Recognizing a good deal:
short-term subsidies and the dynamics of consumer choices

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Abstract

I study the dynamics of consumer choices in response to short-term subsidies. I exploit spatial variation in exposure to subsidies which induced consumers to use a 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, increased adoption re-emerges in previously-subsidized neighborhoods. I explore within-neighborhood spillovers as a mechanism and show that a substantial fraction of increased long-run adoption comes from new users.

Keywords: Technology adoption, matching platforms, environmental quality, urban sanitation, subsidies, development

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1 Introduction

Matching platforms, such as ride-sharing apps 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; Gaineddenova, 2022). In markets for public services, matching platforms may also be an effective tool for addressing externalities exacerbated by market frictions (Johnson and Lipscomb, 2021; Deutschmann et al., 2021a). When launching these services, firms and governments may rely on short-term discounts or subsidies to jump-start consumer adoption.

In this paper, I study the longer-run dynamics of consumer adoption of a matching platform in response to repeated short-term subsidies. In cases when the positive benefits of a technology are easily assessed, a short-run subsidy may be sufficient to increase longer-run household adoption of that technology (Dupas, 2014b; Bensch and Peters, 2020; Carter et al., 2021; Meriggi et al., 2021). However, subsidies may also induce reference dependence among users (Kőszegi and Rabin, 2006), depressing demand at market prices. Understanding how short-term discounts or subsidies affect longer-run household choices is key to designing efficient policy and using limited resources effectively.

The platform I study matches households to providers of septic pit desludgings in Dakar, Senegal. 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. Markets characterized by high search costs, market power, and negotiated prices often exhibit reduced consumer welfare and inefficient matching (Allen et al., 2019; Salz, 2022). These frictions may cause households to rely on manual methods to desludge septic pits, with important negative implications for neighborhood environmental quality and child health.

I exploit quasi-experimental variation in neighborhood-level exposure to subsidies offered to households to identify the longer-run effects on use of the matching platform. In 2013, the Senegalese government launched a call center and matching platform designed to connect households with providers of mechanized desludging services using auctions (Deutschmann et al., 2021a). In 2014, Lipscomb and Schechter (2018) (henceforth, LS) provided subsidies to randomly-selected households in about 400 neighborhoods.[1] In 2017, two years after the subsidies of LS ended, the government ran a major city-wide subsidy and advertising campaign in Dakar intended to increase adoption of improved sanitation services.[2] To access

[1]These subsidies allowed households to purchase a mechanized desludging of their septic pit for a fixed price of about $34 USD, roughly 66% of the average market price. For households without access to subsidies, and for treated households after the subsidies ended, the platform provided each household a price by conducting a just-in-time auction with sanitation service providers. In some areas of the city, these prices were below prices for mechanized desludging services available outside the platform, whereas in others the platform offered prices comparable to the market (Deutschmann et al., 2021a).

[2]The campaign offered households anywhere in the city mechanized desludgings for a fixed, subsidized price of about $33 USD, nearly the same price previously offered during the experiment.

both rounds of subsidies, households were required to call in and use the matching platform.

To identify the impact of the LS subsidies on later use of the platform, I rely on a key feature of the sampling strategy of LS which sampled about 800 grid points from a 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 treated grid point neighborhoods and comparable on observable characteristics at baseline.

I first show that the LS subsidies were effective at increasing consumer adoption of the matching platform while they were available. I find that treated neighborhoods were 98% more likely to have any households call to use the platform than non-treated neighborhoods. This occurred during a period of relatively widespread advertisement of the platform, so the treatment should not have increased awareness of the platform *per se*. Instead, we should interpret these effects during the subsidy period as being primarily driven by the subsidies themselves.

Second, I explore the short-run dynamics of platform use after the LS subsidies were no longer available. I find that in the six months after subsidy availability ended, neighborhoods previously treated were 86% more likely to have any households use the platform to request a mechanized desludging. By eighteen months after the LS subsidies, treated neighborhoods remained 64% more likely to have any households using the platform. Beyond eighteen months after the LS subsidies, adoption in treated neighborhoods faded to be statistically indistinguishable from non-treated neighborhoods. The decline in treatment effects over time suggests that changes in consumer choices may not persist indefinitely absent additional intervention or advertising.

Next, 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. I show that neighborhoods previously offered the LS subsidies were 32% more likely to have any households take advantage of the new round of city-wide subsidies.[4] At the intensive margin, treated neighborhoods made 71% more service requests than non-treated neighborhoods. 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 that the associated increase in advertising for the platform increased the salience of the platform's availability and reminded households to use it.

What explains this persistent platform adoption in previously-subsidized areas? I find suggestive evidence that within-neighborhood spillovers are a mechanism driving longer-run platform adoption. I show that in the six months after the LS subsidies ended, about half of the increased demand in previously-treated areas was from new platform users. This fraction

[3]Throughout the paper, unless otherwise specified I use the term neighborhood to refer to a 100m circle around each grid point, which exactly bisects the distance to the next grid point.

[4]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., 2022).

declines over time, but at eighteen months post-subsidy, about one quarter of the increased demand is still being driven by new platform users. 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 for the platform beyond direct recipients.

To further support spillovers as a mechanism, I show evidence of heterogeneity across space in the persistence of platform adoption. About half of the households offered subsidies by LS fall within 50 meters of the grid point. This 50m "core" represents a fourth of the area of the whole grid point neighborhood. This means the core of the neighborhoods was three times more intensively treated on average, suggesting greater potential to learn from neighbors and observe tangible changes in local environmental quality. I show that use of the platform declines faster at the periphery of treated neighborhoods compared to the core of these neighborhoods.

My findings on spillovers are consistent with work in the literature studying sanitation decisions more generally, with existing evidence suggesting that the health benefits 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). 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). Referrals within neighborhoods can be an important channel to facilitate these spillovers in the short run (Deutschmann et al., 2021b).

This paper contributes to the literature on matching in decentralized markets and consumer platform adoption. The platform I study reduces time costs for consumers 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 markets with high search costs, like residential solar or urban waste management, can improve welfare for both buyers and sellers (Dorsey, 2021; Salz, 2022). 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 need for the service is infrequent. I contribute by showing that consumer use of a matching platform persists after short-term subsidies but declines over time, and that repeated discounts can provide a spark to re-engage with consumers.

I additionally contribute to a broader literature on short-term subsidies and long-run effects in health and sanitation. There is growing interest in studying how short-term interventions impact household behavior in the longer run (Bouguen et al., 2019). In some cases, long run and spillover effects account for a substantial fraction of the overall impacts of a particular intervention (Baird et al., 2016; Ozier, 2018), and persistent long run effects can significantly alter cost effectiveness estimates (Allcott and Rogers, 2014; Nakajima, 2020). The question of whether and how to subsidize technologies, particularly in the presence of health externalities and peer effects, has long been a question of both academic and policy interest.[5] On the one hand, Dupas (2014b) highlights the potential for short-run subsidies to increase long-run demand for experience goods.[6] On the other hand, Fischer et al. (2019) find lower demand for health products following free distribution when the health benefits are not as easily assessed. 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,[7] and that long-run demand increases may be driven in part by spillovers to neighbors of past subsidy recipients.

Finally, this paper builds on work demonstrating the role of price reductions in encouraging households to change sanitation behavior and invest in environmental quality. 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). Similarly, liquidity constraints significantly depress willingness to pay for latrines (Ben Yishay et al., 2017). Johnson and Lipscomb (2021) demonstrate the potential for targeting subsidies through a matching platform to increase mechanized adoption in a highly cost-effective way. Desludging a pit is a regular choice with visible consequences for households and neighborhoods. The platform may facilitate overall increases in mechanized desludging by reducing matching frictions between households and service providers, although my data is not sufficient to detect underlying changes in household sanitation choices. Understanding the potential for learning and past experience to drive long term, persistent behavioral change in household sanitation choices is key to designing optimal sanitation policy. I contribute to this literature by demonstrating the dynamics of consumer matching platform use in response to multiple short-term subsidies, and discuss how funders may use targeted, concentrated subsidies to achieve greater benefits when spillovers impact long-run platform adoption.

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[5] Examples include, but are not limited to, 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). Beyond health and sanitation, Parry and Small (2009) find public transit subsidies to be highly welfare improving at conventional levels, accounting for externalities including congestion, pollution, accident risk, and economies of scale.

[6] 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.

[7] 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 rest of the paper is organized as follows. In Section 2, I describe the context of 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 included 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 elements of this larger sanitation program: the establishment of a sanitation matching platform and 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.

One primary focus of the ONAS sanitation program was to increase household adoption of mechanized desludging services. In 2013, at least 75% of households in Dakar used toilets which are not connected to a sewage network, and instead drain into on-site septic pits (Sene, 2017). These pits fill up and must typically be emptied, or desludged, 1-2 times per year. Households face two main options to desludge their pits. They can 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* 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).

The platform I discuss in this paper was designed to match households to sanitation service providers using an auction system and call center. This platform was primarily 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 platform was to increase competition among mechanized desludging providers, reduce search costs for households, and facilitate regulation of the sector. The majority of trucks active in the sector were registered in the platform, 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 during normal (non-subsidized) operations the resulting auction ending price was offered to that client as the price available through the platform. The mechanized desludging services offered through the platform did not differ in any substantive way from those available elsewhere in the market. In companion work, Deutschmann et al. (2021a) 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. The average cost to hire a truck for a desludging in 2013 was about $50 USD, and the average cost for a *baay pell* manual desludging was about $29 USD (Deutschmann et al., 2021b). Even in areas where prices offered through the platform were not lower than elsewhere in the market, households may have still faced lower search costs or improved bargaining power if they sourced desludgings from elsewhere.

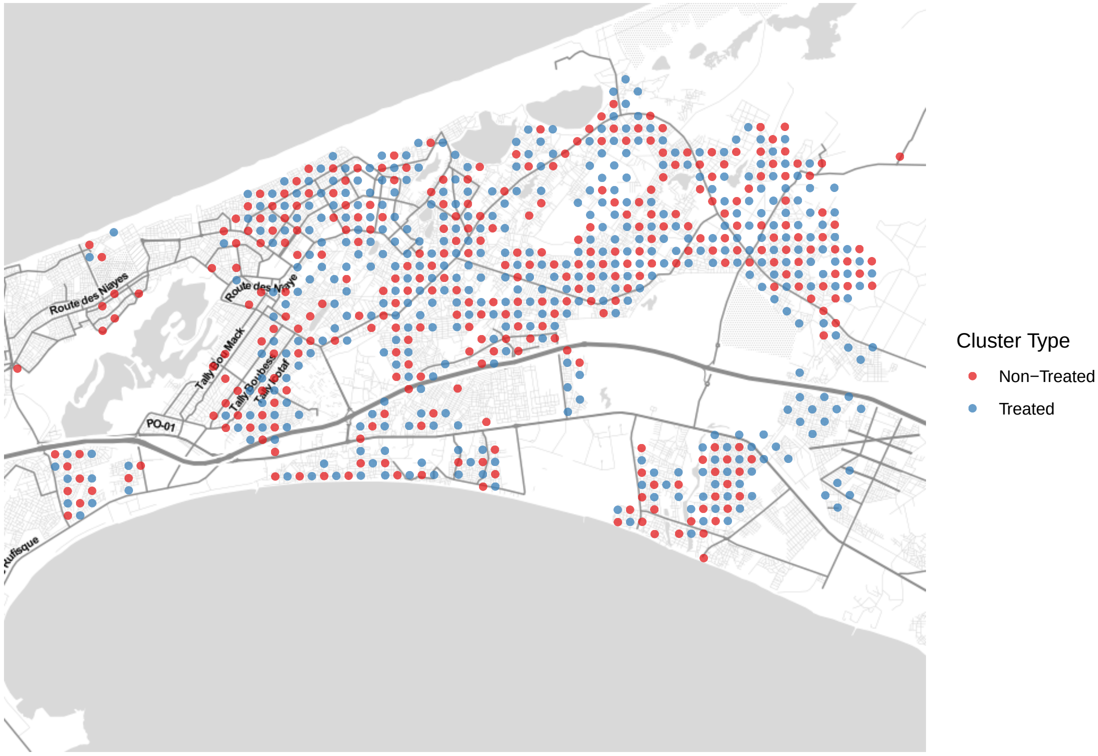
Starting in 2013, researchers conducted a series of experiments in partnership with ONAS to study household demand for mechanized desludging services (Lipscomb and Schechter, 2018; Deutschmann et al., 2021b). These experiments included offering households fixed, subsidized prices to encourage adoption 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 using the matching platform. 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-2014.

Key for this paper is the sampling strategy used to identify subsidized neighborhoods and comparable neighborhoods without any exposure to the experimental subsidies. 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 (the "treated" grid points). The remaining grid points were held out for inclusion in a companion survey without any associated experiment. LS and the companion survey used similar criteria to include or exclude particular grid points, and the final retained sample excludes 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. Figure 1 illustrates the retained set of grid points which were included for each research project. I rely on the locations of these grid points to determine whether a particular area of the city was exposed to experimental subsidies. The surveys conducted with households near treated and non-treated grid points were distinct in time and relied on slightly different criteria for inclusion of a particular household, so I do not directly consider household characteristics from those surveys for my primary empirical strategy.[9]

[8] As Deutschmann et al. (2021b) 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] As Figure 1 shows, in some areas of the city only treated or non-treated grid points were ultimately surveyed, whereas the areas in between were not surveyed in the other survey. In Appendix A I compare results when I use the "full" set of grid points or restrict to the "dense" grid points which all have at least one direct neighbor of the other status. These results are ultimately quite similar.

Figure 1: Location of sampled grid points in Dakar



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.[10] 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 using the matching platform.

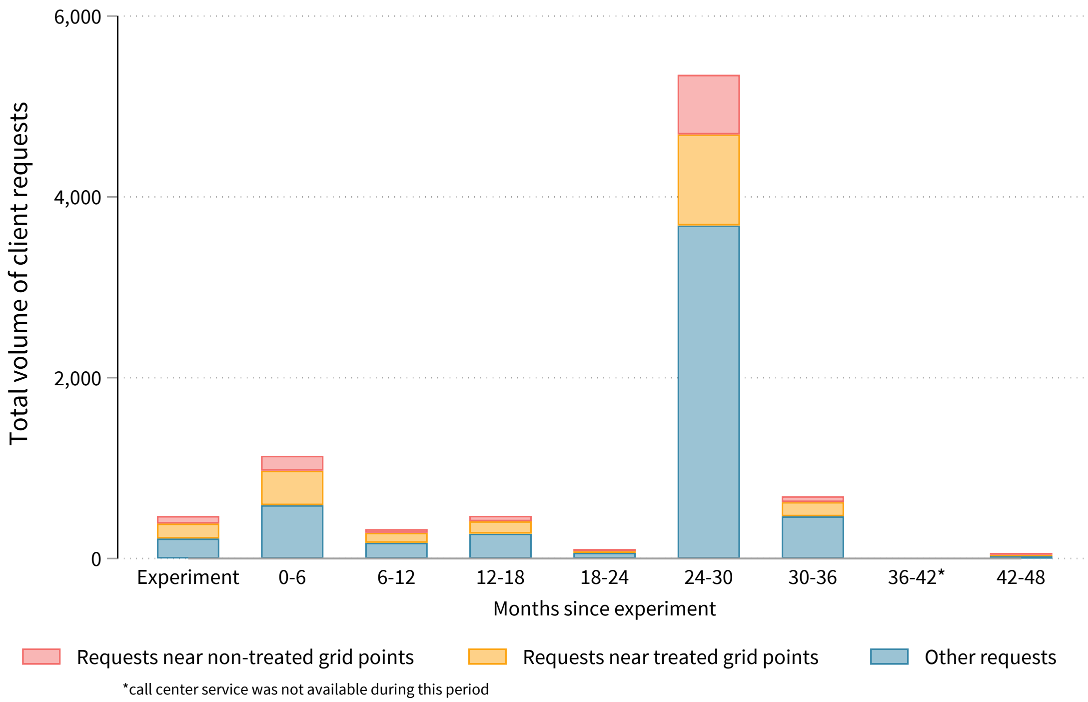
I use administrative data covering the universe of household service requests made to the platform. The platform operated almost continuously from 2013 to 2018, and again in 2019.[11] Households in the data are geo-located using one of two strategies. About one third of platform users match directly to a baseline census conducted in Dakar in 2012, meaning I observe precise coordinates for their location.[12] The remaining two-thirds are

[10]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.

[11]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.

[12]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, 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 platform. Any callers whose phone numbers did not match to an existing entry were added as a new client, with location determined

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 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. 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.

geo-located using a system of landmarks, which matches households to the closest major landmark.[13] 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.[14] 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.

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by their closest landmark.

[13]This landmark system is a core feature of the platform'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 platform 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, although less precisely estimated, when restricted to households with precise coordinates.

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

3 Empirical Strategy

In this section, I briefly discuss the empirical strategy and identification assumptions I use to study the dynamics of matching platform adoption. 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.[15] 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. I additionally analyze separately the number of first time and repeat users of the platform. 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 neighborhood-level, time-invariant differences in use of mechanized desludging due to location, neighborhood accessibility, and baseline wealth. I additionally include time period fixed effects λ_t. Standard errors ϵ_j are clustered at the grid point level. 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 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 sampling methodology described above to identify study areas.[16] In essence, the fact that one set of

[15] 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.

[16] Note this assumption is distinct from the within-treated-grid-points randomization of Lipscomb and

grid points was assigned to the LS 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 Figure 3a, 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. The first column (Treated × Experimental subsidies) demonstrates that there was indeed an increase in platform use when the experimental subsidies were active. For the 18 months following the cessation of the experimental subsidies, platform use remains persistently higher in treated areas relative to non-treated areas. Subsequently, when the city-wide subsidy campaign begins 24 months after the experiment, previously 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 Figure 3b (and in Column 2 of Table A.1), 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 represent adoption of mechanized desludging itself and displacement of manual desludging. The platform did not directly process payments except when facilitating subsidy distribution. Deutschmann et al. (2021b) estimate that the availability of experimental subsidies decreased contemporaneous use of manual desludging by 10%, and that every averted manual desludging in a neighborhood in Dakar could reduce the incidence of diarrhea among neighborhing households by 30%.[17] Further, that paper shows that referrals are an important channel for sourcing mechanized desludgings. If

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 4.2 I discuss several strategies for assessing the robustness of my inference using permutation tests given this particular spatial structure.

[17] 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.

even a small fraction of the increased platform use in previously-treated neighborhoods represents displaced manual desludgings, this could lead to a substantial improvement in neighborhood-level health outcomes.

4.1 Spillovers

Beyond the main effects shown above in Table A.1, it is of interest to consider whether we observe evidence of spillovers within neighborhoods. To explore this, I first 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 subsidy recipients. 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 A.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.

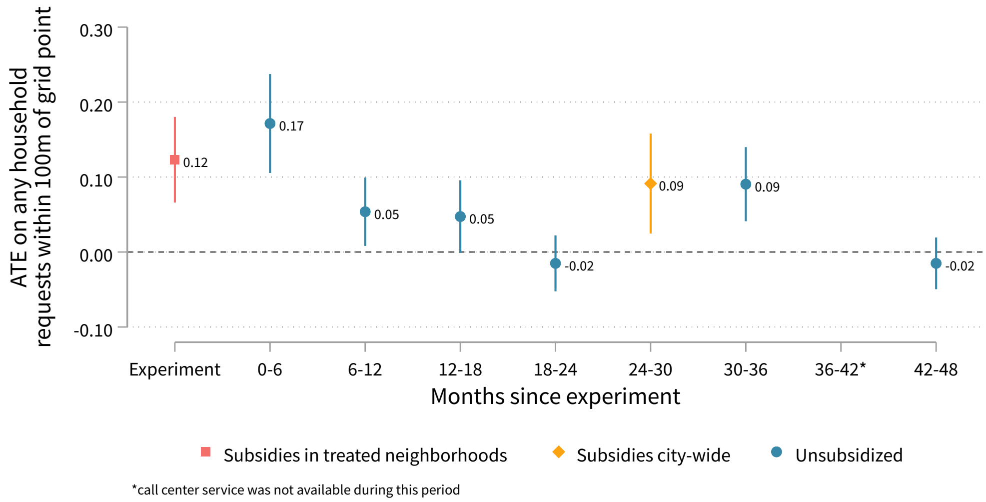
In the six months after the experimental subsidies, roughly half of the increase in client requests in treated neighborhoods is driven by first-time users of the platform. Over the subsequent eighteen months, the proportion of demand driven by repeat users increases. However, when the city-wide subsidy campaign begins, one can again see that about half of the increased demand in previously-treated areas is driven by entirely new users of the platform. 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.[18]

To further test the role of spillovers in driving platform adoption, I present results on spatial heterogeneity within grid point neighborhoods. The median distance from households in the experiment of Lipscomb and Schechter (2018) to the nearest grid point is 50 meters, and the area defined by a 50m radius around the grid point is one-third the size of the area between 50 and 100m from the grid point. This suggests that the "core" of the area around the grid point was treated three times as intensively as the area between 50 and 100m of the grid point. Thus, households within 50m of treated grid points may have been more intensively exposed to neighbors using the platform, as well as possibly more exposed to mechanized desludgings overall. If we observe that platform use is more persistent within this area, this would be further evidence consistent with within-neighborhood spillovers in platform awareness that occur in a relatively concentrated geographic area.

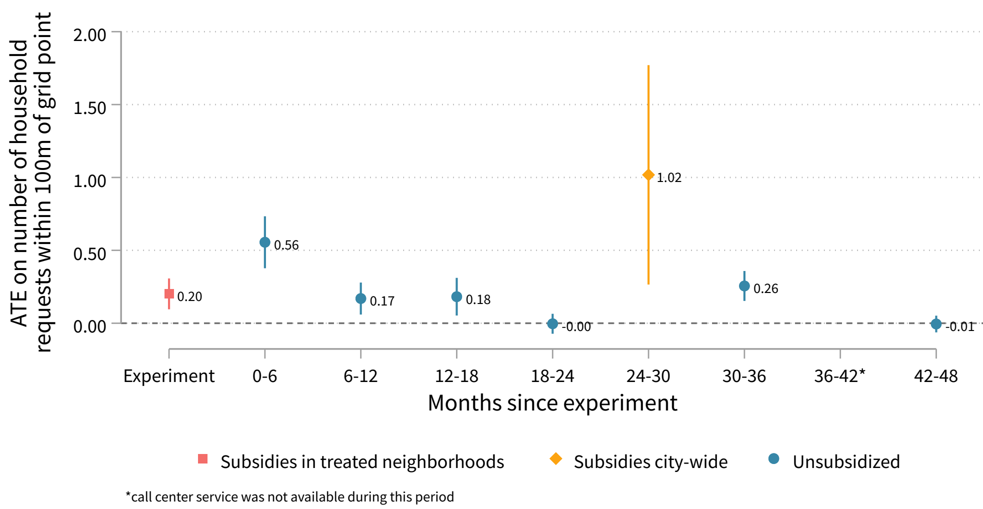
[18] Results are qualitatively similar when I explicitly exclude any households who received a subsidy offer during the original experiment, whether or not they ever used it.

Figure 3: Marginal effects of treatment exposure by period

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

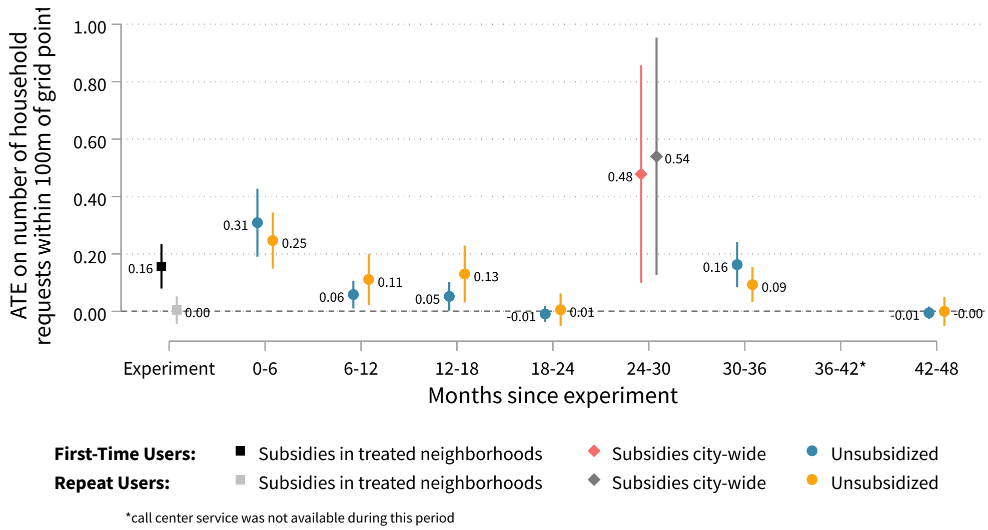


(b) Impact of experimental subsidies on volume of calls from a neighborhood



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 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 3b matches column 2 of Table A.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 A.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 A.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.

To test this, I present results estimated separately for the "core" and "periphery" of each grid-point neighborhood. 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 the results of this exercise in Table A.3.

Comparing columns 1 and 2 in Table A.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.

These results are consistent with the idea that spillovers play a role in driving persistent long-run adoption of the platform. This reinforces the 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 a variety of sanitation outcomes (Bennett, 2012; Deutschmann et al., 2021b). Households in this context typically desludge no more than once every six months. If increases in platform adoption corresponded to overall changes in mechanized desludging use, the pattern of gradual disadoption which occurs more quickly at neighborhood boundaries may suggest 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, and therefore does not use the platform to source a desludging. This would be 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 platform use with possible implications for overall adoption of mechanized desludgings.

4.2 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.4 and A.5 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 include all grid points surveyed in the two baseline surveys. In Columns 1-3 of Tables A.4 and A.5, I present results when 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. Using this alternative sample definition, there are mild changes in the magnitude of coefficients but qualitatively similar results.

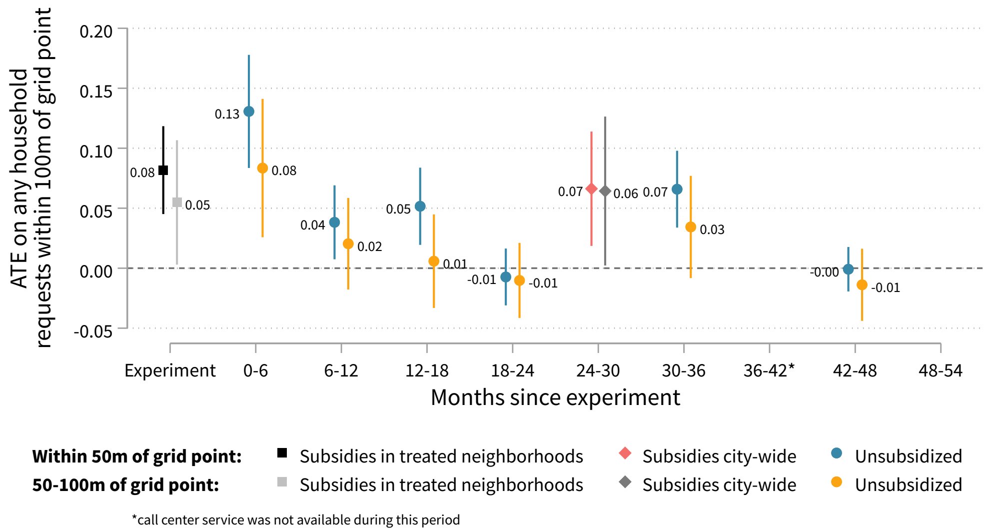
Finally, despite the plausibly random assignment of treatment status, the strategy of assigning every other grid point to treatment status means that the treatment status of a given grid point is perfectly negatively correlated with its immediate neighbors. This complicates the use of standard randomization 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.7.

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.7, are not substantially different from the conventional p-values from the main regression analysis.

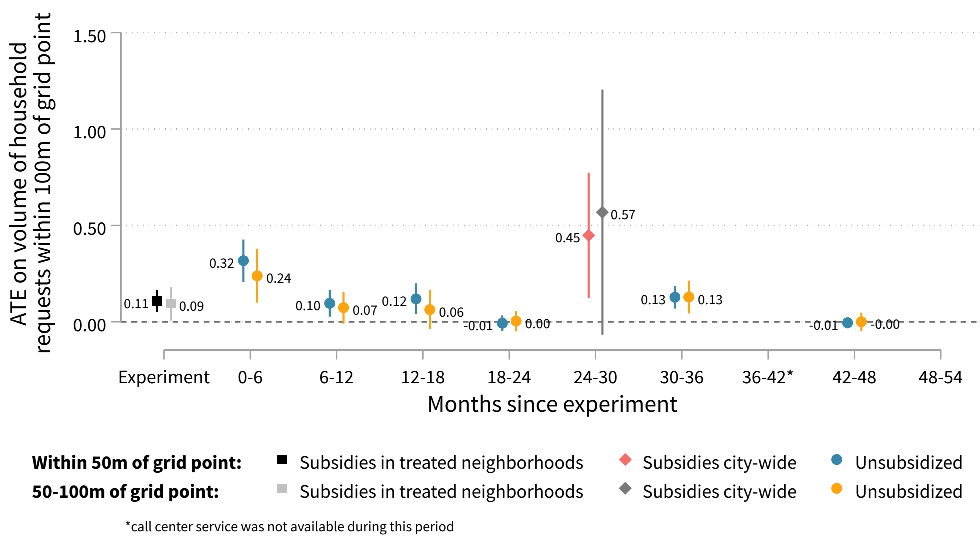
Second, I fix the spatial *structure* of treatment assignment, and randomly shift the set of grid points by up to 100 meters in any direction. For a given counterfactual set of grid points,

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 A.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 A.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.

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.7. 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 explore the dynamics of consumer adoption of a matching platform for sanitation services in Dakar, Senegal. I show that short-run subsidies designed to induce households to source mechanized desludgings through the matching platform had lasting impacts on household demand for the platform, and I provide evidence consistent with within-neighborhood spillovers in platform demand. This is an important market to study consumer decisions: by reducing search costs and increasing the convenience of sourcing a mechanized desludging, the matching platform may induce households to switch away from using manual desludging to empty their septic pits.

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-run 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. As previous work has shown, this is not universally true in other health and sanitation contexts (Dupas, 2014b; Fischer et al., 2019; Bensch and Peters, 2020; Carter et al., 2021; Meriggi et al., 2021).

For policymakers interested in increasing adoption of matching platforms to address market frictions, 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 particularly 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 explore the link between the availability of matching platforms and underlying household decisions. These platforms may have an outsized impact on markets if they provide consumers with a useful outside option, even if not all users end up using the platform for their service. Do matching platforms like the one studied in this paper 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 to desludge their pits? 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

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

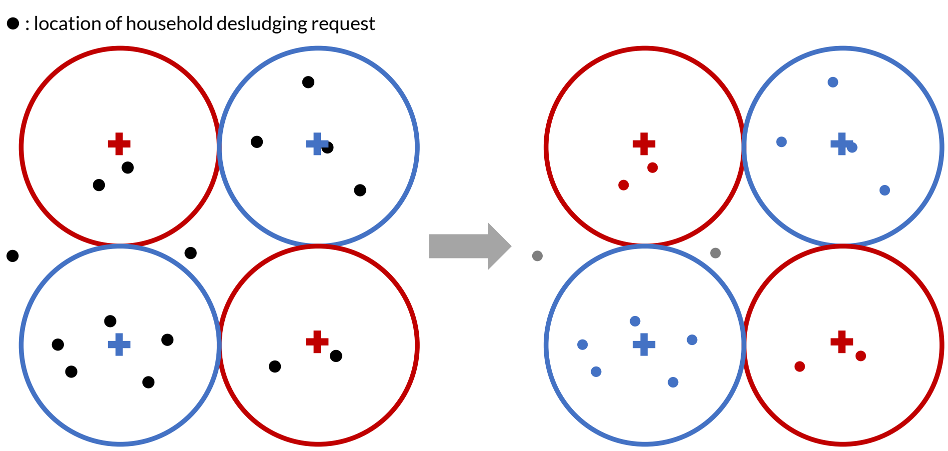

Table A.1: Call center use by period during and after experimental subsidies

	(1) Any Requests	(2) Number of Requests
Treated ×	0.123***	0.201***
experimental subsidies	(0.029)	(0.054)
Treated ×	0.171***	0.555***
0-6 months post-subsidies	(0.034)	(0.091)
Treated ×	0.054**	0.169***
6-12 months post-subsidies	(0.023)	(0.056)
Treated ×	0.047*	0.182***
12-18 months post-subsidies	(0.025)	(0.066)
Treated ×	-0.015	-0.004
18-24 months post-subsidies	(0.019)	(0.035)
Treated ×	0.091***	1.018***
city-wide subsidies	(0.034)	(0.383)
Treated ×	0.090***	0.256***
30-36 months post-subsidies	(0.025)	(0.052)
Treated ×	-0.015	-0.005
42-48 months post-subsidies	(0.018)	(0.029)
0-6 months	0.074***	0.173***
post-subsidies	(0.023)	(0.049)
6-12 months	-0.062***	-0.102**
post-subsidies	(0.020)	(0.041)
12-18 months	-0.051**	-0.054
post-subsidies	(0.020)	(0.043)
18-24 months	-0.093***	-0.156***
post-subsidies	(0.019)	(0.037)
city-wide subsidies	0.159***	1.232***
	(0.026)	(0.220)
30-36 months	-0.028	-0.074**
post-subsidies	(0.020)	(0.033)
42-48 months	-0.093***	-0.161***
post-subsidies	(0.018)	(0.034)
Observations	6867	6867
Number of grid points	763	763
Non-treated baseline mean	0.125	0.207
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 new and repeat callers

	(1) First-Time Users	(2) Repeat Users
Treated ×	0.157^{***}	0.004
experimental subsidies	(0.039)	(0.024)
Treated ×	0.309^{***}	0.247^{***}
0-6 months post-subsidies	(0.060)	(0.050)
Treated ×	0.058^{**}	0.111^{**}
6-12 months post-subsidies	(0.025)	(0.046)
Treated ×	0.052^{**}	0.130^{**}
12-18 months post-subsidies	(0.025)	(0.050)
Treated ×	-0.009	0.006
18-24 months post-subsidies	(0.015)	(0.029)
Treated ×	0.478^{**}	0.540^{**}
city-wide subsidies	(0.193)	(0.211)
Treated ×	0.163^{***}	0.093^{***}
30-36 months post-subsidies	(0.040)	(0.031)
Treated ×	-0.005	-0.000
42-48 months post-subsidies	(0.011)	(0.026)
0-6 months	0.099^{***}	0.116^{***}
post-subsidies	(0.033)	(0.023)
6-12 months	-0.099^{***}	0.040^{**}
post-subsidies	(0.028)	(0.018)
12-18 months	-0.088^{***}	0.076^{***}
post-subsidies	(0.027)	(0.019)
18-24 months	-0.139^{***}	0.025^{***}
post-subsidies	(0.027)	(0.009)
city-wide subsidies	0.635^{***}	0.640^{***}
	(0.118)	(0.116)
30-36 months	-0.076^{***}	0.045^{***}
post-subsidies	(0.026)	(0.014)
42-48 months	-0.150^{***}	0.031^{***}
post-subsidies	(0.026)	(0.011)
Observations	6867	6867
Number of grid points	763	763
Non-treated baseline mean	0.164	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 neighorhood 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.

Table A.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 ×	0.082***	0.055**	0.108***	0.094**
experimental subsidies	(0.019)	(0.026)	(0.029)	(0.045)
Treated ×	0.131***	0.083***	0.317***	0.238***
0-6 months post-subsidies	(0.024)	(0.029)	(0.056)	(0.071)
Treated ×	0.038**	0.020	0.096***	0.073*
6-12 months post-subsidies	(0.016)	(0.019)	(0.035)	(0.042)
Treated ×	0.052***	0.006	0.119***	0.063
12-18 months post-subsidies	(0.016)	(0.020)	(0.041)	(0.052)
Treated ×	-0.007	-0.010	-0.007	0.004
18-24 months post-subsidies	(0.012)	(0.016)	(0.021)	(0.027)
Treated ×	0.066***	0.064**	0.449***	0.569*
city-wide subsidies	(0.024)	(0.032)	(0.166)	(0.324)
Treated ×	0.066***	0.034	0.127***	0.128***
30-36 months post-subsidies	(0.016)	(0.022)	(0.030)	(0.043)
Treated ×	-0.001	-0.014	-0.005	-0.000
42-48 months post-subsidies	(0.009)	(0.015)	(0.015)	(0.024)
0-6 months	0.034**	0.040*	0.057**	0.116***
post-subsidies	(0.014)	(0.021)	(0.025)	(0.043)
6-12 months	-0.003	-0.068***	-0.003	-0.099***
post-subsidies	(0.011)	(0.018)	(0.019)	(0.034)
12-18 months	-0.011	-0.051***	-0.011	-0.042
post-subsidies	(0.010)	(0.018)	(0.018)	(0.038)
18-24 months	-0.008	-0.093***	-0.014	-0.142***
post-subsidies	(0.010)	(0.017)	(0.016)	(0.031)
city-wide subsidies	0.076***	0.125***	0.317***	0.915***
	(0.017)	(0.025)	(0.081)	(0.197)
30-36 months	-0.008	-0.028	-0.017	-0.057**
post-subsidies	(0.010)	(0.019)	(0.016)	(0.027)
42-48 months	-0.020**	-0.085***	-0.028**	-0.133***
post-subsidies	(0.008)	(0.017)	(0.014)	(0.029)
Observations	6867	6867	6867	6867
Number of grid points	763	763	763	763
Non-treated baseline mean	0.023	0.113	0.034	0.173

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.

Table A.4: 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)
	100m	75m	125m	100m	75m	125m
Treated ×	0.132***	0.117***	0.174***	0.123***	0.114***	0.170***
experimental subsidies	(0.032)	(0.027)	(0.035)	(0.029)	(0.024)	(0.031)
Treated ×	0.183***	0.165***	0.194***	0.171***	0.160***	0.176***
0-6 months post-subsidies	(0.037)	(0.033)	(0.040)	(0.034)	(0.029)	(0.036)
Treated ×	0.063**	0.069***	0.062**	0.054**	0.059***	0.050*
6-12 months post-subsidies	(0.026)	(0.022)	(0.029)	(0.023)	(0.020)	(0.026)
Treated ×	0.055**	0.059**	0.055*	0.047*	0.050**	0.051*
12-18 months post-subsidies	(0.027)	(0.023)	(0.031)	(0.025)	(0.021)	(0.028)
Treated ×	-0.016	-0.010	-0.005	-0.015	-0.009	-0.007
18-24 months post-subsidies	(0.021)	(0.018)	(0.025)	(0.019)	(0.016)	(0.022)
Treated ×	0.097***	0.094***	0.137***	0.091***	0.099***	0.121***
city-wide subsidies	(0.037)	(0.034)	(0.038)	(0.034)	(0.031)	(0.035)
Treated ×	0.108***	0.092***	0.128***	0.090***	0.083***	0.106***
30-36 months post-subsidies	(0.028)	(0.023)	(0.031)	(0.025)	(0.021)	(0.028)
Treated ×	0.002	0.006	0.017	-0.015	-0.006	-0.001
42-48 months post-subsidies	(0.019)	(0.015)	(0.022)	(0.018)	(0.014)	(0.020)
0-6 months	0.086***	0.062***	0.121***	0.074***	0.054***	0.116***
post-subsidies	(0.026)	(0.022)	(0.028)	(0.023)	(0.019)	(0.025)
6-12 months	-0.069***	-0.048***	-0.055**	-0.062***	-0.042***	-0.048**
post-subsidies	(0.023)	(0.018)	(0.026)	(0.020)	(0.016)	(0.022)
12-18 months	-0.055**	-0.038**	-0.038	-0.051**	-0.034**	-0.034
post-subsidies	(0.023)	(0.019)	(0.025)	(0.020)	(0.016)	(0.022)
18-24 months	-0.100***	-0.048***	-0.103***	-0.093***	-0.045***	-0.096***
post-subsidies	(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	-0.034	-0.024	-0.017	-0.028	-0.025*	-0.008
post-subsidies	(0.023)	(0.017)	(0.026)	(0.020)	(0.015)	(0.023)
42-48 months	-0.107***	-0.059***	-0.121***	-0.093***	-0.051***	-0.105***
post-subsidies	(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.5: 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.6: Call center use by period during and after experimental subsidies, sample restricted to precise coordinates

	(1) Any Requests	(2) Number of Requests
Treated ×	0.104***	0.155***
experimental subsidies	(0.026)	(0.045)
Treated ×	0.128***	0.336***
0-6 months post-subsidies	(0.031)	(0.067)
Treated ×	0.059***	0.169***
6-12 months post-subsidies	(0.019)	(0.047)
Treated ×	0.028	0.135***
12-18 months post-subsidies	(0.018)	(0.047)
Treated ×	-0.006	0.005
18-24 months post-subsidies	(0.013)	(0.022)
Treated ×	0.067**	0.257**
city-wide subsidies	(0.029)	(0.100)
Treated ×	0.015	0.037*
30-36 months post-subsidies	(0.017)	(0.022)
Treated ×	-0.006	0.010
42-48 months post-subsidies	(0.013)	(0.020)
0-6 months	0.074***	0.122***
post-subsidies	(0.021)	(0.039)
6-12 months	-0.062***	-0.102***
post-subsidies	(0.017)	(0.031)
12-18 months	-0.051***	-0.079**
post-subsidies	(0.016)	(0.031)
18-24 months	-0.071***	-0.113***
post-subsidies	(0.015)	(0.029)
city-wide subsidies	0.093***	0.286***
	(0.023)	(0.069)
30-36 months	-0.048***	-0.099***
post-subsidies	(0.018)	(0.029)
42-48 months	-0.068***	-0.116***
post-subsidies	(0.015)	(0.028)
Observations	6867	6867
Number of grid points	763	763
Non-treated baseline mean	0.082	0.133
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.7: 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 4.2 for more on these procedures. Note that the call center was not in operation in the 36-42 month post-subsidy period.