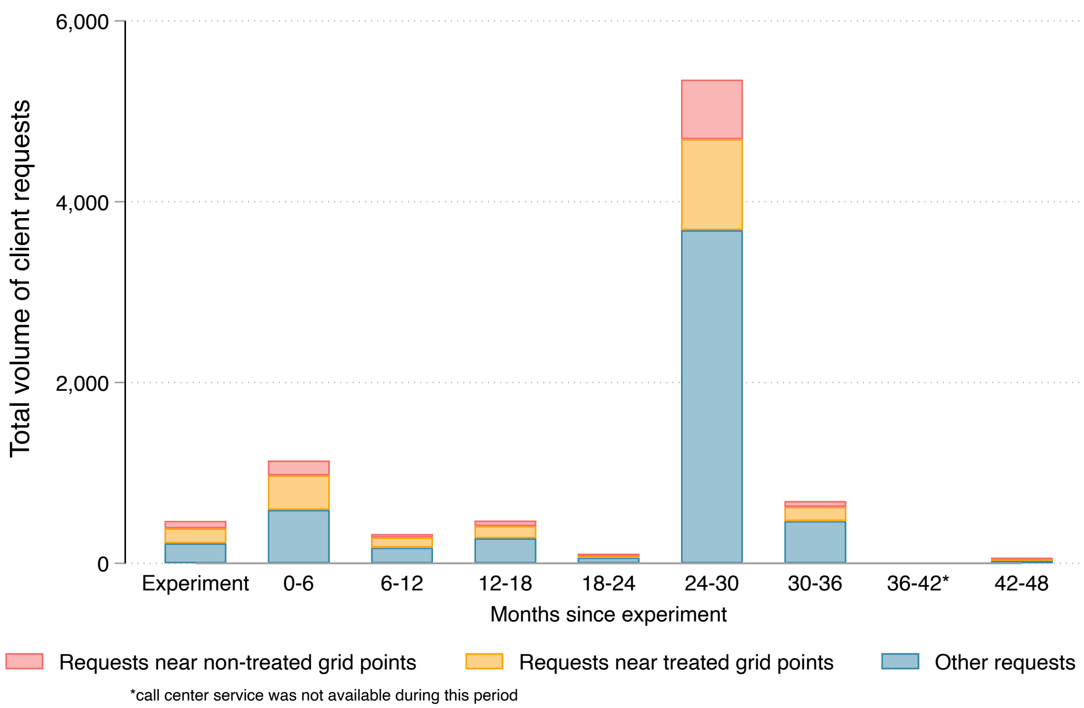
geo-located using a system of landmarks, which matches households to the closest major landmark.[10] I match each household service request to the nearest grid point. In the main analysis below, I restrict the sample to requests within 100 meters of a grid point, since this exactly bisects the distance between treated and non-treated grid points.[11] Figure A.2 demonstrates visually how I attribute households in the data to nearby grid points. Figure 2 shows the total volume of requests handled by the platform in each six-month period, with requests categorized as "near" treated or non-treated grid points following this attribution strategy.

phone numbers, and locations of households in the city with septic pits. The census mapped approximately 65,000 households, which formed the initial database of prospective clients for the call center platform. Any callers whose phone numbers did not match to an existing entry were added as a new client, with location determined by their closest landmark.

[10]This landmark system is a core feature of the call center's underlying auction platform, described in Deutschmann et al. (2021a). Because Dakar does not have a popularly-used system of addresses, this is the primary means by which a call center operator can record a household's location for service provision. The platform database includes more than 2000 landmarks, and the median distance from a household with precise coordinates to the nearest landmark is 93 meters. Results shown below are similar when restricted to households with precise coordinates.

[11]In Appendix A I demonstrate how my results compare if I consider different distance cutoffs.

Figure 2: Total volume of household requests per six-month period



This figure shows the total volume of household requests for desludgings made through the intermediation 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. Requests are categorized as "near" treated and non-treated grid points based on a 100m meter radius around grid points, consistent with the primary radius used for analysis in this paper.

3 Empirical Strategy

In this section, I briefly discuss the empirical strategy and identification assumption. I conduct empirical analysis at the grid point level using administrative data on all household requests for mechanized desludgings recorded in the intermediation platform. I assign household requests to the closest grid point using their location information, as described above in Section 2. 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.[12] For each outcome of interest, I estimate the following equation:

$$Y_{jzt} = \alpha + \sum_{k=0}^{8} \left(\lambda_k \mathbf{1}[k=t] + \zeta_k (T_j \times \mathbf{1}[k=t])\right) + \eta_j + \epsilon_j \tag{1}$$

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. I additionally analyze separately the number of first time and repeat users of the platform. In my preferred specification, I include grid point fixed effects (η_j) to account for neighborhood-level, non-time-varying differences in use of mechanized desludging due to location, neighborhood accessibility, and baseline wealth. Results in the main specifications include standard errors ϵ_{jz} clustered at the grid point level. Identification of the key coefficients of interest, ζ_k, rests primarily on the assumption 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 sampling methodology described above to identify study areas.

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 Table 1, I present results showing the per-period intention-to-treat effects of exposure to the experimental subsidies on subsequent use of the platform. Column 1 reports results from a linear probability model in which the outcome variable is a dummy equal to one if any households from the grid point neighborhood called to use the platform. Figure 3a presents these same results in graphical form. For the 18 months following the cessation of the initial experimental subsidies, platform use remains persistently higher in treated areas relative to non-treated areas. Subsequently, when the

[12] The original subsidy campaign of Lipscomb and Schechter (2018) was twelve months long, but for comparability with the later campaign I consider only the last six months of the subsidized period as the first time period of interest. Most subsidized desludgings during the experiment occurred during this period. The subsequent city-wide campaign ran for six months and launched almost exactly two years after the conclusion of the experimental subsidies.

city-wide subsidy campaign begins 24 months after the experiment, treated areas are again more likely to have any households calling to use the platform, and this effect also persists for the six months following the city-wide campaign.

In Column 2 of Table 1 and in Figure 3b, I show intensive-margin results on the number of household desludging requests recorded in the platform administrative data. Consistent with results at the extensive margin, treated neighborhoods exhibit persistently higher household interest in using the platform for the first 18 months after subsidies end, and again when city-wide subsidies become available. Over the entire post-subsidy period, the average treated neighborhood had nearly twice as many calls as the average non-treated neighborhood. This suggests that a short-term analysis of the effect of the subsidies on platform use would dramatically understate the total gains in adoption.

Because this paper relies only on administrative data from the platform, I cannot say with certainty how many of these calls displaced a manual desludging. Deutschmann et al. (2021b) estimate that every averted manual desludging in a neighborhood in Dakar could reduce the incidence of diarrhea among neighborhing households by 30%.[13] If even a small fraction of the increased desludgings sourced through the platform represent a displaced manual desludging, this could lead to a substantial improvement in neighborhood-level health outcomes.

4.1 New and Repeat Users

Beyond the main effects shown above in Table 1, it is illustrative to consider separately the behavior of first time platform users and repeat callers. If the effects above are driven largely by repeat customers, this may suggest the subsidies were primarily effective at shifting longer-run behavior among receiving households. If, instead, results are driven at least partially by persistent 'new' interest in neighborhoods, this would be consistent with households learning from others in their neighborhood about using the platform to access a mechanized desludging.

To test this, I present results in Table 2 and Figure 4 where each client call is classified as a first time or repeat request. The first time a household appears in the administrative data, I consider this a new request. Any subsequent requests from that household are flagged as repeat requests.

In the six months after the experimental subsidies, roughly half of the increase in client requests in treated neighborhoods is driven by first-time users of the call center. Over the subsequent eighteen months, the proportion of demand driven by repeat users increases. However, when the city-wide subsidy campaign begins, we again see that about half of the increased demand in previously-treated areas is driven by entirely new users of the platform.

[13] Johnson and Lipscomb (2021) find similarly large reductions in Ouagadougou, Burkina Faso, with neighborhood-level diarrhea incidence among children reducing significantly as more households switch from manual to mechanized desludging.

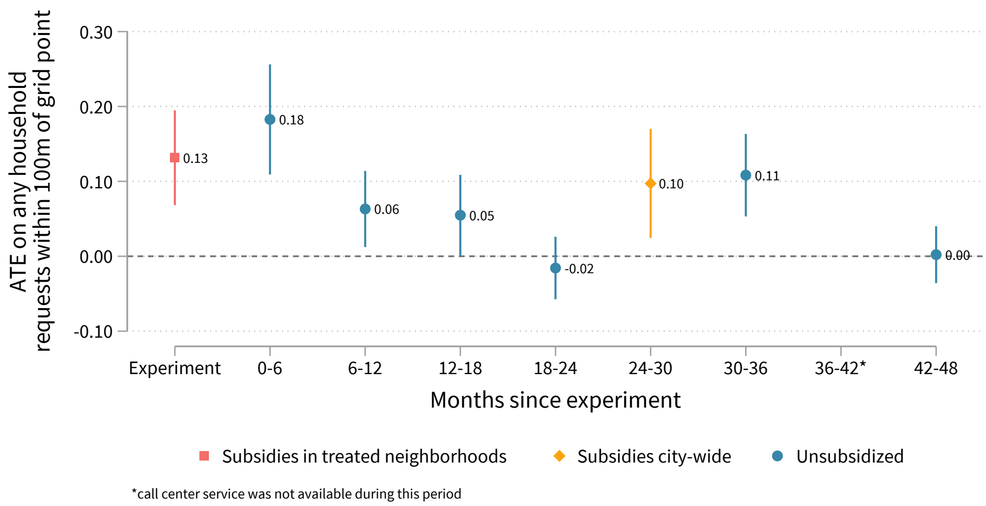
Table 1: Call center use by period during and after experimental subsidies

	(1) Any Requests	(2) Number of Requests
Treated × experimental subsidies	0.132***	0.214***
	(0.032)	(0.060)
Treated × 0-6 months post-subsidies	0.183***	0.585***
	(0.037)	(0.099)
Treated × 6-12 months post-subsidies	0.063**	0.196***
	(0.026)	(0.063)
Treated × 12-18 months post-subsidies	0.055**	0.168**
	(0.027)	(0.069)
Treated × 18-24 months post-subsidies	-0.016	-0.007
	(0.021)	(0.039)
Treated × city-wide subsidies	0.097***	1.084***
	(0.037)	(0.415)
Treated × 30-36 months post-subsidies	0.108***	0.277***
	(0.028)	(0.054)
Treated × 42-48 months post-subsidies	0.002	0.024
	(0.019)	(0.032)
0-6 months post-subsidies	0.086***	0.193***
	(0.026)	(0.055)
6-12 months post-subsidies	-0.069***	-0.110**
	(0.023)	(0.048)
12-18 months post-subsidies	-0.055**	-0.055
	(0.023)	(0.051)
18-24 months post-subsidies	-0.100***	-0.166***
	(0.022)	(0.043)
city-wide subsidies	0.155***	1.266***
	(0.030)	(0.241)
30-36 months post-subsidies	-0.034	-0.090**
	(0.023)	(0.038)
42-48 months post-subsidies	-0.107***	-0.183***
	(0.021)	(0.040)
Observations	5886	5886
Number of grid points	654	654
Non-treated baseline mean	0.138	0.228
Fixed effects	Grid Point	Grid Point

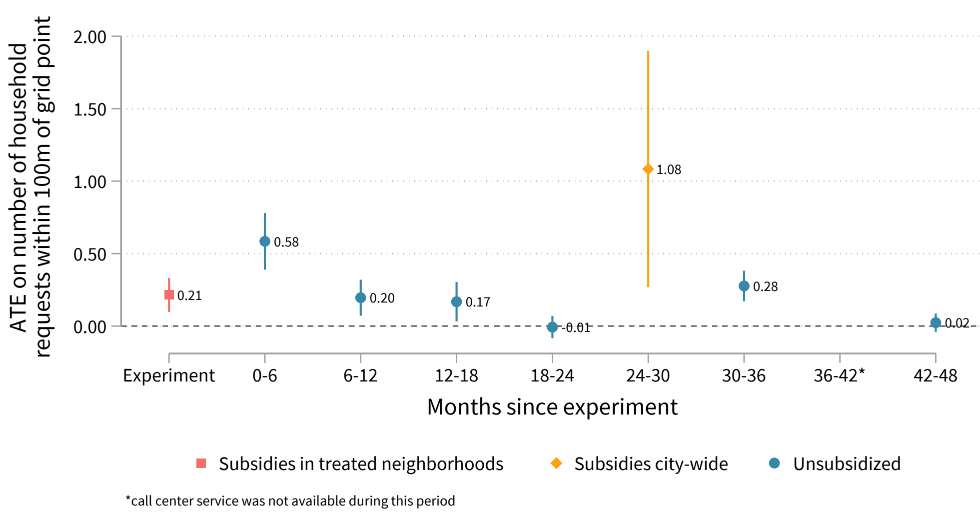
Results in this table are from linear regressions of the outcome variables (shown at the top of each column) on the treatment dummy and time period dummies. All regressions include grid point fixed effects. Non-treated baseline mean is the mean of the outcome variable among non-treated grid points in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

Figure 3: Marginal effects of treatment exposure by period

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

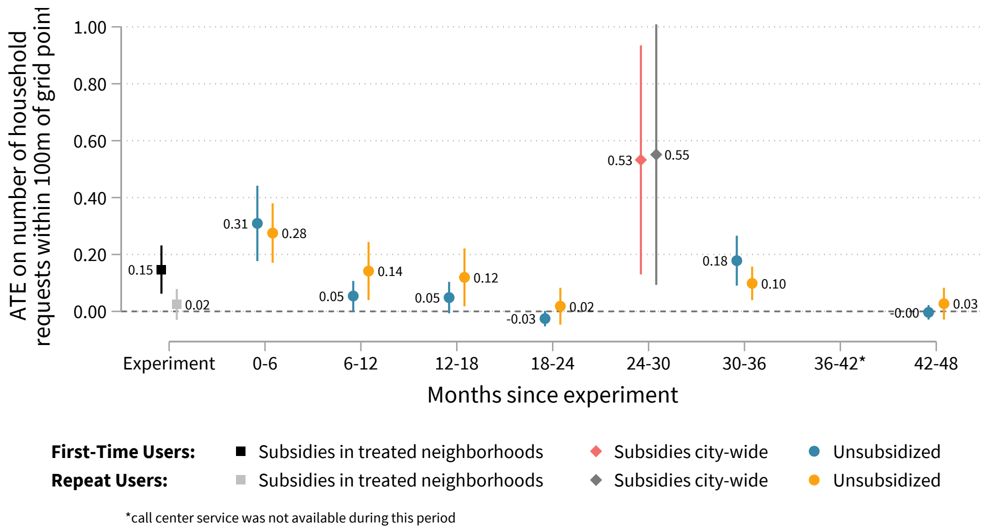


(b) Impact of experimental subsidies on volume of calls from a neighborhood



Results shown in these figures are the per-period treatment effect estimates, shown above in equation (1) as ζ_k. Figure 3a matches column 1 of Table 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 3b matches column 2 of Table 1 and shows estimates in which the outcome is the number of calls 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.

Figure 4: Impact of experimental subsidies on volume of first-time and repeat platform users from a neighborhood



Results shown in these figures are the per-period treatment effect estimates, shown above in equation (1) as ζ_k. 'First-Time User' coefficients match column 1 of Table 2, in which the outcome is the number of households in the area around grid point j who called the platform for the first time in period t. 'Repeat User' coefficients match column 2 of Table 2, in which the outcome is the number of households in the area around grid point j who called the platform in period t and had previously used the service. All regressions include grid point and time period fixed effects, with standard errors clustered at the grid point level.

These results suggest that a sizable fraction of the persistent increase in platform adoption in previously-treated neighborhood may be driven by spillovers to neighbors of past users. Short-run subsidies appear to shift adoption at the community level, not just among direct recipients.

4.2 Spatial Heterogeneity

Next, I present results on spatial heterogeneity within grid point neighborhoods. These results may be suggestive of the underlying mechanism driving the persistence results presented above. If we observe that platform use is less persistent in the periphery of treated neighborhoods, this would be consistent with decision spillovers in overall adoption of mechanized desludging that gradually disappear in the absence of subsidies. If instead call center use is consistent across these treated neighborhoods, then this may suggest that the persistence is driven more by individual substitution from other means of finding a mechanized desludging.

To test this, I present results estimated separately for the "core" and "periphery" of each grid-point neighborhood. The median distance from households in the experiment of Lipscomb and Schechter (2018) to the nearest grid point is 50 meters. Using this as a guide, I define the core and periphery of grid-point neighborhoods as households falling within a 50 meter radius and between 50 and 100 meters from the grid point, respectively. I present

Table 2: Call center use by new and repeat callers

	(1) First-Time Users	(2) Repeat Users
Treated × experimental subsidies	0.147*** (0.043)	0.024 (0.028)
Treated × 0-6 months post-subsidies	0.309*** (0.067)	0.275*** (0.053)
Treated × 6-12 months post-subsidies	0.054** (0.027)	0.142*** (0.052)
Treated × 12-18 months post-subsidies	0.048* (0.028)	0.119** (0.052)
Treated × 18-24 months post-subsidies	-0.025* (0.014)	0.018 (0.033)
Treated × city-wide subsidies	0.532*** (0.205)	0.551** (0.233)
Treated × 30-36 months post-subsidies	0.178*** (0.045)	0.098*** (0.030)
Treated × 42-48 months post-subsidies	-0.003 (0.013)	0.027 (0.028)
0-6 months post-subsidies	0.121*** (0.039)	0.117*** (0.024)
6-12 months post-subsidies	-0.110*** (0.033)	0.045** (0.021)
12-18 months post-subsidies	-0.097*** (0.032)	0.086*** (0.022)
18-24 months post-subsidies	-0.152*** (0.031)	0.031*** (0.011)
city-wide subsidies	0.617*** (0.125)	0.693*** (0.133)
30-36 months post-subsidies	-0.086*** (0.030)	0.041*** (0.013)
42-48 months post-subsidies	-0.166*** (0.031)	0.028** (0.011)
Observations	5886	5886
Number of grid points	654	654
Non-treated baseline mean	0.183	0.000
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. First-Time Users is the number of requests from a given neighborhood for which a household first appeared in the data. Repeat Users is the number of requests from a neighborhood by households which had previously used the platform. 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.

the results of this exercise in Table 3.

Comparing columns 1 and 2 in Table 3, one can see that the treatment effect in the periphery of the neighborhood declines more quickly to become statistically indistinguishable from zero twelve months after the conclusion of the experiment. By contrast, treatment effects in the core areas remain more consistent in both magnitude and statistical significance. Results at the intensive margin, in columns 3 and 4, generally match this story. Figure 5 presents these results graphically.

It is difficult to know how much these results do indeed represent changes in underlying household behavior, given that I rely only on administrative data to conduct this analysis. Nevertheless, these results are consistent with the idea that decision spillovers play a role in driving persistent long-run adoption of the call center. This is reinforced by results above that demand increases are driven in part by new platform users. Coordination and decision spillovers seem to play an important role in driving community-level changes in sanitation outcomes (Bennett, 2012; Deutschmann et al., 2021b). Households in this context typically desludge no more than once every six months. Given the pattern of gradual disadoption which occurs more quickly at neighborhood boundaries, it may be the case that, over time, the marginal household in the periphery no longer finds it worthwhile to choose a mechanized desludging if an insufficient number of neighbors have recently done so. This is consistent with findings in other contexts studying Community-Led Total Sanitation programs, in which community-level sanitation gains persist in the short run but may not persist in the long run (Tyndale-Biscoe et al., 2013; Crocker et al., 2017; Orgill-Meyer et al., 2019). By contrast, in the core of neighborhoods treated with subsidies, a larger fraction of households may have changed their behavior at once, increasing the persistence of mechanized adoption.

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 gaps 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.1 and A.2 I present results using two alternative radii (75 meters and 125 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 restrict analysis to "dense" grid points, since in some areas of the city only treated or non-treated grid points ended up being surveyed. In Columns 4-6 of Tables A.1 and A.2, I present results using the full set of grid points. Using this alternative sample definition, there are mild changes in the magnitude of coefficients but qualitatively similar results.

Finally, despite the plausibly random assignment of treatment status, the strategy of assigning every other grid point to treatment status means that the treatment status of

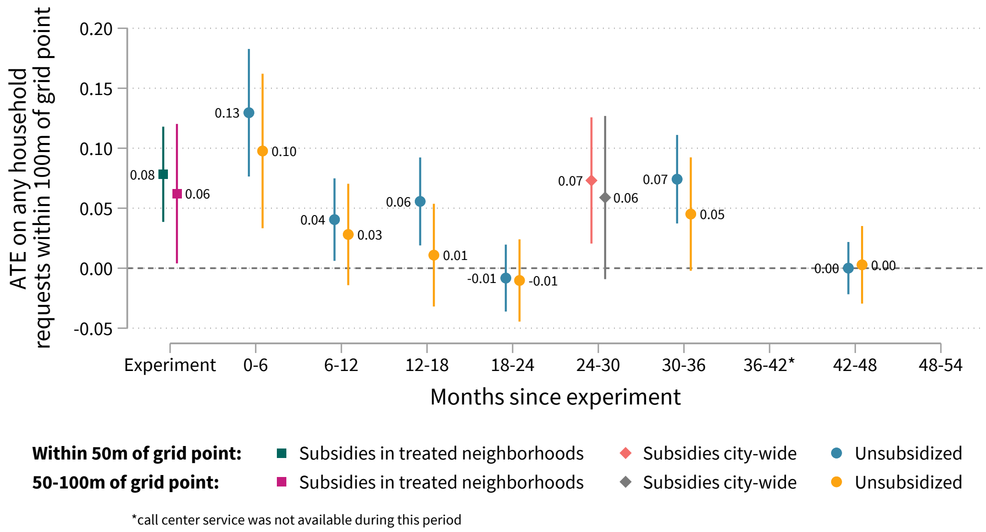
Table 3: Call center use by period during and after experimental subsidies, with neighborhood core and periphery considered separately

	Any Requests		Number of Requests	
	(1)	(2)	(3)	(4)
	0-50m	50-100m	0-50m	50-100m
Treated × experimental subsidies	0.078***	0.062**	0.105***	0.109**
	(0.020)	(0.030)	(0.032)	(0.050)
Treated × 0-6 months post-subsidies	0.130***	0.098***	0.320***	0.264***
	(0.027)	(0.033)	(0.062)	(0.077)
Treated × 6-12 months post-subsidies	0.041**	0.028	0.105***	0.091*
	(0.018)	(0.022)	(0.040)	(0.047)
Treated × 12-18 months post-subsidies	0.056***	0.011	0.117***	0.051
	(0.019)	(0.022)	(0.044)	(0.054)
Treated × 18-24 months post-subsidies	-0.008	-0.010	-0.008	0.001
	(0.014)	(0.017)	(0.024)	(0.029)
Treated × city-wide subsidies	0.073***	0.059*	0.488***	0.595*
	(0.027)	(0.035)	(0.189)	(0.344)
Treated × 30-36 months post-subsidies	0.074***	0.045*	0.144***	0.133***
	(0.019)	(0.024)	(0.034)	(0.043)
Treated × 42-48 months post-subsidies	0.000	0.003	-0.005	0.028
	(0.011)	(0.017)	(0.017)	(0.026)
0-6 months post-subsidies	0.045***	0.041*	0.076**	0.117**
	(0.017)	(0.024)	(0.030)	(0.048)
6-12 months post-subsidies	-0.003	-0.076***	0.000	-0.110***
	(0.012)	(0.021)	(0.022)	(0.040)
12-18 months post-subsidies	-0.010	-0.059***	-0.007	-0.048
	(0.011)	(0.021)	(0.021)	(0.044)
18-24 months post-subsidies	-0.007	-0.103***	-0.010	-0.155***
	(0.012)	(0.020)	(0.019)	(0.037)
city-wide subsidies	0.079***	0.124***	0.359***	0.907***
	(0.019)	(0.029)	(0.097)	(0.211)
30-36 months post-subsidies	-0.007	-0.038*	-0.014	-0.076**
	(0.012)	(0.022)	(0.018)	(0.032)
42-48 months post-subsidies	-0.021**	-0.100***	-0.028*	-0.155***
	(0.010)	(0.020)	(0.016)	(0.035)
Observations	5886	5886	5886	5886
Number of grid points	654	654	654	654
Non-treated baseline mean	0.024	0.128	0.034	0.193

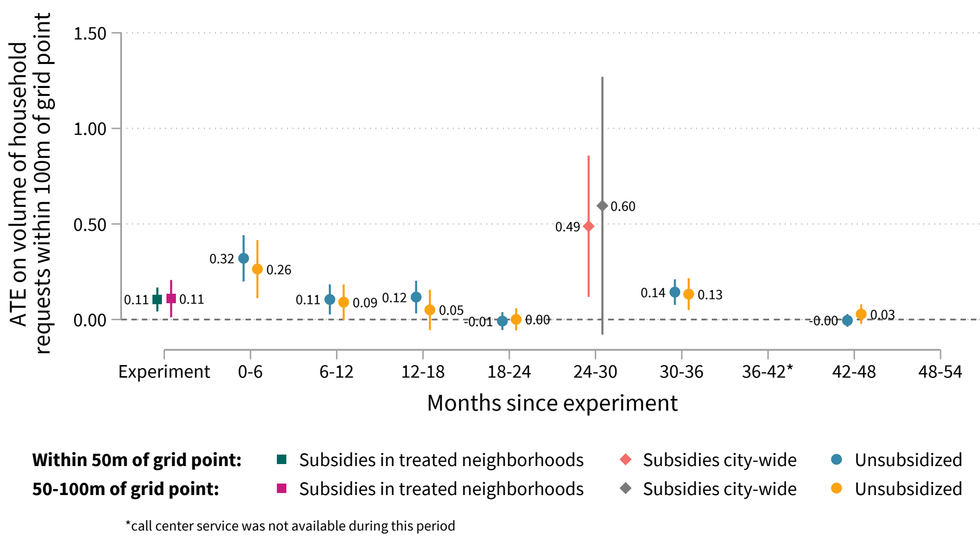
Results in this table are from linear regressions of the outcome variables (shown at the top of each column) on the treatment dummy and time period dummies. Columns 1 and 3 consider the area within 50 meters of grid points, whereas columns 2 and 4 consider the area between 50 and 100 meters from grid points. 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.

Figure 5: Marginal effects of treatment exposure by period, in core and periphery of grid point neighborhoods

(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 ζ_k. Figure 5a matches columns 1 and 2 of Table 3 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 5b matches columns 3 and 4 of Table 3 and shows estimates in which the outcome is the number of calls 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.

a given grid point is perfectly negatively correlated with its immediate neighbors. This complicates the use of standard randomization inference procedures (Young, 2019) as there are only two possible treatment assignments that maintain both the spatial correlation and the location of grid points. I proceed with two exercises in the spirit of randomization inference, in which I relax in turn the maintained spatial correlation and the fixed location of grid points. The results of these exercises are shown in Table A.3.

First, I fix the set of grid points as in the experiment, but relax the "every-other-grid-point" treatment assignment. Instead, I conduct a simple randomization inference procedure in which counterfactual treatment assignments can have any spatial correlation, conditional on the set of grid points included in the analysis. The results of these procedures, shown in square brackets in Table A.3, are not substantially different from the conventional p-values from the main regression analysis.

Second, I fix the spatial *structure* of treatment assignment, and randomly shift the set of grid points by up to 100 meters in any direction. For a given counterfactual set of grid points, I repeat the procedure of assigning households to neighborhoods (illustrated above in Figure A.2) and create the resulting counterfactual cluster-level panel. Results of these procedures are shown in curly brackets in Table A.3. In general, they mirror the previous randomization inference exercise and the conventional p-values, with several exceptions where the p-values from this procedure exceed conventional levels of significance in contrast to results from the other procedures. Nevertheless, the qualitative interpretation of my results generally holds.

5 Conclusion

In this paper, I have shown that short-run subsidies had lasting impacts on consumer adoption of a call center platform for desludging services in Dakar, Senegal. This is an important market to study consumer decisions, given the potential for substantial health externalities when households desludge their pits manually.

My paper has implications for the design of optimal sanitation policy. In this case, previous exposure to subsidies increased use of the call center platform both when it was and was not subsidized. As previous work has shown, this is not universally true in other health and sanitation contexts. For policymakers interested in increasing adoption of intermediation platforms, and for firms seeking to establish these platforms, my results suggest a role for short-run subsidies. Taken together with the results of Johnson and Lipscomb (2021), targeted short-term discounts for the poorest households may be a cost-effective strategy for inducing longer-run behavioral change, perhaps re-occurring periodically to reinforce the longer-run change. Furthermore, I show that these effects of these subsidies may spill over within neighborhoods, inducing new households to adopt the platform in addition to sparking persistent changes in demand among recipients. Subsidies may also be particularly effective when geographically concentrated, as suggested by the spatial heterogeneity I observe in my results.

Future work could further explore the link between adoption of intermediation platforms and switching towards the underlying technology. Do intermediation platforms represent an opportunity to reduce behaviors with costly health externalities, or do they primarily capture interest from consumers who would already have chosen the more sanitary option? The data used for this paper do not permit me to conclude with certainty that the increase in platform adoption represents an overall reduction in manual desludging. Nevertheless, the magnitude of the change in adoption would represent a substantial improvement in health conditions in previously-subsidized neighborhood if it did correspond to changes in overall desludging behavior.

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A Additional Tables and Figures

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

Table A.1: Robustness table: Call center 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)
	100m	75m	125m	100m	75m	125m
Treated × experimental subsidies	0.132***	0.117***	0.174***	0.123***	0.114***	0.170***
	(0.032)	(0.027)	(0.035)	(0.029)	(0.024)	(0.031)
Treated × 0-6 months post-subsidies	0.183***	0.165***	0.194***	0.171***	0.160***	0.176***
	(0.037)	(0.033)	(0.040)	(0.034)	(0.029)	(0.036)
Treated × 6-12 months post-subsidies	0.063**	0.069***	0.062**	0.054**	0.059***	0.050*
	(0.026)	(0.022)	(0.029)	(0.023)	(0.020)	(0.026)
Treated × 12-18 months post-subsidies	0.055**	0.059**	0.055*	0.047*	0.050**	0.051*
	(0.027)	(0.023)	(0.031)	(0.025)	(0.021)	(0.028)
Treated × 18-24 months post-subsidies	-0.016	-0.010	-0.005	-0.015	-0.009	-0.007
	(0.021)	(0.018)	(0.025)	(0.019)	(0.016)	(0.022)
Treated × city-wide subsidies	0.097***	0.094***	0.137***	0.091***	0.099***	0.121***
	(0.037)	(0.034)	(0.038)	(0.034)	(0.031)	(0.035)
Treated × 30-36 months post-subsidies	0.108***	0.092***	0.128***	0.090***	0.083***	0.106***
	(0.028)	(0.023)	(0.031)	(0.025)	(0.021)	(0.028)
Treated × 42-48 months post-subsidies	0.002	0.006	0.017	-0.015	-0.006	-0.001
	(0.019)	(0.015)	(0.022)	(0.018)	(0.014)	(0.020)
0-6 months post-subsidies	0.086***	0.062***	0.121***	0.074***	0.054***	0.116***
	(0.026)	(0.022)	(0.028)	(0.023)	(0.019)	(0.025)
6-12 months post-subsidies	-0.069***	-0.048***	-0.055**	-0.062***	-0.042***	-0.048**
	(0.023)	(0.018)	(0.026)	(0.020)	(0.016)	(0.022)
12-18 months post-subsidies	-0.055**	-0.038**	-0.038	-0.051**	-0.034**	-0.034
	(0.023)	(0.019)	(0.025)	(0.020)	(0.016)	(0.022)
18-24 months post-subsidies	-0.100***	-0.048***	-0.103***	-0.093***	-0.045***	-0.096***
	(0.022)	(0.017)	(0.023)	(0.019)	(0.015)	(0.020)
city-wide subsidies	0.155***	0.128***	0.169***	0.159***	0.125***	0.181***
	(0.030)	(0.026)	(0.031)	(0.026)	(0.023)	(0.028)
30-36 months post-subsidies	-0.034	-0.024	-0.017	-0.028	-0.025*	-0.008
	(0.023)	(0.017)	(0.026)	(0.020)	(0.015)	(0.023)
42-48 months post-subsidies	-0.107***	-0.059***	-0.121***	-0.093***	-0.051***	-0.105***
	(0.021)	(0.016)	(0.022)	(0.018)	(0.014)	(0.019)
Observations	5886	5886	5886	6867	6867	6867
Number of grid points	654	654	654	763	763	763
Non-treated baseline mean	0.138	0.076	0.155	0.125	0.068	0.139

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. Columns 1 and 4 use the preferred 100m radius to define grid point neighborhoods, whereas columns 2 and 5 use 75m and columns 3 and 6 use 125m 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.2: 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)
	100m	75m	125m	100m	75m	125m
Treated ×	0.214***	0.194***	0.313***	0.201***	0.182***	0.300***
experimental subsidies	(0.060)	(0.047)	(0.074)	(0.054)	(0.042)	(0.065)
Treated ×	0.585***	0.508***	0.718***	0.555***	0.478***	0.651***
0-6 months post-subsidies	(0.099)	(0.081)	(0.118)	(0.091)	(0.075)	(0.109)
Treated ×	0.196***	0.177***	0.241***	0.169***	0.149***	0.203***
6-12 months post-subsidies	(0.063)	(0.051)	(0.078)	(0.056)	(0.045)	(0.068)
Treated ×	0.168**	0.137***	0.239**	0.182***	0.136***	0.256***
12-18 months post-subsidies	(0.069)	(0.052)	(0.095)	(0.066)	(0.051)	(0.087)
Treated ×	-0.007	-0.008	0.042	-0.004	-0.009	0.036
18-24 months post-subsidies	(0.039)	(0.033)	(0.051)	(0.035)	(0.029)	(0.044)
Treated ×	1.084***	1.008***	1.452**	1.018***	0.918***	1.381***
city-wide subsidies	(0.415)	(0.313)	(0.587)	(0.383)	(0.280)	(0.532)
Treated ×	0.277***	0.206***	0.302***	0.256***	0.191***	0.279***
30-36 months post-subsidies	(0.054)	(0.045)	(0.068)	(0.052)	(0.041)	(0.063)
Treated ×	0.024	0.019	0.081*	-0.005	-0.000	0.044
42-48 months post-subsidies	(0.032)	(0.025)	(0.045)	(0.029)	(0.023)	(0.039)
0-6 months	0.193***	0.107***	0.262***	0.173***	0.105***	0.269***
post-subsidies	(0.055)	(0.039)	(0.068)	(0.049)	(0.037)	(0.064)
6-12 months	-0.110**	-0.062*	-0.107*	-0.102**	-0.057**	-0.093*
post-subsidies	(0.048)	(0.032)	(0.059)	(0.041)	(0.027)	(0.051)
12-18 months	-0.055	-0.041	-0.031	-0.054	-0.034	-0.031
post-subsidies	(0.051)	(0.033)	(0.063)	(0.043)	(0.029)	(0.053)
18-24 months	-0.166***	-0.062**	-0.207***	-0.156***	-0.059**	-0.190***
post-subsidies	(0.043)	(0.031)	(0.051)	(0.037)	(0.026)	(0.044)
city-wide subsidies	1.266***	0.617***	1.679***	1.232***	0.589***	1.649***
	(0.241)	(0.117)	(0.300)	(0.220)	(0.106)	(0.268)
30-36 months	-0.090**	-0.045*	-0.041	-0.074**	-0.045**	-0.025
post-subsidies	(0.038)	(0.026)	(0.064)	(0.033)	(0.023)	(0.054)
42-48 months	-0.183***	-0.083***	-0.238***	-0.161***	-0.074***	-0.207***
post-subsidies	(0.040)	(0.027)	(0.048)	(0.034)	(0.023)	(0.040)
Observations	5886	5886	5886	6867	6867	6867
Number of grid points	654	654	654	763	763	763
Non-treated baseline mean	0.228	0.110	0.286	0.207	0.099	0.255

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. Columns 1 and 4 use the preferred 100m radius to define grid point neighborhoods, whereas columns 2 and 5 use 75m and columns 3 and 6 use 125m 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.3: Call center use by period during and after experimental subsidies, with randomization inference p-values

	(1) Any Requests	(2) Number of Requests
Treated ×	0.13***	0.21***
experimental subsidies	(0.030)	(0.060)
	[0.00]	[0.00]
	{0.03}	{0.18}
Treated ×	0.18***	0.58***
0-6 months post-subsidies	(0.040)	(0.100)
	[0.00]	[0.00]
	{0.02}	{0.02}
Treated ×	0.06**	0.20***
6-12 months post-subsidies	(0.030)	(0.060)
	[0.01]	[0.03]
	{0.17}	{0.08}
Treated ×	0.05**	0.17**
12-18 months post-subsidies	(0.030)	(0.070)
	[0.04]	[0.05]
	{0.33}	{0.21}
Treated ×	-0.02	-0.01
18-24 months post-subsidies	(0.020)	(0.040)
	[0.37]	[0.94]
	{0.67}	{0.92}
Treated ×	0.10***	1.08***
city-wide subsidies	(0.040)	(0.420)
	[0.00]	[0.02]
	{0.04}	{0.00}
Treated ×	0.11***	0.28***
30-36 months post-subsidies	(0.030)	(0.050)
	[0.00]	[0.00]
	{0.00}	{0.00}
Treated ×	0.00	0.02
42-48 months post-subsidies	(0.020)	(0.030)
	[0.89]	[0.75]
	{0.92}	{0.69}
Observations	5886	5886
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. P-values from two randomization inference procedures (with 250.00 iterations) are shown in square and curly brackets. See Section 4.3 for more on these procedures. Note that the call center was not in operation in the 36-42 month post-subsidy period.

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